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LECTURES

ON THE

PRINCIPLES AND PRACTICE

OF

PHYSIC

DELIVERED AT KING'S COLLEGE, LONDON

BY

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CONTENTS

OF

THE SECOND VOLUME.

PAGE

LECTURE XLI.

Diseases of the Thorax. General Observations. Dyspnœa. Cough. Methods of exploring the physical conditions of the chest, by the senses of sight, touch, and hearing 1

LECTURE XLII.

Catarrh; its varieties. Acute Bronchitis. Dry Sounds attending the Respiration; Rhonchus and Sibilus: Moist Sounds; Large and small Crepitation: how these are produced, and what they denote. Treatment of Acute Bronchitis. Collapse of the Lung—diffused, and lobular. Sudden Infarction of a large Bronchus. Peripneumonia Notha 21

LECTURE XLIII.

Influenza. Symptoms and progress. Conjectures as to its cause. Treatment. Hay Asthma. Chronic Bronchitis. Its varieties. Morbid anatomy of these affections. Dilatation of the Bronchi . 43

LECTURE XLIV.

Hooping-cough: symptoms; duration; complications; pathology; treatment. Pneumonia: its stages and morbid anatomy; auscultatory signs 68

LECTURE XLV.

Pneumonia continued: its general symptoms; pain, dyspnœa, cough, expectoration. Course of the disease. Prognosis. Treatment. Lobular Pneumonia 88

LECTURE XLVI.

PAGE

Pleurisy. Its anatomical characters; false membranes; liquid effusion; effects of these upon the shape and contents of the chest, and upon its healthy sounds. Symptoms of Pleurisy . . . 108

LECTURE XLVII.

Pleurisy continued. Recapitulation of Symptoms; of Diagnostic Signs. Causes of Pleurisy. Pneumothorax; its Conditions and Signs. Treatment of Pleurisy. Empyema. Paracentesis Thoracis 128

LECTURE XLVIII.

Pulmonary Hæmorrhage: its varieties; its connection with pulmonary consumption, and with disease of the heart. Pulmonary Apoplexy. Prognosis in Hæmoptysis. Symptoms. Treatment 154

LECTURE XLIX.

Pulmonary Emphysema; vesicular and interlobular. Anatomical characters of vesicular Emphysema; physical signs; general symptoms; causes: treatment. Interlobular Emphysema; its anatomical characters, symptoms, cause, and cure. Œdema of the lungs. Phthisis Pulmonalis 173

LECTURE L.

Phthisis continued. Vomicæ; adhesions of the pleuræ; ulceration of the larynx and trachea—of the intestines; fatty liver; waxy liver; auscultatory signs of a vomica; gurgling, cavernous respiration, pectoriloquy: cracked-pot sound; general symptoms of phthisis; cough, expectoration, hæmoptysis, dyspnœa, pain, hectic fever, frequency of pulse, diarrhœa, wasting, œdema, aphthæ . 200

LECTURE LI.

Phthisis continued. Diagnosis. Forms and varieties of Phthisis. Ordinary duration. Age at which it is most frequently fatal. Influence of sex; and of occupation. Question of Contagion. Treatment 224

LECTURE LII.

PAGE

Melanosis of the Lung; spurious and true. Other forms of malignant Pulmonary Disease. Accidental intrusion of solid substances into the air-passages 251

LECTURE LIII.

Diseases of the Heart: usually partial. Changes in its Muscular Texture. Mechanism of those Changes. Natural Dimensions of the Heart. Natural Sounds. Modifications of these by Disease. Review of the Physical and General Signs that accompany Cardiac Disease 268

LECTURE LIV.

Diseases affecting the muscular texture of the heart; and their treatment. Fatty degeneration. Rupture. Changes to which the valves of the heart are subject. Effects and diagnosis of those changes. Angina pectoris 291

LECTURE LV.

Pericarditis: its frequent connection with Acute Articular Rheumatism. Rheumatic Carditis. Anatomical Characters of Acute Inflammation of the Pericardium; of the Endocardium. General Symptoms. Auscultatory Signs. Relations of Carditis with Rheumatic Fever 322

LECTURE LVI.

Treatment of Acute Pericarditis and Endocarditis: blood-letting; mercury; blisters. Chronic and partial Inflammation of the Pericardium. Disease of the Aorta. Thoracic Aneurisms; their various situations, and symptoms: plan of treatment . . . 353

LECTURE LVII.

Diseases of the Veins. Phlebitis; Pyæmia; consecutive scattered Abscesses. Treatment. Effects of the obstruction of large Venous Trunks 376

LECTURE LVIII.

PAGE

Asthma: its nature; complications; exciting causes; and treatment. Diseases of the Œsophagus: Inflammation; Stricture; Spasm; Dilatation 392

LECTURE LIX.

Diseases of the Abdomen; sometimes difficult to identify. Method of investigating these diseases; by the eye, the hand, the ear. Inflammation of the Peritoneum; its symptoms; and causes. Puerperal Peritonitis. Peritonitis from Perforation . . . 415

LECTURE LX.

Treatment of Acute Peritonitis; Bleeding, Opium, Rest. Chronic Peritonitis: Granular Peritoneum. Ascites; Ovarian Dropsy; Diagnosis of these Diseases. Other forms of Abdominal Dropsy . 433

LECTURE LXI.

Pathology of Chronic Ascites; of Ovarian Dropsy. Treatment of these two disorders. Internal remedies: Extirpation of the ovarian sac: Paracentesis Abdominis 446

LECTURE LXII.

Acute Gastritis: symptoms; anatomical characters; treatment. Chronic Inflammation of the Stomach; thickening of the Mucous Membrane; Ulceration; symptoms and treatment of the disorder: Softening and Perforation by the Gastric Juice. Cancer of the Stomach 464

LECTURE LXIII.

Hæmorrhage from the Stomach: sometimes from a large vessel, usually capillary. Idiopathic Hæmatemesis. Vicarious Hæmatemesis; Hæmatemesis from Gastric disease or injury; from disease in other organs. Melæna. Hæmatemesis from a morbid state of the blood. General phenomena of Hæmatemesis. Diagnosis. Treatment 495

LECTURE LXIV.

PAGE
Dyspepsia. Physiology of Digestion. Symptoms of Dyspepsia. Treatment and Prevention, Dietetic and Medicinal . . . 508

LECTURE LXV.

Enteritis: its symptoms; causes; treatment. Mechanical occlusion of the Intestinal Tube. Colic. Colica Pictonum: its symptoms, complications, treatment, and prevention 530

LECTURE LXVI.

Diarrhœa. Sporadic, or Summer Cholera. Epidemic Cholera . 568

LECTURE LXVII.

Dysentery. Diarrhœa Adiposa. Intestinal Concretions. Worms. 602

LECTURE LXVIII.

Tape-worms, continued. Cyst-worms. Hydatids. Trichinæ. Guinea-worm. Liver-fluke. Strongulus gigas. Spurious Worms. Spontaneous Generation. General Symptoms of the presence of Intestinal Worms. Particular symptoms, and remedies, of the common Round Worm, of Thread-worms, of Tape-worms . . . 624

LECTURE LXIX.

Diseases of the Liver. Acute Inflammation. Abscess of the Liver. Causes and Treatment of Acute Hepatitis. Acute Yellow Atrophy of the Liver. Chronic Hepatitis. Jaundice. Its Symptoms, Causes, and Species 656

LECTURE LXX.

Treatment of the various Species of Jaundice. Diseases of the Gall-bladder; of the Spleen; of the Pancreas. Diseases of the Kidneys. Nephritis and Nephralgia. Phenomena constituting a 'fit of the Gravel.' Different kinds of Gravel. Diseased states of the Urine. Description and Remedies of the Lithic, Phosphatic, and Oxalic Diatheses 685

LECTURE LXXI.

PAGE

Suppression of Urine. Diabetes; Qualities of the Urine; Symptoms; Anatomical Appearances; General Pathology of the Disease. Treatment. Diuresis 718

LECTURE LXXII.

Albuminous Urine. Means of detecting the Albumen. What it imports. Anatomical characters of Bright's Kidney, acute and chronic. Symptoms. Nature of the Disease 741

LECTURE LXXIII.

Anasarca; its consideration resumed. Distinction of Chronic General Dropsy into cardiac and renal. Characters and signs of each of these varieties. Treatment 758

LECTURE LXXIV.

Chylous Urine. Hæmaturia; its diagnosis, general and particular; Local Disorders of the Urinary Organs on which it depends; Treatment. Disease of the supra-renal capsules; Bronzed Skin. Abdominal Tumours 777

LECTURE LXXV.

Acute Rheumatism; Symptoms; Varieties: Treatment. Chronic Rheumatism; Phenomena; Plan of Cure.——Gout: Description of a Paroxysm; Progress of the Disease; general state of the Health in Gouty Persons; Causes of the Disease; Diagnosis between Gout and Rheumatism 802

LECTURE LXXVI.

Pathology of Gout. Prognosis. Prejudices respecting the disease. Treatment: during the paroxysms; during the intervals. Cutaneous Diseases 826

LECTURE LXXVII.

Exanthemata. They are contagious; sometimes epidemic. Period of the eruption; period of incubation. Theory of contagious Febrile Diseases. Thermometry of Disease 842

LECTURE LXXVIII.

PAGE
Typhus Fever. Phenomena of the first and of the second week; Delirium, Mulberry Rash: of the third week; Recovery, or death in the way of Coma, of Apnœa, of Asthenia. Symptoms that precede and usher in those modes of dying. Typhoid Fever; points of distinction between it and Typhus in respect of symptoms, of modes of attack. Rose-coloured Spots. Ulcerations of the Intestine. Relapsing Fever. Further differences between these three Fevers 864

LECTURE LXXIX.

Exciting Cause of Continued Fevers. Their treatment . . . 895

LECTURE LXXX.

Small-Pox. Inoculation. Vaccination. Their comparative advantages. Treatment of Small-Pox 926

LECTURE LXXXI.

Chicken-Pox. Measles. Scarlet-Fever 962

LECTURE LXXXII.

The Plague. Erysipelas. Erythema nodosum. Urticaria. Prurigo. Scabies 992

LECTURE LXXXIII.

Herpes; Eczema; Pompholix; Lepra; Psoriasis; Impetigo; Boils; Carbuncle; Purpura; Scurvy. Conclusion of the Course . . 1012

Epilogue 1040

Index 1041

LECTURES

ON THE

PRINCIPLES AND PRACTICE OF PHYSIC.

LECTURE XLI.

Diseases of the Thorax. General observations. Dyspnœa. Cough. Methods of exploring the physical conditions of the chest, by the senses of sight, touch, and hearing.

From the throat—and especially from that part of it with which we were last occupied—the transition is natural and immediate to the *thorax*. Now the interior of the chest is the theatre of numerous and most important morbid changes. Within that cavity are lodged two of the three organs most essential to life. The heart, lungs, and brain, have been said, by a bold figure of speech, to constitute the tripod of life: and the two former are planted in the thorax. In the same region of the body lie also the greater blood-vessels, and many other parts of scarcely less consequence. With respect to the heart, its alternate swinging movement cannot long stop, and the patient continue to live: and a pause of three minutes in the play of the lungs would in most cases be irremediably fatal. And lesser impediments to the free working of either of these two vital organs are productive of much danger and distress; and lead often to consecutive changes of a very serious kind, in various other portions of the body.

These parts, of which the function is so indispensable, and of which the disorders are so grave and perilous, are encased in such a manner by the ribs and other boundaries of the chest, that they can neither be seen nor handled. And until a very late period in the history of medicine—until our own times, in fact—the diagnosis of the formidable maladies that befall the viscera of the thorax was exceedingly uncertain and imperfect.

Physicians were able, indeed, by the observation of indirect symptoms, manifested through the general system—by remarking the presence of inflammatory fever, I mean—to infer that inflammation had been somewhere lighted up: and symptoms that denoted disturbed function of the respiratory apparatus—cough, difficult breathing, or local pain—might suffice to apprise them that the inflammation was situated within the chest. But what tissue it affected, where was its exact place, what was its extent, or what were its physical consequences—these were points concerning which they had no means of obtaining any precise knowledge. 'Under the title of pneumonia or pneumonic inflammation,' says Cullen, 'I mean to comprehend the whole of the inflammations affecting either the viscera of the thorax, or the membrane lining the interior surface of that cavity: for neither do our diagnostics serve to ascertain exactly the seat of the disease, nor does the difference in the seat of the disease exhibit any considerable variation in the state of the symptoms, nor lead to any difference in the method of cure.' You will see, as we proceed, how very inaccurate this last statement would be, if it were made under our present mode of investigating these diseases. 'Pneumonic inflammation' (he continues) 'however various in its seat, seems to me to be always known and distinguished by the following symptoms:—Pyrexia, difficult breathing, cough, and pain in some part of the thorax.'

I state these things to you—who do not recollect the time, as I do, when no medical man in this country could, with truth and candour, say more of his knowledge of diseases of the chest than Cullen said—that you may the better estimate the exceeding value of the discovery of what is called the method of auscultation, for the detection and discrimination of disease; and most particularly of thoracic disease. In the present day we are able to obtain more exact information respecting the disorders of the parts contained within the chest, than of any other internal and therefore invisible parts of the body. Indeed, in a vast number of instances, we can tell, as accurately as if we saw them, the actual condition of the thoracic viscera: can follow, step by step, the successive processes of disease or of repair, in which they are involved. We can penetrate beyond the symptoms which denote deranged function, and comprehend those much less fallible signs which arise from alterations of structure. And this vast addition to our pathological knowledge has been given us by the simple application of one of our five senses to the investigation of disease, the sense of hearing, which for so many centuries had been (strangely as it now seems) neglected, or but little used. Through the aid and use of the ear it has come to pass that those diseases which, besides being the most common and the most destructive, were also the most obscure, are now better understood than the diseases of any other internal part whatever.

The direct symptoms which arise out of the changed conditions of the parts affected in thoracic complaints, are so mixed up with all that we know or can learn of such complaints, that what in other cases is called the morbid anatomy of a disease, becomes, here, a part of its descriptive history. I shall not say, therefore—as in regard to many other maladies I am obliged or I find it convenient to say—so and so are the symptoms; and afterwards, so and so are the morbid appearances: but I shall describe the morbid conditions in the outset, as the only way of rendering the symptoms which result from them intelligible.

But before I enter upon the subject of auscultation, it may be useful to make a few remarks upon those symptoms of thoracic disease which were previously known, and which depend upon, or rather which express, derangement of the pulmonary functions.

One of the most constant, and obvious, and distressing, and instructive of these symptoms, is embarrassed or laborious or hurried breathing: what is technically called *dyspnœa.* You know that by a healthy adult, under ordinary circumstances, the act of respiration is performed, unconsciously almost, about eighteen times in a minute. There is one act of respiration for about every four beats of the heart. In various diseases this proportion is materially altered. The reciprocal movements by means of which, in measured succession, air is drawn into and again let out of the lungs, are performed with hurry, or effort, or unusual slowness. Dyspnœa implies some deviation from the natural manner or rate of alternately expanding the thorax, and suffering it to collapse again: of inspiring and expiring: in one word, of breathing. The patient himself may, or may not, be conscious of this deviation. In most of the cases in which dyspnœa claims to be regarded as a symptom, he *is* conscious of it. Now upon what does this symptom depend? It may be ascribed generally to an altered proportion between the quantity of atmospheric air that reaches the lungs, and the quantity of blood that is sent into them, from the right side of the heart, to be converted from venous to arterial. Let me remind you that respiration is an automatic movement; subject nevertheless to the occasional control of the will. The pulmonary branches of the par vagum constitute the principal and constant *excitor*, as the nerves that supply the muscles of respiration are the *motor* links of the nervous chain by which the automatic movements are governed. It is believed that the presence of venous blood in the capillary vessels of the lungs forms the natural stimulus to the pulmonary part of the par vagum. In the ordinary breathing of a healthy person, this stimulus or impression is not felt: perhaps because being slight and habitual, and exactly apportioned to the need of the individual, it is not attended to: or, it is at once appeased by the admission of air, and the corresponding change in the blood. But when that change is not immediately or perfectly accomplished, then arises the distressful sensation which everybody has felt, but which our own language has no one word to express. The French call it the *besoin de respirer*. The English phrase, *want of breath*, denotes the peculiar sensation equally well. It calls into exercise, frequently, the voluntary power of performing the mechanical acts of breathing—a power which is superadded to the automatic process.

Various are the ways in which the natural manner and frequency of the respiratory movements may be disturbed. They were fully considered when I was on the subject of death by apnœa. They all operate, ultimately, by destroying the just equilibrium between the blood and air which meet to undergo mutual and chemical changes in the lungs. We have lately been considering certain diseases in which the difficulty and distress of breathing are often extreme. In severe laryngitis, the only inlet for the air is narrowed at its very entrance: there is more blood passing through the lungs than can be arterialised under the ordinary motions of respiration: instinctive efforts take place to increase the quantity of air; to make up by more numerous acts of inspiration for the diminished amount of air introduced by each single act. For a time these compensatory efforts may suffice. But if the access of air be still impeded, blood but half decarbonised begins to circulate through the arteries; and to linger and stagnate in the lungs: the lips become livid, and the skin grows dusky. Make, however, a free and timely opening in the pipe that should conduct air to the lungs, and the balance being restored between the blood in those organs and the air that reaches them, the dyspnœa is soon at an end. The quantity of blood being the *same* then, but the air inspired *too little*, there will be dyspnœa. The very same thing occurs whenever a portion of lung from being spongy is rapidly rendered solid. No air can then penetrate it: nor *perhaps* any blood: but the same quantity of blood as before arrives at the right side of the heart, and is transmitted thence through the pulmonary artery: and consequently those portions of the lungs which are pervious to blood and air, are supplied with blood in excess, and require air in excess: i.e. dyspnœa is necessitated. And you will perceive that similar consequences may arise from any pressure made upon the lung, obliterating in a certain degree its cellular structure; as by fluid collected in the pleura; by enlargement of the heart; by aneurism of the great vessels; by tumours, of whatever kind, within the chest; or by pressure upwards against the diaphragm by reason of a distended abdomen, whether the distension be occasioned by disease, such as ascites, or by obesity, or by a full stomach, or by a gravid uterus. A like disproportion will ensue, if the free expansion of the thoracic cavity be prevented by pain, by disease or rigidity of its boundaries, or by palsy of its muscles through interruption of the nervous circle whereon their contractions depend.

But on the other hand the balance may be destroyed from the opposite quarter: the air admitted during a single ordinary inspiration being the same, the quantity of blood requiring to be converted from purple to scarlet may be augmented; and in that case also, in order to maintain the due equilibrium, more numerous acts of respiration must be performed: in other words, dyspnœa will arise. This is the case under brisk exercise: the pressure of the muscles upon the veins propels their contents with greater velocity towards the right side of the heart; the heart contracts more frequently in proportion as it is more rapidly filled with blood; a greater quantity than usual is sent through the pulmonary artery to the lungs; and the individual breathes more quickly, to supply this augmented quantity of blood with air: he is out of breath, in a state of dyspnœa. But this is not disease. Disease, however, will often have the same effect. The quickened circulation in fevers, any obstacle to the free passage of the blood from the heart into the arteries, will tend to gorge the lungs with blood, to destroy the requisite equilibrium between the air and the blood in those organs, and so give rise to dyspnœa.

Other conditions still may be mentioned, as predisposing to hurry of the breathing—a peculiar state of the nervous system;

certain qualities of the blood;—but I need not dwell on these at present.

The whole subject may receive illustration if we take the two extreme conditions: the condition on the one hand in which all air is shut out from the lungs; the condition in which all blood is excluded on the other. The inlet of air is sometimes plugged by a solid morsel of food that has gone literally 'the wrong way.' The inlet of blood—the pulmonary artery—is sometimes plugged by a solid mass of fibrin brought thither by the circulating current. In both these conditions the dyspnœa is extreme. Strictly speaking, though violent efforts to breathe are made, true respiration is impossible.

The cause of dyspnœa, then, as I explained to you in one of the earlier lectures of the course, might be generally and briefly described as *default of the free circulation of aërated blood.*

There are two important corollaries deducible from what I have now been stating. In the first place, you must perceive how intimately the functions of the heart and lungs are dependent upon each other; and how disease originating in either of these vital organs may readily be the cause of consecutive disease in the other. We shall have many examples of this before us as we proceed. It would afford materials for an interesting essay, this mutual relation between cardiac and pulmonary disease. At present I merely glance at it in passing.

In the second place, what I have said of dyspnœa must have sufficed to show you that, taken by itself, it has not much value as a diagnostic symptom. All that it tells us is, that the healthy and natural relation between the quantities of blood and of air in the lungs is disturbed: but to determine the cause of that disturbance—to decide whether the heart be in fault, or the lungs, or both, or neither—we must have recourse to other sources of information.

Cough is another of the symptoms mentioned by Cullen as denoting disordered function of the breathing apparatus. I need scarcely tell you that it is produced by closing the glottis after a deep inspiration, and then making a sudden and strong expiration. Its purpose is the dislodgment of mucus which may have collected in excess in the air-passages, or of any other source of irritation to the membrane lining those parts. To be effectual it requires the admission of a certain quantity of air, and the possession of a certain degree of muscular strength. The lungs must be sufficiently inflated beyond the collected mucus, which may then be coughed up and 'expectorated.' In old and feeble persons labouring under chronic bronchitis with profuse secretion from the mucous surface, strength is often wanting to cough the phlegm up; and they die suffocated.

But the sensation which prompts to the act of coughing may arise from various other causes besides the accumulation of mucus in the air-passages. Any slight irritation about the glottis; a long, trailing, and tickling uvula; the inspiration of irritating vapours; pressure of any kind upon the respiratory organs; may any of them produce cough. Nay, it sometimes is provoked by sympathy with other parts; an instance of which we have in what is called a stomach-cough. Some morbid condition, some irritation, of the stomach exists, which being appeased, the cough ceases. You will recollect the name and the functions of the *pneumogastric* nerves. We have, in the fact just mentioned, another example, in addition to those which I glanced at in a former lecture, of irritation of the sentient extremities of one branch of a nerve, declaring itself by uneasy sensations referred to other branches of the same nerve. For these reasons, therefore, cough is not more diagnostic of particular diseases situated in the thorax, than is dyspnœa. There are, indeed, certain varieties of cough, as there are certain modifications of the breathing, from which we may obtain very useful information, even with respect to the nature and seat of *some* diseases: and these varieties and modifications I will point out as I go along.

Let me admonish you, also, before we come to auscultation, not to fall into an error which has been too common; that of trusting entirely to the ear in the investigation of thoracic disease, to the exclusion or neglect of those phenomena which are perceptible by the eye, or by the hand; or of those indirect revelations which are furnished by the condition of other parts and functions, or by the previous history of the patient. Even before the discoveries of Avenbrugger and Laennec, physicians were too remiss (if we may judge from their writings) in what may be called the mechanical exploration and notice of the actions of respiration. A good deal may be learned, sometimes, by merely placing one's hand upon the chest, or belly, as I shall explain more particularly by and by: and a great deal, also, may be made out, in some cases, by the simple inspection of those parts, when they are uncovered. You may see, for example, that the ribs, in respiration, scarcely move at all, while the belly rises and falls alternately with the descent and ascent of the diaphragm. This is called *abdominal respiration.* It may arise from a painful condition of the intercostal muscles, or of the pleuræ, rendering the patient *unwilling* to elevate his ribs; or it may arise from disease of the spinal cord, between the origins of the phrenic nerve and of the intercostal nerves, rendering the patient *unable* to raise them; or the same *inability* may result from disease of the lungs themselves. The symptom may guide us at once to the seat of the malady. Again, the breathing may be entirely *thoracic*, no motion of the abdomen taking place; and this may depend upon an affection of the diaphragm, or of the pleura which is reflected over it; or upon disease, accompanied with tenderness, within the abdomen—upon peritonitis for example; or upon mere distension of the abdomen. Or, by looking at the naked chest, you may see that one side of it moves, and that the other moves less, or does not move at all: and the motionless side may be of the natural size as compared with the other, or it may be flattened and contracted, or it may be round and bulging; and most important conclusions, and most important indications of treatment, will flow from a knowledge of these circumstances. The general form of the chest is also instructive. Never neglect, then, to examine the thorax, in cases where it is supposable that the disease may be seated in that part of the body, by your *eye*, as well as by your *ear*. The eye needs but little training to enable it to perceive and comprehend those signs which are within its sphere: the ear, unfortunately, requires to be carefully educated. I will just remark, further, that in the case of females no indelicate exposure of the person need be made. In most instances the morbid conditions I have been adverting to may be recognised through a thin linen covering.

Auscultation signifies the investigation of internal diseases through the sense of hearing: and it is especially applicable, for reasons which I either have stated or will state, to the study of *thoracic* diseases. Its full aim includes all that we learn by listening to the noises caused by the movements of the lungs, and of the heart; and all that we gather by striking the chest, and attending to the resulting sound. But in general, the term *percussion* is used to express this last mode of eliciting information, although the information is strictly auricular: while the term auscultation denotes the art of distinguishing diseases by hearkening to internal sounds, by means of the ear applied to the naked or thinly covered surface of the body; or by means of some conductor of sound interposed between the ear of the listener and the person of the patient. In the first of these two modes the auscultation is said to be *immediate*; in the second, *mediate*. By percussion we ascertain the degree of resonance, or want of resonance, of the part struck: by auscultation we learn the qualities and modifications of the voice, as transmitted through the walls of the chest; and of the breath-sounds; and of the sounds of the heart. The invention of the method of percussion we owe to a German, Avenbrugger, whose excellent treatise upon it was translated, and brought into general notice, by Corvisart. For the more brilliant discovery of auscultation we are indebted, as everybody knows, to Laennec.

Now it will save us much trouble, and conduce, I hope, to your future progress as practical auscultators, if, before I speak of any of the diseases of the chest, I premise some general observations respecting these modern methods of examining the human body, with the view of detecting and discriminating its diseases. Indeed, I could not make myself intelligible unless I did so.

And first, with respect to percussion, which you will please to recollect is nothing else than auscultation of, or listening to, sounds which we ourselves artificially and purposely produce.

You know, every child knows, by daily experience, that different substances, when struck, give out very different modifications of sound. If you strike a drum, you get one kind of sound; if you strike a brick wall, you get another. The one is loud, trembling, and prolonged; the other dull, short, and flat. But why should I attempt to describe in words things which are familiar to you already? Bodies that are solid, or inelastic, give the dull flat sound in proportion to their solid thickness, or their want of elasticity. On the other hand, hollow vessels, i.e., vessels containing air, with thin, firm, elastic boundaries, give out a sound more or less approaching in its qualities to that of a drum: the sound is called a *hollow* sound from that circumstance. If you have a wooden cask containing air only, it is resonant when struck: fill it half full of water, and the lower part will render a flat sound, the upper empty portion a hollow sound; less hollow, however, than when the vessel contained no water: fill it up with water, and the *whole* is dull on percussion: pour out the water, and fill the cask loosely with wool—it will still be resonant, though in a different and less clear note than when it held air alone.

Now this experiment may be transferred to the human chest, which is a cavity, bounded by firm, thin, tense, and elastic walls, and containing, in its natural state, the spongy lungs, which are full of air; and other parts that are solid, whereof the heart is the chief. If you strike the surface of the chest (it requires a little knack to do it properly), and if the blow fall over a portion of healthy lung, you will produce a resonant or hollow sound. If the lung be not there, if it be pushed aside, and its place supplied by some more solid or inelastic substance, by fluid for example, you will hear a dead sound. So you will if the lung *be* there, but has lost its spongy character, is void of air, and somehow or other solidified. But you may have a resonant sound, though the lung is in a state of disease; nay, though the lung is not there: so that percussion alone cannot always be depended upon. I shall tell you, hereafter, how to guard against being misled by it in such cases. Again, if you strike over the region of the heart, you will get a positively dull sound, or at any rate a much duller sound than in most other parts of the chest.

It is really a singular thing, that this method of searching for indications of disease, and of health, should have been so long neglected or overlooked in our profession. I am sure that I had a practical acquaintance with the principle of percussion long before I knew anything of physic; and so, I make no doubt, have most of you. Many a time, when wishing to know whereabouts I might drive a nail firmly into a wall, I have tried with the hammer to find which was brickwork, and which was wooden joist; and percussion is an art in daily use for similar purposes, with carpenters and bricklayers. Yet it does not appear to have been thought of by physicians till the middle of the last century, when Avenbrugger, after studying its results, '*inter tœdia et labores*,' for seven years, published at Vienna his 'Inventum novum, ex percussione thoracis humani, ut signo, abstrusos interni pectoris morbos detegendi.' This was almost totally neglected, however, until, as I stated before, Corvisart's work on diseases of the heart brought it into general notice.

Avenbrugger and Corvisart, and indeed everybody who used percussion at all, until a very few years ago, employed *direct* percussion: that is, they struck the chest with the extremities of their fingers. More recently, *mediate* percussion has been introduced into practice, by M. Piorry. In mediate percussion some solid substance is placed upon the spot, the resonance of which is about to be ascertained, and the blow is made upon that substance, which is called a *pleximeter*—a stroke-measurer. A round thin plate of ivory, laid flat upon the surface, forms the most common sort of pleximeter; but metal, or wood, or leather, or india rubber, may be employed. Many persons, and I am one of them, use no other pleximeter than the fingers of the left hand.

I shall explain, as briefly as I can, the method of employing percussion, and the cautions requisite to render it an effectual and a true interpreter of the state of the parts beneath the stricken surface.

The position of the patient is of some consequence. It should be one that is convenient to the examiner, and not inconvenient to himself; and it should be one calculated to render the part struck as firm and tense as possible. The best position of all is a sitting position, on a firm chair. But you may percuss a patient very effectually as he sits up in bed, or while he stands, or some parts even when he is lying down. A good deal is said—more, in my opinion, than is necessary—about the effect of curtains, and so forth, in deadening the sound. I do not believe they will ever interfere with your conclusions, especially as we learn more from comparing the sounds given out upon percussing the corresponding parts of the opposite sides of the chest, than from the absolute resonance or want of resonance of any single part. But there are some exceptions to this; and if your patient can be made to sit on a chair in the middle of the room, so much the better.

Then, if you are about to percuss the front of his chest, let him suffer his arms to hang loosely down, and let him throw his head back. If you desire to explore in this way the lateral portion of the thorax, he may place the hand of that side upon his head, and lean a little to the opposite side. If you would know how the posterior part of the chest sounds he may lean forwards, let his arms hang loosely between his knees, and bend down his head.

Next, as to the mode of percussing. For direct percussion, the ends of the fingers of the right hand should be brought together, and into a line with each other, so that no one of them may project beyond the rest; and care should be taken first, to compare the sound produced by striking any part of the chest on one side, with that produced by striking the corresponding part on the other side. It follows from this rule that we should not examine all the points on one side before passing to the other, because we should thus lose the remembrance of, and the power of accurately comparing, the sounds obtained from corresponding points. It is best to strike first on one side of the body, and then on the corresponding spot of the other side. It follows also that we are not to compare the result of percussion on one of the *ribs*, with the result of percussion on one of the *intercostal spaces*. The blow should fall *upon* the rib, and *parallel* to it.

A second point requiring attention, is the state of the chest in regard to the act of breathing. If one side be percussed after the movement of inspiration, and the other after that of expiration, some little difference in the resulting sounds will be manifest, even in the healthy condition of the thorax. And this might mislead. Let corresponding spots on the two sides be therefore both struck, either while the chest is expanded, or while it is collapsed, or while the patient holds his breath.

Thirdly, you must take care to strike the corresponding parts at the same angle, and not with the fingers perpendicular to the surface on one side, and inclined obliquely to it on the other: also to strike corresponding parts with the same degree of force. And the blow should not be hard enough to give the patient pain; indeed such a blow would not produce a good sound. It should be smart and quick; the ends of the striking fingers should not *remain* on the chest. Under some circumstances, however, the patient cannot bear to be percussed at all.

These latter cautions are most necessary when *direct* percussion is employed; over which *mediate* percussion has, however, many advantages. Some of these are obvious. In the first place, the space examined by mediate percussion is very exactly defined and limited. Secondly, you may strike the pleximeter much more forcibly than you could strike the unprotected body, and so produce a more decided sound. Even when the surface is morbidly sensitive, or the patient unusually irritable, so that percussion in the ordinary way cannot be performed at all, it may generally be done through the pleximeter. A third and very great advantage is, that mediate percussion is available when made over certain parts where, even although there may be no pain occasioned by it, ordinary *im*mediate percussion is attended with no useful result. Parts, I mean, where there is much fat, and parts which are fleshy, or œdematous. If the pleximeter be pressed firmly upon these parts, even upon the mamma in females, the hollow sound is attainable; whereas, if they were struck by the fingers, the sound would be perfectly dull. Mediate percussion may be applied also, with effect, through the clothes.

I say that a convenient way, and one which I find quite sufficient, of employing mediate percussion, is by making a pleximeter of the finger, or fingers, of the left hand; taking care that they be closely in contact with the subjacent parts, and *similarly* applied to corresponding spots, and that the backs of the fingers be outwards. Piorry declares, indeed, that the resonance produced by this mode is scarcely one-tenth part so great as that elicited by using a thin, solid, and elastic plate. For all practical purposes, however, I am certain that the finger, as it is the readiest, so also is it a very satisfactory and sufficient pleximeter. It has, moreover, this positive advantage, that the sound made by striking *it* is not loud, and does not obscure or interfere with that which depends upon the condition of the chest.

It is right that you should be aware of differences of sound which belong to the individual. *Cœteris paribus*, the sound given out on percussion is more resonant at the end of the act of *in*spiration than of *ex*piration; in childhood and youth than in middle age, because of the greater elasticity of the walls of the chest at the earlier period; for the same reason, in middle age than in old age; in women than in men; in thin persons than in fat; and, *they say*, in nervous irritable people than in those of a contrary temperament.

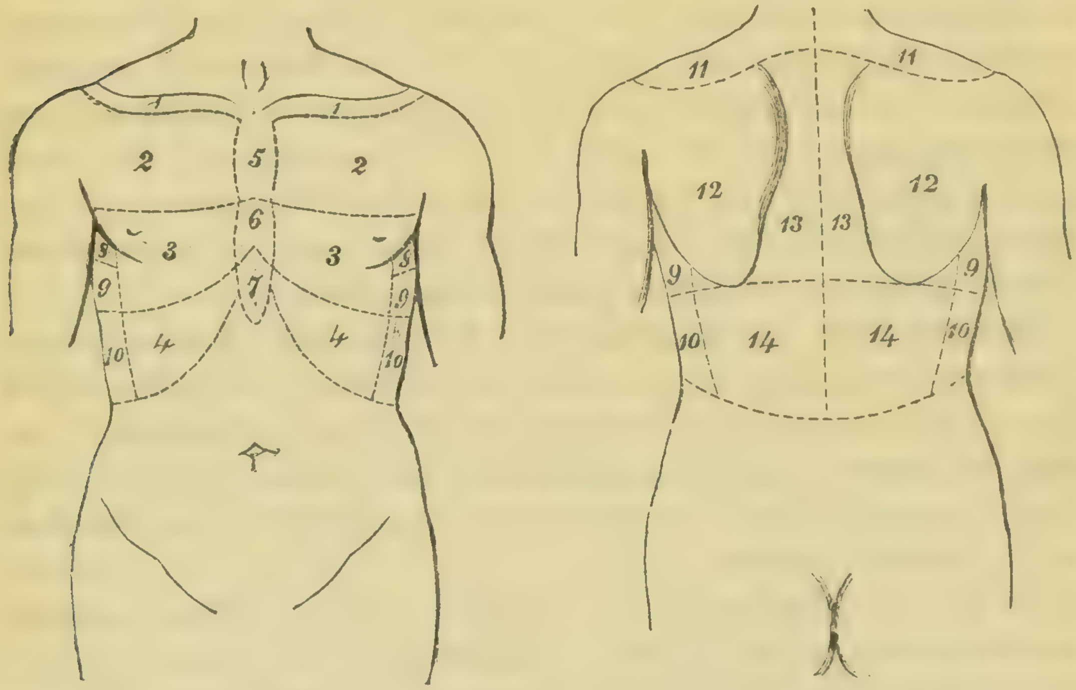
And it is still more necessary that you should be aware of differences of sound given out, in health, by different parts of the surface of the thorax, *in the same individual*. And in order to explain this more distinctly, and for the sake of reference hereafter, let me here exhibit to you the *regions* of the thorax, as they have been artificially mapped out for the purposes of auscultation.

It is unnecessary for me to describe particularly the method followed in this arbitrary division of the thorax into regions. I will state the names that have been commonly applied to them, and the sounds which in health they respectively yield, according to the numbers in the diagram before you.

The *first* region is that of the *clavicles*; one, of course, on each side. Upon these bones it is immaterial whether direct or mediate percussion be made. The sound given out should be very clear at their sternal extremities, dull at their humeral extremities, and clear at their middle points. The resonance diminishes from the sternal towards the acromial end of the clavicle. These parts correspond to the summits of the lungs.

The *second* region is the *subclavian.* It lies between the clavicle and the fourth rib on each side. Beneath this superficial region lies the upper lobe of the lung; and towards the sternum large bronchi are situated. You will understand, therefore, that the sound educed by striking this part ought to be very clear.

A little lower down, number *three,* is the *mammary* region, extending from the fourth to the seventh rib on each side, and answering to the middle lobe of the lungs. Here also the resonance afforded by percussion is clear; but in the lower part of this region on the left side, we find the heart, which is more or less covered by lung: and on the right side the liver begins to mount. The sound is somewhat modified and deadened by these deeper-seated viscera. In women we can determine the degree of resonance of this region through the mammæ, by *mediate* percussion only.



The *fourth* is the *infra-mammary* region. It comprehends that part of the bony compages of the thorax on each side which lies between the seventh rib and the edge of the cartilages of the false ribs. Into this region, on either side, may descend the thin anterior margins of the lower lobes of the lungs; but in the same region, on the right side, lies the liver, over which the sound of percussion is dull; and on the left side is placed the stomach. Hence when the stomach is tympanitic, a preternaturally resonant sound will proceed from this part; and when the stomach contains no gas, the sound will be irregularly dull.

All the regions hitherto described are double. There is still the mesial part of the front of the chest to be subdivided, and we may call the three regions there situated, and numbered 5, 6, 7, the *upper sternal* region, the *middle sternal*, and the *lower sternal*. In all these the sound on percussion ought to be clear, except perhaps in the inferior portion of the last, which may be rather dull; or which, from its vicinity to the stomach, may be tympanitic.

The *eighth* region is the *axillary*; the axilla above the fourth rib on each side. The *ninth* is the *lateral* region, between the fourth and seventh ribs. In both these regions the resonance should be distinct and clear. The *tenth*, which may be called the *lower lateral* region, below the seventh rib at the sides, gives the same sounds as the infra-mammary, namely, on the right side, a dull sound, on the left a sound which at times is preternaturally hollow: on account of the presence of the liver in the former place, and of the stomach in the latter.

But we have yet to look at the hind part of the thorax. Here we have the space (region *eleven*) which forms the top of the shoulder, and lies between the clavicle in front, and the superior spine of the scapula behind: the *acromial* region. Very little can be made out by percussion in this spot; the sound is dull. But the depression immediately above the scapula, the *supra-spinal fossa*, as it is called, is a space which affords valuable information to the *ear* applied over it.

The *twelfth* region is the scapular. It comprises the part covered by the scapula on either side. It corresponds to the posterior parts of the upper and middle lobes of the lungs, but yields a dead sound, unless a pleximeter be used.

The *thirteenth* region is the *intra-scapular*. It lies between the inner edge of the scapula and the spine, on each side. It corresponds to the roots of the lungs, to their upper and to the middle parts of their lower lobes. You may get a good clear sound here, if the patient's arms be well crossed, and his head bent forwards, and his back arched, so as to stretch and tighten, and make as thin as possible, the superincumbent muscles; you will find also that the ridge of the spinal column in this part gives a hollow sound.

The last region to be mentioned, number *fourteen*, the *dorsal*, is the lowermost part of the ribs behind. It answers to the base of the lungs, and gives a clear sound: but the liver on the right side is apt to render its inferior portion dull; and the stomach on the left to make it unduly resonant.

Lateral curvature of the spinal column is usually associated with an unsymmetrical chest; one side of the chest projects more and is rounder than the other. You might expect that it would be the more resonant of the two under percussion. But it is not so; the more convex side is the less resonant. The explanation seems to be that an arched rib yields less to the blow than a flatter or straighter rib.

Now, knowing these circumstances, if you find that a dull sound is yielded on percussing a part that should render a hollow sound, you conclude that beneath the part struck there is not the natural quantity of air. But whether this arises from consolidation of the lung itself, and the obliteration of its cells, or from fluid effused into the cavity of the pleura, you cannot, by mere percussion, determine. So, again, if the sound be unnaturally tympanitic, you cannot say whether that results from what is called pneumothorax, or from emphysema of the lung. Percussion testifies that *air* is there; but further this deponent sayeth not.

The information which the exercise of percussion may leave thus imperfect, auscultation of the sounds produced by the acts of breathing, speaking, or coughing, will in most cases supply.

If the ear be laid close to the surface of a healthy thorax (or if the instrument called the stethoscope be interposed between that surface and the ear), the ear will hear the air enter and fill the lungs, and then withdraw and leave them again, in perpetual succession. The sound produced by this ingress and egress of air has been called the *respiratory murmur*. I might tell you that this sound, to my mind, is rather a rustle than a murmur: that it is like the sighing of the wind through the branches of a tree. I might say, with Laennec, that it resembles the sound made in the deep inspiration of a sleeping person; or, with Mr. Davies, that it reminds one of the soft murmur of a pair of bellows, of which the valve does not click. But one minute's appliance of your ear to the subclavian region in a child, or even in an adult, will give you a clearer notion of the nature and character of this sound than any verbal description could convey. Yet, respecting this natural respiratory murmur, there are some things of which it is desirable that you should be previously informed.

In the first place, the *entrance* of the air is much more noisy than the *exit*: which, sometimes, can scarcely be heard. You will see, by and by, the importance of noticing this fact.

In the next place, the murmur of respiration is not equally audible in all persons. It may differ much in intensity, though not in kind, in two persons, each of whom is in perfect health. Thus it is much more loud and distinct in children than in grown persons. So remarkably is this the case, that when we meet with an unusually noisy respiratory murmur in an adult, we say that his breathing is *puerile*; it has the character of the respiration of a child. Now, I tell you beforehand that puerile respiration in the lung of an adult is generally a sign of disease; and it is mostly partial; heard in certain parts only of the chest.

Again, *cæteris paribus*, the natural murmur of respiration is more clearly audible in lean and spare persons than in such as are muscular and fat. Fat and muscle are bad conductors of sound, and act as dampers. Listening to the breathing through a thick layer of adipous tissue, is like listening (as Dr. Latham says) to the respiration of a person through his clothes.

But if you take two healthy men who are alike in respect of leanness and fatness, you may often find that the respiratory murmur is very loud in the one, and very feeble, or almost inaudible, in the other: but in this last it *becomes* audible when he makes a deep and forced inspiration.

The reason of this difference is not very well understood. The breathing may be very indistinct, though the thorax be capacious, and well formed, and healthy. Some persons seem to require less effort than others to maintain the due equilibrium between the air in the lungs, and the blood in the lungs; as if they had not only pulmonary space enough, but to spare. So that the difference in the manner of breathing, and in the sound occasioned by the passage of the air in and out, depends, in all probability, upon individual peculiarities of the circulation. At any rate, it is very important that you should be aware of the existence of these differences.

But the sounds which reach the ear applied to the chest of a breathing person will differ in different parts, and under different circumstances. The sound given out by the air as it passes through the trachea and larger bronchi, differs from that which results from its passage into and out of the smaller bronchi and air-passages. I am anxious not to refine too much in these matters: and, therefore, I pass by minuter points of difference. Place the stethoscope over the trachea, and you will hear just such a sound as you might expect to hear: the sound of air rushing through a tube of considerable size, a blowing noise. We will call that sound, which you will recognise again when you have once heard it, *bronchial* breathing. It accompanies the outward as well as the inward passage of the breath. Again, place your ear or your stethoscope upon the right mammary region; there you shall hear that rustling sound, which I propose to call *vesicular* breathing, and which is chiefly audible during inspiration. We shall find these distinctions of much use in the discrimination of disease. We shall find, for example, that the breathing sometimes is bronchial, where it *ought* to be vesicular.

Now if in any part of the chest where we should hear breathing, we can hear none, this may result from consolidation of the subjacent portion of lung; or from some obstacle in a large bronchus, preventing the air from entering that part; or from air being in any way shut up and stagnant in that part; or from an effusion of liquid into the pleura at that part. And here again percussion comes into play, and determines for us which of these possible circumstances is the one really present. If the part when struck emits a hollow sound, there is stagnant air beneath it, either in the healthy lung, or in the lung altered by disease, or in the cavity of the pleura. If a flat solid sound be given out, there is solid lung beneath, or liquid in the pleura, between the ear and the lung. All these points, and the conclusions to be deduced from them, will become clear to you, I trust, as we proceed. Minuter analysis of the diagnostic signs would be premature.

A word or two preliminary I must say about the *voice.*

The voice passes outwardly through the mouth and nostrils into the surrounding air; its sound passes also inwardly, through the trachea and bronchi into the lungs, and it may be heard through the lungs, by the ear laid flat against the chest. But it gives quite a different sensation to the ear in different places. If you place the stethoscope on the trachea, the voice will articulate itself into your ear as if it came from and through the instrument. This sound, which is natural here, would be unnatural, and a mark of disease, if heard beneath the clavicles. When heard beyond its natural situation, it is called *pectoriloquy.* A less degree of this, a sound like that of a person talking into a tube, and whose words, for that reason, are muffled and indistinct, is called bronchial voice, or *bronchophony.* When to this modification of the voice there is added a twanging vibration, a cracked discordant tremor, resembling the squeak of Punchinello, or (as some think) the bleating of a goat, *ægophony* is said to exist. All these unnatural modifications of the voice are indicative of most important changes within the thorax. I merely introduce them to you now: you will become better acquainted with them in due time.

It is a curious thing—of which I am unable to give you any satisfactory explanation, but which I had noticed a hundred times before I saw it mentioned in any book—that the resonance of the *listener's* voice, whose ear is applied to the patient's chest, is apt to be exalted or modified by certain conditions of the subjacent parts. I am not aware that any inference can be drawn from this phenomenon beyond what other symptoms would still more readily and accurately supply.

What is true of the natural voice, is true, *mutatis mutandis*, of that unnatural vocal sound, *cough*. The cough may be so modified by the condition of the internal parts, as to reveal that condition.

There are yet other ways in which some information can be gathered respecting the interior of the chest. If you give certain patients a sudden smart jog while your ear is applied to their thorax, you may hear a splashing sound; like that produced by shaking a barrel or a bottle partly full of water. From this expedient you in fact determine that the cavity of the pleura, or perhaps a large excavation in the lung, does contain both air and a liquid. This is called the method of *succussion*. It was employed long before the other forms of auscultation were thought of. Hippocrates mentions it.

Again (but that is not auscultation), you may sometimes collect useful information respecting the state of the chest by simply laying your open hand upon it: by *palpation*. In most persons there is a distinct vibration or thrill produced by their speaking, which thrill is very sensibly felt by the hand. Now this thrill may be felt on one side of the chest and not on the other. And such a difference is an unequivocal sign of disease.

The positions which I described before as being the most convenient for the performance of percussion, are the most convenient also for the purposes of auscultation. You may listen with the unassisted ear, or you may listen through the stethoscope. This, as you know, is a solid or perforated piece of wood, of which one end is adapted to the ear, and the other, which is to be applied to the chest, is hollowed out, or expanded, into the shape of a bell, or funnel. The object of the instrument is to collect and convey to the ear of the observer the vibrating impulse of the air, or of the solid walls of the thorax, occasioned by the motions within. The stethoscope is sometimes useful for examining a circumscribed spot in the thoracic cavity. With it we gauge, more nicely than we could do with the naked ear, the signs furnished by the patient's voice. We *must* use it also when we would investigate the breathing in parts over which the ear itself cannot easily be applied; and in cases where, from the filth of the patient, or because he has some infectious disorder, we desire to avoid any immediate contact with his clothes or person; and in cases of young or old ladies, to whose breasts it might not be seemly or delicate to be putting our faces. But, with these exceptions, the stethoscope, in my experience, is rather a hindrance than a help. Much, however, depends upon custom. I can best distinguish most of the sounds to be heard within the thorax by my unassisted—perhaps I should say my unimpeded—ear: and I therefore employ the stethoscope or cylinder in such cases only as I have just adverted to. I cannot do without the instrument, but I do without it as much as I can. Care should be taken that the end of the stethoscope next to the ear be closely and comfortably fitted to that organ; and that its other extremity be blunt, so as not to hurt the patient, and not so large as to make its close and exact apposition with the wall of the chest difficult or impossible, in thin persons especially; and further, that it *be* uniformly and evenly applied to the surface of the chest. If these precautions be attended to, the shape and fashion of the instrument are very unimportant.

LECTURE XLII.

Catarrh; its varieties. Acute Bronchitis. Dry Sounds attending the Respiration; Rhonchus, and Sibilus: Moist Sounds; Large and Small Crepitation: how these are produced, and what they denote. Treatment of Acute Bronchitis. Collapse of the Lung —diffused, and lobular. Sudden Infarction of a large Bronchus. Peripneumonia Notha.

In the last Lecture I described to you, in a general manner, the method of auscultation; or the employment of the sense of hearing in the investigation of disease; and particularly of *thoracic* disease. I explained, as well as I could, the different sounds which result from percussing various parts of the chest; and from the entrance and exit of air during the several acts of breathing, speaking, and coughing. I mentioned certain conditions in which those natural sounds are abolished: but I did *not* speak, except incidentally and very cursorily, of the altered and new sounds to which the presence of disease within the thorax has been found to give rise. Nor do I propose to enter now upon any *formal* account of these morbid sounds. I shall endeavour to make you acquainted with their characters, and causes, and signification, as they arise in the progress of the separate thoracic diseases which I am about to consider.

Before you can possibly appreciate the *morbid* sounds, you must make yourselves familiar with the *natural and healthy*. You must have a standard, by which you may measure the numerous deviations from the natural sounds, that will reach your ear in disease. Listen to the voice and the breathing of healthy children—or of some of your friends and fellow-students—and you will soon recognise those respiratory sounds which accompany the perfect state and working of the breath-machine.

I intend to commence with those diseases of the lungs in which the *mucous membrane* lining the air-passages is primarily or principally involved. This membrane is often affected *alone*. It is much exposed to known *causes* of disease: to alternations of temperature in the air which is constantly passing over it; to the irritation produced by acrid or noxious matters, solid or gaseous, which are mixed and inhaled *with* the air. And when disease commences in *other* parts of the lungs, it seldom fails to reach, sooner or later, the *mucous membrane.* In diseases also of the *heart* the same mucous membrane is very liable to be consecutively affected, by reason of those alterations in the condition of its capillary vessels which the disorder of the circulation produces.

Now I shall first point out the changes to which this mucous membrane is liable; and then describe the modifications of the natural sounds that result from such changes; and lastly, consider the different forms of disease which these changes constitute, and these altered sounds denote.

In a former part of the course I gave you some account of the peculiarities which differences of texture impress upon the phenomena and process of inflammation: and among the rest, I spoke of the behaviour of the *mucous* tissue when inflamed.

The mucous membranes, in the state of health, are perpetually *moist.* The exhalation of this moisture, to a certain amount, and *not beyond* a certain amount, constitutes an essential part of their healthy functions. Now their *inflammation* (for I am about to consider first the inflammatory affections of the membrane of the air-passages; some of them indeed I have already discussed), I say the inflammation of these mucous surfaces alters their ordinary secretion. An inflamed mucous membrane is in the first instance *dry*; its secretion is suspended. But this is not the only change that takes place in it; it becomes tumid also, swollen, thicker than before; it is redder than natural; and its sensibility undergoes a perceptible modification. *Pain,* in mucous membranes, is *not* a common phenomenon: for their texture enables them to expand or dilate freely, so that they escape much tension, and the pain which is produced by tension: but their natural sensations are blunted, and new and uneasy sensations arise in them: sensations of heat, fulness, itching. It happens that we can *see* a portion of the mucous membrane that belongs to the air-passages: and by noticing the changes produced in it by inflammation, we infer those which are apt to take place in the parts we *cannot* see. We have all often experienced in our own persons an inflammatory state of the membrane lining the nasal cavities; the schneiderian membrane. At first the nostril is preternaturally dry: yet, though it is dry, you cannot breathe through it: it is stuffed up; not with accumulated mucus, but by the mere swelling of the membrane: the sense of smell is perverted or lost; the part is evidently red; it is tender also and irritable; the contact of atmospheric air a little colder or a little less pure than common, provokes sneezing. The affection extends often into the frontal sinuses; and headache and oppression ensue; or it passes into and through the lacrymal sac, the conjunctiva participates in the inflammation, the puncta lacrymalia become impervious, and the tears flow over the cheek. And with all this there is sometimes shivering or chilliness; and the pulse, especially in the evening, becomes a little more frequent than common. There is slight fever. After the unusual dryness, the membrane begins to secrete a thin serous fluid, having acrid properties; for it reddens and frets the alæ nasi and upper lip over which it flows. By degrees, this thin serous fluid becomes thicker, and as it becomes thicker it becomes less irritating also; more viscid, opaque, and yellow: the swelling of the membrane diminishes; it is less raw and sensitive: at length the secretion resumes its natural *quality*, and is reduced to its natural *quantity* again; and the tumefaction of the membrane entirely disappears. This is the course of what is popularly called a *cold in the head.* When the defluxion from the nasal membrane is considerable, systematic writers call the complaint *coryza*; when it is attended with much pain and weight about the *frontal sinuses*, it is named *gravedo.* It is a variety of *catarrh.* In catarrh, sometimes one part, and sometimes the whole, of the mucous membrane of the air-passages suffers inflammation. If the disorder go down into the lungs, it is said to be a *cold in the chest*; or, from one of the most prominent of its symptoms, *a cough*; in medical language, *bronchitis.* It sometimes travels from one part of the membrane to another. Beginning, for example, in the nose, it gradually creeps down into the wind-pipe and lungs. Sometimes the inflammatory condition passes from the throat into the eustachian tubes, and produces deafness; or down the gullet and to the stomach, causing qualmish or other uneasy sensations, and loss of appetite. And occasionally this order appears to be reversed. There are some persons who will tell you that whenever anything disagrees with their stomachs, whenever dyspepsia is produced by some error in diet, they are sure to have catarrh.

Now I have adverted to this *cold in the head*, or coryza, because the phenomena which are open to our inspection in the schneiderian membrane take place also, no doubt, in the bronchial. The membrane is first dry, and tumid, and irritable; the uneasy sensations of which it is the seat prompt to the action of coughing. The chest feels tight, stuffed, constricted. There is some hoarseness, and a sense of roughness and soreness in the wind-pipe; and a dry cough, which seems to arise from some irritation about the glottis. Sometimes, with these symptoms, pains in the limbs, like the pains of rheumatism, occur; the appetite is impaired; the patient is thirsty; and a general lassitude is felt all over the body.

But what effect has the altered state of the membrane upon the sounds elicited by percussion; or heard within the chest, by the applied ear, during respiration? Why it brings us acquainted at once with two remarkable modifications of the natural sound of breathing; and these I must describe and explain.

I will take this opportunity of again recommending you to read and study the lectures on clinical medicine published by Dr. Latham. They contain a very plain and clear account of the auscultatory signs of disease within the chest; and he speaks of these signs in more easy and popular language, with less of over refinement, and a less subservient adoption of the French mode of thinking and writing on these subjects, than any other English author that I know of. I recommend his volumes the rather also, because he uses the nomenclature which is the most familiar to myself: in fact, as we some years ago saw and talked of these matters together in the wards of St. Bartholomew's, we were likely to employ the same terms.

When you listen, I repeat, to the breathing of a healthy person, you hear, as the breath goes in and out, but especially as it goes in, a smooth and gentle rustle—the *respiratory murmur*, or the *vesicular breathing*. But when the inner surface of the bronchial tubes, and of their ramifications, is preternaturally dry, and tumid, this sound is altered: you hear a hissing, or wheezing, or whistling, as the breath goes in and out; and this is technically called *sibilus*; or you hear a deeper note, a snoring noise, as the patient inspires or expires—a sound like the cooing of a pigeon, or the bass note of a violin, or the droning hum of an insect in its flight; and this is called *rhonchus*. These two, in their various modifications, constitute the *dry* sounds of respiration; and it will be worth while, once for all, to reflect upon their cause and nature, and the manner in which they are combined, and what they denote. You are aware that when air is driven through a cylindrical tube of a certain size, and when that tube is narrowed in a particular way at one or more points, a musical note is produced. Now this is what often happens in the larger bronchi; this is what *always* happens in them when *rhonchus* is present. Rhonchus belongs to the larger divisions of the bronchi exclusively; and as these are often, for a time, exclusively affected, so rhonchus may exist *alone*. It will be grave or deep in proportion to the length and diameter of the tube in which it is produced. When the sound *is* grave and deep, the hand placed upon the chest may frequently perceive a trembling or thrill communicated to its parietes. I believe that rhonchus is mostly occasioned by portions of viscid, half-solid mucus, which adhere to the membrane, and cause a virtual constriction of the air-tubes, and act as vibrating tongues while the air passes by them. I conclude such to be the case, because it seldom happens that the rhonchus cannot be got rid of by a vigorous cough. It will soon begin again, perhaps, or it will commence in some other part, but the effort of coughing, which detaches and removes the adhering tough mucus, dislodges also, for the time, the rhonchus. Yet, rhonchus in a given spot may be permanent: a tumour, or a tubercle, may flatten one of the air-tubes, and convert it into a musical instrument. For the most part, you will find what I have told you holds true: you may suspend the rhonchus by getting the patient to make a hearty cough. Now in the natural state of the chest, we do not, except in particular spots, hear the transit of the air through the larger bronchi. Whatever sound it makes is damped by the spongy lung, or covered by the vesicular breathing. But rhonchus, in its turn, may overpower the vesicular murmur, and render it inaudible. It does not prevent it, but it outroars it, as it were. Yet this is seldom the case: you hear the rhonchus, and, if you listen attentively, you may in general hear, mingling with it, the vesicular murmur also. Recollect, then, that rhonchus belongs to the larger divisions of the air-tubes; that it denotes their partial narrowing; that it is a dry sound; and that the condition of which it is expressive implies usually no danger: there is no material obstacle to the passage of the air through these larger tubes into the vesicular structure beyond them.

I must further admonish you, that in your earlier essays in auscultation you will be apt to deceive yourselves in respect of the exact place in the lung in which the rhonchus which you hear is produced. It is so loud a sound, that when it proceeds from a single bronchial tube it may be plainly audible over the whole of that side of the chest; and sometimes, more obscurely, over the other side too.

When air is driven with a certain degree of velocity through a small pipe, it gives rise to a hissing noise. It is by forcing air through a cylinder perforated by a slender tube, that Professor Wheatstone obtains the sound of the letter S in the talking machine which he has constructed, after Kempelen's model. Precisely this condition we have in the smaller bronchial ramifications, when the inflammation in catarrh or bronchitis has reached *them*, and rendered the membrane lining *them* tumid. And sibilus is the result of this change. Now sibilus, like rhonchus, may exist alone; and, inasmuch as the sibilus proceeds from the smaller air-tubes, adjacent to the pulmonary vesicles, it *abolishes* the natural respiratory murmur. It does not, like simple rhonchus, merely drown it, but it takes its place. If you hear the respiratory murmur mingling with sibilus, you may be sure that some of the lesser air-tubes are narrowed, and some free: you cannot have both sounds at once from the same ramifications of the bronchi. Sibilus is a sound of more serious import, therefore, than rhonchus; it bespeaks a condition of greater danger. It belongs to the smaller air-tubes and vesicles, and denotes that they are in the first stage of inflammation, which has diminished their natural calibre by rendering the membrane tumid. It is a dry sound, but you cannot cough it away.

I say rhonchus may occur alone, and sibilus may occur alone; but very often indeed they both occur together; and may be heard in various parts in different degrees: causing a strange medley of groaning, and cooing, and chirping and whistling, and hissing, mixed, it may be, here and there, with the natural respiratory murmur. When you hear sibilus over the entire surface of the chest, the mucous membrane is universally affected, and the case is a severe one, and attended with more or less of hazard.

It is just possible that a sibilant sound may proceed from a large air-tube, when its bore has been narrowed to a very minute slit or orifice; but this possibility does not interfere with the general distinctions that I have been endeavouring to point out.

Now in these cases we neither obtain nor require any information from percussion, except of a negative kind. Supposing the inflammation confined to the mucous membrane, the resonance on percussion will not be sensibly diminished; the lung is everywhere spongy still, and air reaches every part of it, though not with the usual freedom.

There is one exception to this. Occasionally, though rarely, a piece of tough phlegm may seal up, as it were, the very entrance of one of the principal bronchial tubes, and so prevent the air from passing to or from the portion of lung to which that tube conducts. When this happens, it is very likely to puzzle the auscultator for a time. There is air in the sealed-up portion of lung, therefore percussion gives a natural sound; but the air is at rest, therefore no sound of respiration is audible. An effort of coughing unstops, perhaps, the bronchial tube; and then the air is again heard to enter and to depart from that portion of lung. I shall advert to this sort of accident again.

Finally, I may remark, that these dry sounds, rhonchus and sibilus, are heard during the *breathing*; they have no relation to the voice or to the cough.

After a while, the inflamed membrane begins again to pour out fluid; but it is not the vaporous, bland, moderate exhalation of health; it is a glairy, saltish, transparent liquid, like white of egg somewhat; and if it be expelled only after much coughing, it will be frothy also, i.e., it will contain many bubbles of air entangled in it. At first thin, and even watery, the fluid expectorated soon becomes stringy and tenacious, and the more so in proportion to the intensity of the inflammation. With this new condition of the membrane, we have new sounds—sounds which result from the passage of air through a liquid; sounds which are occasioned by the formation and bursting, in rapid succession, of numerous little air-bubbles. These sounds are called *crepitations*. This process may take place in the larger air-tubes, or it may take place in the smaller, or in both. In the larger tubes the *bubbles* will be larger, and the ear can readily distinguish this; we have *large crepitation*. In the smaller air-tubes, we have, in the same way, *small crepitation*. There is no difference between these sounds, except in degree; and they graduate insensibly into each other. But there is a considerable difference in the nature of the intimations which their well-marked varieties convey. If there be merely large crepitation, without any other morbid sound, it is produced in the larger tubes. Air passes, notwithstanding, into the vesicular structure *beyond* the accumulated liquid; and vesicular breathing *exists*, though perhaps it *cannot be heard*, on account of the crepitation. But the state of the patient is not a state of peril. On the other hand, small crepitation has its seat in the smaller air-tubes and cells; it supersedes the vesicular breathing, and, if extensive, it bespeaks considerable danger.

Rhonchus and large crepitation are respectively the dry and moist sounds that belong to the larger bronchi; sibilus and small crepitation the dry and moist sounds of the smaller branches. When the latter sounds are heard over a considerable part of the chest, there is, I say, usually a good deal of distress, dyspnœa, and cough; and the fever which attends the local inflammation is at its height. By and by the expectoration becomes opaque, and more consistent, and of a greenish or yellowish colour; it is brought up with more ease; the crepitation, great and small, diminishes; perhaps rhonchus reappears: but at last the parts return to their original condition; and the natural, smooth, equable rustle of the breathing is again everywhere audible.

These are all the morbid sounds to which active and recent inflammation of the mucous membrane of the air-passages ever gives rise: rhonchus and sibilus; large and small crepitation. Having once described their nature and causes, I need not repeat the description if we find them accompanying other diseases; but their *import* may be different. I may mention here, that as crepitation results from the passage of air amongst and through a liquid, from the rupture of the little air-bubbles so produced, the *kind* of liquid may vary. If the air in going and returning, meet with serum, or with pus, or with blood, it will occasion exactly the same bubbling noise. Hence the French term for what I have been calling crepitation, viz., *mucous rattle*, is very objectionable. From the sound itself, we cannot tell whether it proceeds from *mucus* or from some other liquid present in the air-passages; and from this objection the word crepitation, whatever exception may be taken against it on other accounts, is free.

Having thus embraced the occasion of explaining these auscultatory signs, I will now resume the history of catarrh. It implies inflammation of the mucous membrane of the air-passages; and it receives different appellations, according to the district of that membrane which it chiefly plagues: gravedo, in the frontal sinuses; coryza, in the schneiderian membrane of the nose; bronchitis in the trachea and lungs.

Catarrh is the commonest of all disorders. Not one man in ten thousand passes a winter without having a *cold* of some sort. And this name points to its ordinary cause; cold somehow applied to the body. It does not always or often result, I apprehend, from cold air brought into contact with the membrane itself, in the process of breathing; but from cold, and especially from cold and wet, applied to the external integument. It is unnecessary for me again to go over the circumstances under which the application of cold is most likely to prove injurious. Catarrh is usually a trivial disorder, and runs its course in a few days, if abstinence be observed with respect to animal food and stimulating liquor, and if the patient remain in an equable temperature, and avoid re-exposure to the cause of his malady. I am now speaking of the milder forms of catarrh. We are not often consulted for this complaint. Every man, in regard to a cold, thinks himself qualified to be his own doctor. But if you *are* consulted, keep your patient in the house, or even in bed; let him live upon slops; give him a gentle aperient, and then some of those medicines which are esteemed to be diaphoretic: small doses of James's powder; three drachms of the liquor ammoniæ acetatis, with a drachm of the spiritus ætheris nitrici, and an ounce of camphor mixture; or a saline draught with an excess of alkali, and a few grains of nitre, or a little antimonial wine; give some such dose three or four times a day; and just before he goes to bed let him take four or five grains of Dover's powder, and put his feet and legs into a warm bath, or, what is better, take a hot-air bath. In this way you may conduce to his *recovery*; and he may be simple enough to suppose that you have *cured* him.

Yet I believe catarrhs *may* sometimes be *cured*; and the natural recovery from them *may* be, *sometimes*, accelerated. If you follow the old maxim, which says, '*venienti* occurrite morbo,' you may occasionally stop a cold on the threshold, as it were, by an opiate. And to persons who are habitually troubled with slight catarrhs, this piece of practice may prove of the greatest value. A surgeon who resides in this neighbourhood, and with whom I was a fellow-student, is exceedingly subject to what he calls a snivelling cold. For many years he used to bear this as he best might; and that, to say the truth, was very ill and impatiently. On one occasion, almost by accident, he took twenty drops of laudanum just as one of his colds was beginning to torment him; and he found that the initiatory symptoms vanished. Since that time he has constantly had recourse to the opiate under similar circumstances; and whereas he used formerly to be very miserable for three or four days, he now is quite well and comfortable in the course of half an hour. And this is not a solitary case. A former and valued student of this College writes me word, that for twenty years he has been almost invariably successful in arresting incipient coryza, attended with much sneezing and defluxion, by a similar but probably a better prescription. He gives ten minims of liquor morphia, with seven or eight of vinum antimoniale, in a little draught, every three or four hours. Two, or at most three doses are generally sufficient. Sir Henry Halford's practice in similar cases was to send his patient to bed, and to direct him to take a beaker of hot wine negus, with a table-spoonful of the syrup of poppies in it.

There is also a period in catarrh which has gone on unchecked, when you may accelerate its departure—'speed the going guest'—by a good dinner, and an extra glass or two of wine. But this pleasant method is scarcely to be advised for persons of delicate habit; or in whom any phthisical tendency is suspected to exist; or who are prone to inflammation. And it is not to be tried with anyone till the fever is over, and the expectoration thick and loose.

I must not omit to mention the *dry* plan of cure; although (I confess it with some shame) I have never yet tried it either upon myself or upon others. Dr. C. J. B. Williams, who invented it I believe, has a high opinion of its efficacy. It certainly has the merit of simplicity, for it consists merely in abstinence from every kind of drink. No liquid, or next to none, is to be swallowed until the disorder is gone. The principle here concerned is that of cutting off the supply of watery materials to the blood. The wants of the system exhaust from the circulating fluid all that can be spared for the sustentation of the tissues, or for the natural evacuations; and there is nothing left to feed the unnatural secretion from the inflamed mucous membrane. Its capillary vessels cease to be congested; the morbid flux is diverted, and the inflammation starved away. Such is the theory. Habitual topers might hold the remedy to be worse than the disease; but Dr. Williams assures us that the necessary privation is not very hard to bear: and that it achieves a cure, upon an average, in forty-eight hours. He allows, without recommending, a table-spoonful of tea or milk for the morning and evening meals, and a wine-glass of water at bed-time.

One great advantage of this plan is, that it does not require confinement to bed, or to the house. The man whose business calls him abroad, may, with appropriate clothing, pursue his customary employment, and his cure is all the while going on. In fact, exercise, inasmuch as it promotes perspiration, helps the recovery; whereas the system of warm drinks and diaphoretics renders the body more susceptible to atmospheric vicissitudes; and, to be effectual, implies restrictions which are oftentimes extremely inconvenient.

Dr. Williams observes, that while this dry treatment is serviceable in catarrhal bronchitis, it is *most* successful in coryza, the snivelling cold in the head. It must be put in force in the very commencement of the disorder.

You may often do much by way of *prevention*, for persons who are unusually liable to take colds. I have remarked before upon the great value of the *shower-bath* for that purpose. I could mention several instances in which persons have got rid of the tendency to catch cold, by the habitual use of this aspersion. It should be begun in the summer, and the water should at first be tepid; but in a short time quite cold water may be employed; and being once begun, the practice may be continued throughout the winter. I stated formerly, that the effect of exposure to cold was, *cœteris paribus*, in proportion to the intensity and the duration of the *sensation* of cold that it produced. The intensity of the sensation of cold under the shower-bath is considerable, but the duration of it is momentary. It operates as a prophylactic in this way: it inures the surface to a lower temperature than it is likely to be subjected to at any other part of the day. The lesser degrees of cold have then no injurious effect, unless they are long protracted. For those who cannot procure a shower-bath, or who cannot bear its shock, cold sponging will be found exceedingly salutary.

But inflammation of the membrane lining the air-passages may be, and often is, a very acute and dangerous disorder, i.e., the inflammation may be both intense and diffused; it may descend into the vesicular texture, and occupy the whole surface of the membrane on one side of the chest, and then it may prove a very grave disease; or it may involve the whole lining membrane of both lungs, and then it is always attended with considerable peril. To cases of this kind the term *acute bronchitis* is applicable.

The inflammation will sometimes, when it is thus *general* over the whole membrane, linger for a considerable period in its first stage; and it may even, after so lingering, subside again without ever passing beyond the first stage. By the first stage, I mean the stage of dryness. Very little notice of this modification of bronchitis has been taken by authors. Dr. Latham has given a distinct and graphic description of it, to the accuracy of which I can testify from my own experience.

You will find cases of it detailed in his book. Since they were published, some striking instances of this form of the disease have occurred to myself. One, which happened lately, I will relate by way of example. I was asked by an old pupil of the hospital to see a lady, his patient, in Gordon Square. I found her feverish, and in a state of extreme dyspnœa, sitting up in bed from inability to lie down, labouring for her breath; her face turgid and rather livid, her nostrils working, her shoulders elevated. She could scarcely speak, but expressed, in what she did say, a dread of immediate suffocation. She had been in nearly the same state for a day or two. On listening at her back I could hear the air slowly wheezing and whistling into her lungs everywhere, and then leaving them still more slowly, with a prolonged growl, something like that of an angry cat. There was no true vesicular breathing; there was no crepitation: and there was no part into which the air did not, although with difficulty, find its way. The chest was everywhere resonant on percussion. There could be no doubt that the membrane throughout was tumid and dry, and in the earliest stage of inflammation. Depletion had already been employed in this case, and we had recourse to the tartar emetic. This was given in free and repeated doses, till it produced nausea and sickness. Whenever it did so, the pulse diminished in force, the face became blanched, and the breathing much easier; and the medicine was then suspended until these effects had gone off, when it was repeated in the same manner. The disease was not, however, brought at once to an end by this treatment; it was kept at bay for a day or two longer, and then a copious secretion from the membrane took place, with great relief to all the distressing symptoms. Then, of course, crepitation became universally audible. Except the debility which it left behind, the patient soon recovered from the bronchial inflammation.

It seems probable that with the earliest and dry stage of bronchitis some element of spasm may mingle. Whether this be so or not, inhalation of the steam of hot water is often very soothing and useful. It is one of the best expectorants I know of when it answers at all; but to some persons it proves irritating, and they derive no help from it.

In the great majority of instances the inflammation does not thus linger in its first stage: the membrane soon begins to pour forth glairy mucus; so that we do not often meet with *sibilus*, without finding at the same time, in some part of the same lung, that there is also small and large crepitation. It is of some importance to attend to the characters of the mucus that is expectorated. It is transparent and adhesive. If you pour it from one vessel into another, it flows out in one mass of extreme tenacity; it will draw out sometimes like melted glass; and the degree of viscosity is a tolerably accurate measure of the degree of the existing inflammation. Upon the surface of the viscid mucus there is usually more or less froth, the *quantity* of it depending on the facility or the difficulty with which the sputa are brought up. If the patient do not expectorate till after a long fit of coughing, during which air has been many times inspired and expired, and has thus got intimately mingled with the mucus contained in the air-passages, the expectoration will contain numerous little air-bubbles; will be very frothy. Sometimes also, during this stage of the complaint, the sputa are marked with streaks of blood.

While the expectoration possesses the characters I have been describing, the inflammation is still active, and the fever and dyspnœa considerable. This correspondence between the general symptoms and the matters spat up was well known to the ancients, who said that such expectoration was still *crude*. But in proportion as the inflammation approaches to resolution, the appearance and qualities of the sputa are changed: the mucus loses by degrees its transparency, is mixed with masses or pellets that are opaque, and of a yellow, white, or greenish colour; and these masses, few at first, increase more and more in number, until they constitute the whole of the sputa. Such expectoration as this is commonly accompanied by a marked remission in the different symptoms of the bronchial inflammation: it announces that the inflammation is terminating in resolution. It is such as the ancients spoke of as being *concocted*, or *ripe*. However, the characters of the opaque sputa expectorated towards the end of an attack of acute bronchitis are subject to much variety.

It will often happen that the expectoration, after having thus become opaque, and parti-coloured, will go back again to its former condition of transparency, and stickiness, and froth: and that is a very certain index of a return or increase or extension of the inflammation: so that the character of the matter expectorated exhibits, in a certain degree, the progress of the inflammation; and consequently constitutes one point of guidance to our treatment. The nature of the expectoration forms also an important particular in our means of distinguishing bronchitis from pneumonia; as I shall further explain when I speak of the latter disease.

I have described acute bronchitis as it appears when it terminates favourably: in such cases the inflammation generally begins to abate somewhere from the fourth to the eighth day of the disease. But acute bronchitis may terminate *unfavourably*. When the inflammation is universal and intense, the fever high, and the embarrassment of respiration great, with a painful sense of tightness and constriction about the chest—if the symptoms do not yield to the treatment employed, or if judicious treatment have not been adopted, or have been too long delayed, signs of impending suffocation begin to show themselves: the lips, and cheeks, and tongue assume a purplish colour; a livid paleness takes the place of the former red flush; the expression becomes more and more anxious; delirium comes on, and rapid sinking. These things indicate, you know, the circulation of blood that is in a great measure venous through the arteries; and the venous blood acts as a poison when it so circulates. Profuse, cold, clammy sweats ensue; and the patient dies of apnœa. His breathing is choked by the morbid secretion which occupies the bronchial tubes, small as well as large, and which he has not strength enough left to cough up.

Accordingly, when we examine the thorax after death so produced, we find, in the first place, that the lungs do not subside upon the admission of the pressure of the atmosphere to their external surface. We next find the trachea, and bronchi, and their ramifications, blocked up by a frothy adhesive mucus, resembling that which during life had been expectorated: and the membrane which lines them is red and thickened.

The treatment proper for these acute and dangerous forms of bronchitis is a matter of some nicety. The abstraction of blood, as I formerly stated to you, has not that decided power over inflammation of *mucous* tissues which it possesses over the adhesive inflammation to which serous membranes are liable. Nevertheless, if there be much fever, a hard pulse, and great oppression of the breathing, and particularly if these symptoms present themselves in a young, strong, and robust individual, it may be proper to take away blood. And that expedient will seldom fail to *relieve* the symptoms; even when its ultimate effect may be injurious. The patient's distress arises from his inability to supply air enough to arterialise the venous blood which is carried to his lungs; and by diminishing the quantity of blood in those organs, you will, *pro tanto*, mitigate his uneasiness. But a great part of the danger to be apprehended in the advanced periods of the disease, is that the patient may not have muscular power enough to disembarrass his air-passages of the phlegm that overloads them; to draw a strong breath, and to achieve a vigorous cough. And this danger must be borne in mind in our earlier curative efforts. If you decide to take blood at all, take it from the surface by cupping. The space between the scapulæ is the most eligible spot for the application of the cupping-glasses; but they may also be placed upon the front of the thorax. The amount of blood to be thus abstracted, and the question of repeating the cupping, must be determined by the condition of the patient's pulse, which supplies a better measure of the propriety of depletion than is furnished by the local symptoms.

After the bowels have been cleared by a mercurial purgative, you will find the tartar emetic a very valuable medicine in these acute cases of bronchitis. It should be given in such doses as will excite nausea; and if vomiting be occasioned, you may still go on with the medicine after the sickness has subsided. The depression which this substance produces is great, but it is temporary, and it is brought about without expending blood. One of the best effects of the antimony is, I conceive, the production of diaphoresis in the early and dry stage of bronchitis. When the secretion from the affected membrane is copious, the antimony may even be injurious.

If symptoms of debility and sinking have begun to show themselves, it will be necessary to give wine or brandy, and stimulating expectorants. I presume that the carbonate of ammonia, which is often extremely useful in such cases, acts as an expectorant, by giving a fillip to the muscular power. But it is supposed by some persons to exercise some specific influence upon the bronchial membrane. However this may be, five or six grains of it, given in camphor mixture or in the infusion of senega every four or six hours, are often followed by free expectoration and a marked improvement.

One of the things of which patients are much disposed to complain, is the violent or importunate cough; and another is, the want of sleep and of rest: indeed the one of these is often, in a great measure, the cause of the other:—the urgency or frequency of the cough prevents the patient from sleeping. Under such circumstances the good effects of a full narcotic at bed-time are sometimes very striking. Patients who for previous nights have been perpetually harassed by cough, and who are worn out by the disturbance of their rest, will sleep tranquilly, and in the morning expectorate largely and freely, and declare themselves wonderfully the better for their opiate. Yet opium is a ticklish remedy in these cases. Patients not a few—some within my own knowledge—labouring under general or extensive bronchitis, have been put so soundly to sleep by a dose of opium on going to bed, that they have not waked again. I believe you may receive it as a golden rule, not to give opium —I mean in a full dose, so as to force sleep,—if you see any venous blood mingling in the general circulation,—if the complexion be dusky, and the lips in any degree blue. The circulation of half-arterialised blood through the brain is in itself a powerful cause of coma; and if you add the influence of an opiate, the coma may easily be made fatal. While the cheeks and lips remain florid, and when the first violence of the disease has abated, an opiate will do capital service. But for allaying cough, and procuring sleep, the newly discovered remedy, *chloral*, may be safely given, when opium might be dangerous.

Counter-irritation is frequently of great use, as an auxiliary measure, in the treatment of bronchitis. Sensible relief of the cough, and of the oppressed breathing, often follows the rising of a large blister laid across the front of the chest. When the dyspnœa is extreme, and a more speedy counter-irritant is required, you may have recourse to the mustard poultice, or the turpentine stupe.

I have been speaking of acute bronchitis, uncombined with any other pulmonary disease; and it is curious how little disposed the inflammation often seems to be to extend itself from the mucous membrane to the neighbouring tissues. The reason, doubtless, is, that this membrane is furnished with a distinct set of blood-vessels, the bronchial arteries, and veins: while the substance of the lungs is supplied by the pulmonary. We could not tell, merely by attending to the general symptoms, whether the inflammation was limited to the inner membrane or not; but by making use of the sense of hearing, we *are* able to determine this. If the inflammation should spread to the parenchymatous texture of the lungs—i.e. if the bronchitis should pass into pneumonia,—this circumstance would be disclosed by physical signs, which I shall in due time describe and explain; and it would demand certain modifications of our plan of treatment.

In the later stages of acute bronchitis, and in various disorders of the bronchial membrane, a peculiar condition of the lung is apt to result, mechanically, from obstruction of the air-tubes by inspissated mucus. This condition is one of great pathological interest; but it was not recognised, or if recognised it was not clearly understood, till of late years. It has been well described and explained by some modern French and German writers. The best English accounts of it to which I can refer you are those of Dr. William Gairdner, as it occurs in the bronchitis of adult life—of Dr. West, as it is modified somewhat in the lungs of children—and of Sir James Alderson, in its connection with hooping-cough.

The condition of which I speak is that in which portions of the lungs are completely exhausted of air. Naturally, you know, the healthy lung contains a residual quantity of air even after the most forcible act of expiration. When taken from the body the healthy lung is moist, has a spongy elastic feel, and crackles slightly under pressure. But through the operation of disease, portions of the living pulmonary tissue may become as thoroughly devoid of air as the whole of that tissue is in infants who have never breathed. Those empty portions are firm, tough, dry, of a dull red colour, and they sink when placed in water. The surface exposed by their section looks to the naked eye like a piece of muscle. Hence they are sometimes said to have become *carnified*. This state has been confounded with, but is very different from, a morbid state that I shall soon have to describe, in which the lung, from its resemblance in texture to liver, is said to be *hepatised*; in which there is the same dull red colour, and the same absence of crepitation under pressure, but the affected tissue is friable, and its cut surface moist and granular. In the one case the air-cells are empty; in the other they are choked up with the exudations proper to inflammation: in neither case do they contain air.

This empty condition of the pulmonary substance may result from the expulsion of the air by pressure from without: as when the lung is pushed firmly against the ribs and vertebral column in pleurisy attended with liquid effusion—to be described hereafter. The lung thus compressed and looking like flesh has been not unaptly called carnified lung. But the cause of the absence of air is more often internal, and consists in the plugging up of one or more air-tubes; and then *collapse* of the lung is said to have taken place: and this is the more common and the more correct term.

The mechanism of this collapse is very simple and intelligible. Small portions of tough and sticky mucus are driven to and fro in the larger air-tubes during the alternate acts of respiration, or in the paroxysms of a cough. Mostly they are expelled by the cough which their presence provokes. But it may happen, and it often does happen, that during inspiration one of these pellets, forced strongly backwards in a tube which gradually becomes smaller and smaller, shuts up that tube, and all the smaller branches that proceed from it beyond the place of the obstruction. In expiration the plug moves a little outwards again, so as to allow a part of the imprisoned air to escape; but returning in inspiration, it does not permit any fresh air to enter. Repetitions of this process exhaust, or nearly exhaust the air from the portion of lung thus mechanically sealed up. The portion so exhausted suffers collapse.

Collapse of the lung may be *diffused*, or *lobular*.

In the first of these forms, which is also the more common of the two, the collapse extends over a considerable portion of one lung, or of both lungs. The collapse may not be complete, nor the absence of air total. The affected piece of lung may still therefore float in water. Its colour, which varies according to the quantity of blood contained in it, is usually a dark, brownish red, gradually shading off sometimes into the hue of the adjoining lung. This diffused collapse is generally met with in the posterior parts of the lungs.

The second, or lobular form of collapse, is more sharply defined, and its well-marked outline strikes the eye at once. The affected lobules, or bunches of lobules, occupy the anterior edges of the lungs, as well as other situations. They are slightly depressed below the general surface of the lung. Seen through the pleura, they have a dark red or violet colour; and when cut into they present a brown or mahogany hue. This form of collapse occurs chiefly in the lungs of children, and it was formerly regarded as the consequence and evidence of lobular pneumonia. The condensation is not due, however, to present or to by-gone inflammation, but simply to the absence of air. When the change is of recent date, the collapsed portions may be restored to their natural volume and condition, by blowing air into them through their proper bronchial tubes.

When collapse has taken place to any considerable extent during an attack of bronchitis, the breathing is apt to become laborious. The act of inspiration is performed with effort, difficulty, and distress; while that of expiration is comparatively easy. The patient is unable to lie down. All the muscles accessory to the inspiratory movements are called into play, yet little air finds entrance. Dr. Gairdner speaks of this 'long-drawn, exhausting, inadequate inspiration, as being probably peculiar to obstructive bronchitis.' The *paroxysms* of severe dyspnœa incidental to bronchial disorders are doubtless often owing to casual changes in the position of portions of tenacious mucus in the air-tubes.

You will bear in mind this condition of collapse, and the laboured respiration associated with it; for I shall have to point out to you a striking contrast, in regard to the manner of breathing, when we come to the consideration of pure pneumonia.

Collapse of the lung, in proportion to its extent and its proximity to the surface, must modify, and in some degree lessen, the resonance of the chest under percussion. When such modification of the stroke-sound springs up in the course of an attack of bronchitis, and the patient's respiration becomes at the same time laborious, these conjoint symptoms will generally suffice to reveal the presence and the character of what may be called an accident of the disease. From this accident, when it is recent and uncomplicated, both reason and observation teach us to believe that the lung may recover, upon the removal of the obstruction, and the consequent readmission of air. And it seems probable, as Dr. Gairdner has suggested, that the muscular contractions of the bronchial tubes themselves have frequently a greater share than the movements of respiration, in promoting the expulsion of the accumulated mucus.

A similar accident may sometimes convert an apparently slight attack of bronchitis into a most perilous and quickly fatal malady. A large plug of tenacious mucus may all at once enter and stop up the principal bronchus of one, or of the other lung. Sudden and urgent dyspnœa ensues, and unless the plug be dislodged, the patient may perish before any collapse can be effected. Instantly, in that portion of the lung to which the bronchus conducts, all sound of respiration ceases; yet over this same portion of lung, in which no sound, healthy or morbid, is heard by the ear applied to the thorax, percussion gives the natural hollow sound. If the obstructive mass be fortunately expelled, or displaced, in the desperate struggle for breath, the sound of respiration is re-established as suddenly as it had previously disappeared; and the dyspnœa also ceases. In some cases, however, the noise of the pulmonary expansion does not so return, the difficulty of breathing increases, suffocation becomes imminent, and death by apnœa takes place rapidly.

Andral relates two instances of death from this cause; one of which was the following. A coachman, fifty years old, had been several times a patient in La Charité, for obstinate pulmonary catarrh, with slight dyspnœa, and puriform expectoration. Every time he went away relieved, but not cured. On both sides of his chest could be heard all the varieties of rhonchus. In one spot the column of air which penetrated the bronchi imitated the snoring of one in a deep sleep; in another spot it was like a dull and prolonged groan; in a third, a sound resembling that made by bellows, and in a fourth the cooing of a turtle-dove, were exactly simulated. On the last occasion of his entering the hospital, his respiration was still tolerably free. One morning he was found in a state of unusual anxiety. In the middle of the night, after a violent paroxysm of cough, his breathing (he said) had suddenly become very much oppressed. It was discovered, on auscultation of his chest, that no air penetrated into the upper lobe of the right lung; yet that part sounded well on percussion, even louder than the corresponding part of the other side, which was morbidly dull. The difficulty of breathing went on augmenting, and the man was soon dead.

Besides other marks of disease in the lungs, the primary bronchus leading to the upper lobe on the right side was closed up completely by tough mucus, and exhibited the appearance of a full cylinder.

In the other case, also, the obstructed bronchial tube supplied the upper lobe of the right lung.

It may seem strange that the interruption of the access of air to so small a portion of the lungs should be attended with such serious consequences, when we know that the greater part of each lung may be impermeable by air, and yet the patient live a long time, and often without any great dyspnœa. The explanation of the apparent difficulty seems to be, that in the one case the prevention of the arrival of air in the part affected is sudden, in the other gradual. Moreover, the remaining portions of the lungs are performing their functions imperfectly.

When once attention has been awakened to the kind of accident just mentioned, the diagnosis would not seem to be difficult. We may suspect obstruction of one of the bronchi when considerable dyspnœa comes on suddenly during the continuance of simple bronchitis; and our suspicion will be confirmed if at the same time respiration ceases to be audible in a certain portion of the lung, while the sound given by percussion over the same part remains unaltered. Emphysema of the lung (which I shall explain hereafter) is the only other condition which could give rise to similar physical signs.

Andral judiciously suggests the employment of emetics, and the inhalation of steam, in such cases. The inhalation of the vapour of turpentine with steam is a powerful expectorant.

I shall have to speak of some varieties of chronic bronchitis; but there is a mixed form of pulmonary disease that requires to be noticed, in which acute or subacute inflammation engrafts itself upon changes that are chronic and abiding. Sydenham calls the disorder to which I now refer *peripneumonia notha*—bastard peripneumony. *Catarrhus senilis* is another of its names. It may be described as chronic bronchitis, occurring in old persons, and very apt to be converted into pneumonia, or to be greatly aggravated in degree during winter, or upon any accidental exposure. This is the common complaint of persons advanced in life. I mention it here in compliance with the usual custom, and because this is as convenient a place for its introduction as any. But it would be an error to regard it as exclusively a disease of the mucous membrane of the lungs. An habitually congested state of that membrane, marked by some shortness of breath, and some expectoration, and by the constant presence of some degree of crepitation in the lower parts of the lungs—these are circumstances which are of daily occurrence as consequences of *disease of the heart*; and it is in persons whose habitual health is of this kind, that what is called peripneumonia notha, which implies a diffused inflammation of the pulmonary mucous membrane, with sometimes an enormous secretion from its surface, is most apt to supervene. Almost all such persons will tell you that there are periods at which they experience slight febrile attacks, and exacerbations of their complaints: they have pain in the breast or side, headache, heat, and thirst; and at these periods the cough and expectoration are always aggravated, and continue for some time to be more than commonly severe. 'The disease,' says Cullen, who, following Sydenham, has given a good description of its general symptoms, 'has often the appearance only of a more violent catarrh; and after the employment of some remedies, is entirely relieved by a free and copious expectoration. In other cases, however, the feverish and catarrhal symptoms are at first very moderate, and even slight; *but* after a few days these symptoms suddenly become considerable, and put an end to the patient's life, when the indications of danger were before very little evident.'

The truth is (and we learn the truth by the evidence of auscultation), that in these cases *pneumonic* inflammation is often suddenly set up. There is no security, as Dr. Latham observes, that the portions of lung which yield crepitation to-day may not be solid and impervious to-morrow. Dr. Latham is of opinion that in this disease the inflammation is apt to travel over the bronchial membrane from place to place, as erysipelas is seen sometimes to wander over the surface of the body. I know not how this may be; but certainly death is often produced in these persons by the sudden spoiling of even a moderate portion of the lung, or by its sudden *closure*. For it is more than probable that, in many of these cases, the distressful symptoms result (especially when they bear no inflammatory character) from the rapid accession of pulmonary collapse. In their ordinary condition, the patients have just enough, and no more, of the respiratory apparatus in an effective state, whereby to subsist; and when a fresh part of it is rapidly rendered solid or useless by an access of pneumonia or of collapse, they quickly perish. But they die also from another cause. The nicety of treatment which I spoke of as being required in certain stages of acute bronchitis, is still more apparent and necessary here. We have to trim between opposite sources of danger—the Scylla of unchecked inflammation, the Charybdis of increasing weakness and inability to expectorate the effused mucus, which is tending to suffocate our patient. Cupping and blisters are the remedies most suitable when there is evidence of recent inflammation—what are called expectorants and perhaps emetics, when we have reason to suspect collapse. Medicines which are at the same time diuretic are also serviceable—the spiritus ætheris nitrici, the preparations of squill, and of digitalis.

LECTURE XLIII.

Influenza. Symptoms and progress. Conjectures as to its cause. Treatment. Hay Asthma. Chronic Bronchitis. Its varieties. Morbid anatomy of these affections. Dilatation of the Bronchi.

CATARRH, which was the principal subject of the last lecture, occasionally prevails far and wide as an epidemic disease. I speak, indeed (February 4, 1837), during the immediate presence of one such visitation, although the extreme violence of the complaint that has been raging among us is now fast subsiding. You can hardly be without curiosity to know what has been learned respecting an influence which has thus, on a sudden, before your eyes, disturbed and sickened a whole community. I have here used, without thinking of it, the very word by which, in a foreign version, the disorder is denominated. It has received, however, various names; for it has been known and noticed from remote antiquity. Cullen calls it *catarrhus e contagio*; and under that head, in his 'Nosology,' you will find a copious reference to recorded accounts of epidemic catarrh, as it has been observed to spread over great portions of the world. In France the disorder thus prevailing is styled the *grippe*. The Italians, putting the cause for the effect, call it *influenza*, the *influence*: and this last term influenza has now become naturalised in our language. Since Cullen wrote there have been four or five more of these epidemics; one in 1782, which extended over all Europe, visiting every country therein, affecting more than one-half of its inhabitants, and proving fatal to very many of them. You will find, in the third volume of the 'Transactions of the College of Physicians,' a good account of the disease as it then showed itself in this country. In the spring of 1803 another instance of it occurred; and of this the history, as compiled from the testimony of a hundred and twenty-four observers, is preserved in the ninth and tenth volumes of the 'London Medical and Physical Journal.' In the month of April, in the year 1833, the influenza again made its appearance, and prevailed extensively, both here and elsewhere; and of the influenza of 1837 you have had, and you still have, the opportunity of being observers. A very good and instructive sketch of this epidemic malady, compiled by Dr.

Hancock, is to be found in the 'Cyclopædia of Practical Medicine.' To that article, to the publications I just now mentioned, and to the works enumerated by Cullen, I may refer you for much which is curious and interesting in the history of the disease; but which would not be so well adapted to our immediate purpose in this place—namely, that of seizing upon the practical facts which have been ascertained respecting influenza.

One characteristic feature of this species of catarrh, as distinguished from the ordinary sporadic disorder, is the sudden occurrence, in the outset, of more decided febrile disturbance. The first two patients whom I saw in the epidemic of 1833 had just the symptoms which frequently mark the commencement of an attack of continued fever; and I did not know, at my first visit, what was about to happen to them. The symptoms, taking them altogether, are somewhat as follows. The patient is chilly, and perhaps shivers; presently headache occurs, and a sense of tightness across the forehead, in the situation of the frontal sinuses; the eyes become tender and watery; and sneezing and a copious acrid defluxion from the nose ensue, followed or accompanied by heat and uneasiness about the throat, hoarseness, a troublesome cough, a sense of constriction in the chest, and oppression of the breathing. In short, the symptoms are the symptoms of catarrh; including in that term all the varieties thereof that are sometimes met with separately—gravedo, coryza, bronchitis: and with these symptoms, a sudden, early, and extraordinary subdual of the strength; and, most commonly, great depression of spirits. The debility which comes on at the very outset of the complaint is one of its most singular phenomena, taking place, in some cases, almost instantly, and in a much greater degree than would seem proportioned to the other symptoms of the malady which it thus ushers in. Indeed, this rapid and remarkable prostration of strength is more essentially a part of the disorder than the catarrhal affection, which sometimes (though rarely) is absent, or imperceptible. It is upon the mucous membranes, however, that the stress of the disease generally falls; especially upon the internal lining of the air-passages. Those of the alimentary canal seldom escape entirely; but they suffer in a less degree. The tongue is white and creamy, the palate loses its sensibility, the appetite fails, nausea and vomiting are not uncommon, and sometimes there is diarrhœa. The pulse, in the uncomplicated disease, is soft, and generally weak. The skin, at first hot and dry, soon becomes moist, and sometimes exhales a peculiar musty smell. The patients complain also of pains in the limbs and back, and of much soreness, a bruised, fatigued, or tender feel, along the edges of the ribs, and in various parts of the body.

In its simple form and ordinary course, the disease abates of its violence after two, three, or four days, and the patient is usually convalescent before the termination of the week; but cough and much debility are apt to survive the other symptoms, and while these continue the complaint is very easily renewed. Pre-existing disease, and peculiar constitutional habits and tendencies, modify considerably the character of the influenza, as it affects different persons. It is apt to be complicated with bronchitis, with pneumonia, with rheumatic affections of the joints, with neuralgic pains. I do not attempt to represent in detail its various phases: they are fit subjects of study for yourselves.

I have remarked that Cullen makes this species of catarrh to proceed from contagion. But the visitation is a great deal too sudden and too widely spread to be capable of explanation in that way. I will not say that the disease may not be in some degree infectious; for there is reason to believe that other epidemic disorders, having many points of analogy with the influenza, *are* somehow imparted from one individual to another, although they are mainly produced by some influence which resides in the atmosphere. There are facts in the history of influenza which furnish a strong presumption that the exciting cause of the disorder is material, not a mere quality of the atmosphere; and that it is at least *portable.* The instances are very numerous, too numerous to be attributed to mere chance, in which the complaint has first broken out in those particular houses of a town at which travellers have recently arrived from infected places. But this great and important question of contagion I hope to examine with yoú more rigorously on a future occasion. What I wish to point out now is the fact that the influenza pervades large tracts of country in a manner much too sudden and simultaneous to be consistent with the notion that its prevalence depends exclusively upon any contagious properties that it may possess. You are aware that it has recently seized upon all parts of this metropolis—and I believe I may say of nearly the whole kingdom—within the space of a very few days. It has been observed to occur also, at the same time, on land, and on board different ships which have had no communication with the shore, nor with each other. Thus it is stated in the 'Transactions of the College of Physicians,' that on May 2, 1782, Admiral Kempenfelt sailed from Spithead with a squadron, of which the 'Goliah' was one. The crew of that vessel were attacked with influenza on May 29; and the rest were at different times affected; and so many of the men were rendered incapable of duty by this prevailing sickness, that the whole squadron was obliged to return into port about the second week in June, not having had communication with any shore, but having cruised solely between Brest and the Lizard. This happened in one part of the fleet. In the beginning of the same month another large squadron sailed, all in perfect health, under Lord Howe's command, for the Dutch coast. Towards the end of the month, just at the time, therefore, when the 'Goliah' became full of the disease, it appeared in the 'Rippon,' the 'Princess Amelia,' and other ships of the last-mentioned fleet, although there had been no intercourse with the land. Similar events were noticed in the epidemic of 1833. One or two curious instances of the sudden sickening of considerable bodies of men in different places at the same time were related to me on good authority. On April 3 in that year—the very day on which I saw the first two cases that I did see of the influenza, all London being smitten with it on that and the following day—on that same day the 'Stag' was coming up the Channel, and arrived at two o'clock off Berry Head, on the Devonshire coast, all on board being at that time well. In half an hour afterwards, the breeze being easterly and blowing off the land, forty men were down with the influenza, by six o'clock the number was increased to sixty, and by two o'clock the next day to one hundred and sixty. On the self-same evening a regiment on duty at Portsmouth was in a perfectly healthy state, but by the next morning so many of the soldiers of that regiment were affected by the influenza, that the garrison duty could not be performed by it. I make no doubt that facts of a like nature have occurred during the present epidemic, and will be made known in due time. They illustrate several important points respecting the disease; viz., the impossibility of accounting for its prevalence upon the principle of mere contagion—the suddenness of its invasion—and the early and extreme prostration of strength with which it is attended.

The occurrence of epidemic catarrh, as well as of most other epidemics, is unquestionably connected with some particular state or contamination of the atmosphere. What that state is, or what may be the kind of contamination, no one knows.

The present epidemic followed hard upon the sudden thaw that succeeded the remarkable snow-storm of the final week of the last year. A similar coincidence between the breaking out of the same disorder, and a sudden elevation of the temperature of the atmosphere, happened at St. Petersburgh in the epidemic of 1782. 'On a cold night,' Maertens says, 'the thermometer rose 30° of Fahrenheit; the next morning 40,000 people were taken ill with the influenza.' Now if every epidemic had been preceded by similar changes in the weather, we might resolve the universal prevalence and sudden accession of the complaint into the effect of the cold and damp state of the air, produced by the thaw. But it is not so; for, as Dr. Hancock observes, there has not been any uniform connection between any one sensible quality of the atmosphere as to heat or cold, rain or drought, wind or calm, and the invasion of the epidemic. 'Et tempore frigidiori et calidiori, et flante tam Austro quam Boreâ, et pluvioso et sereno cœlo, peragravit hasce omnes Europæ regiones, et omnia loca indiscriminatim.' Irregularities and great vicissitudes of weather have, however, gone before the disease in very many instances; but sometimes one condition of the atmosphere, and sometimes another, has been its immediate forerunner; and the epidemic has frequently been observed to fall partially and capriciously, as a blight falls upon a field or district. Short, in his chronological history of the weather, says that thick ill-smelling fogs preceded, some days, the epidemic catarrh of 1557. Jussieu states that the grippe of the spring of 1733 appeared in France immediately after offensive fogs, 'more dense than the darkness of Egypt.' So also in 1775, Petit informs us that in France the disease was ushered in by thick noisome fogs. In the same year it visited the shire of Galloway in Scotland, where, we are told, 'a continual dark fog and particularly smoky smell prevailed in the atmosphere for five weeks, the sun being seldom seen.' Dr. Darwin has recorded that, in 1782, 'the sun was for many weeks obscured by a dry fog, and appeared red as through a common mist;' and he supposes that 'the material which thus rendered the air muddy probably caused the epidemic catarrh which prevailed in that year.' You will call to mind here the dark fog which brooded over this city in the midst of the raging of the distemper about ten days ago, and which was repeated, in a less degree, on Wednesday last (Feb. 1).

It has been observed also, that shortly before, or during, or soon after, the prevalence of these epidemic catarrhs, *epizootic* diseases have raged; various species of brutes, and of birds, have been extensively affected with sickness; while on some occasions prodigious swarms of insects have made their appearance. In short, a great variety of facts concur to render it probable that some peculiar condition of the air existed, which, though it might be favourable to the multiplication of some species of living creatures, such as the insects just referred to, operated as a poison upon the human body, and upon the bodies of many of the brute creation.

It is a very curious circumstance in the history of epidemic catarrhs, and worthy of your reflection, that they *travel* ; migrate, as it were, from one place to another ; and moreover, that they hold, for the most part, to certain courses, in spite even of opposite winds, and of variations of temperature. It has been noticed that the influenza generally follows a westerly direction, or one from the south towards the north-west. In this remarkable property it resembles, as you may perhaps be aware, the epidemic cholera.

Although the general descent of the malady is, as I have said, very sudden and diffused, scattered cases of it, like the first droppings of a thunder-shower, have usually been remembered as having preceded it. The disorder is most violent at the commencement of the visitation; then its severity abates; and the epidemic is mostly over in about six weeks. Yet the morbific influence would seem to have a longer duration. In a given place nearly all the inhabitants who are susceptible of the distemper suffer it within that period, or become proof against its power. But strangers who, after that period, arrive from uninfected places have not, apparently, the same immunity.

The locality does not appear to be thoroughly cleared of the poison for some time; or perhaps a more cautious statement of the fact would be, that the disorder generally shows itself again in succeeding years, but in a milder and less general form. This must depend either upon some remaining dregs, or possibly some revival, of the injurious influence; or else upon some abiding predisposition impressed upon the bodies of men by its former visit. You may hear, every year, of Mr. *So-and-so* having the influenza. In many instances, no doubt, common sporadic catarrh is dignified by that name; but it is certain also that many of the colds, and bronchial disorders, of the seasons which immediately follow a period of genuine influenza, are attended with much more languor, debility, muscular aching, and distress, than belong to an ordinary attack of catarrh.

All this is very curious and very mysterious. All this, or much of it, is true also of the diseases which are known to prevail occasionally as epidemics. The facts that have now been mentioned respecting the influenza, warrant, I think, the conclusion that it does *not* depend upon any mutations in the ordinary and obvious qualities of the atmosphere; upon any degrees or variations, I mean, of its temperature, its motions, or its moisture; upon what is expressed in the single word *weather*. Concerning a calamity so generally felt, and so obscure in its origin, conjecture, you may well believe, has not been idle. One hypothesis assigns the complaint to some change in the electrical condition of the air; to its becoming negatively electric; or to its being such as to cause an excessive accumulation of electricity in the animal economy. The facts adduced in support of these views are of this kind. Meat, sent up by means of a kite high into the atmosphere, during the prevalence of the disease, has returned putrid. Large heavy separate clouds, in a state of negative electricity, have been observed just before the setting in of an epidemic. Thunder-storms, and tumults of the atmosphere, have occurred at the same periods. During the raging of one epidemic, 300 women engaged in coal-dredging at Newcastle, and wading all day in the sea, escaped the complaint. It has been thought that this exemption might be accounted for by supposing that the almost constant immersion of the body in a conducting medium prevented any undue collection of electricity.

Again, it has been conceived that the tolerably definite course of the epidemic, in its migrations, might be somehow connected with magnetic currents.

One of the most recent and most plausible conjectures respecting the exciting cause of influenza, is that which refers it to the presence in the atmosphere of an excessive quantity of *ozone*. The attention of physicians was first directed to this substance by M. Schönbein of Bâsle, in a paper which you will find in the 'Medico-Chirurgical Transactions' for 1851. Pure or atmospheric oxygen, when exposed to the action of electrical sparks, is transformed into an odoriferous matter, which is therefore called ozone, and which is believed to be merely an allotropic form of oxygen. Most persons who have stood near an electrical battery at the time of its discharge must have been sometimes aware of the peculiar smell. The same odour pervades the air in thunder-storms. Now this ozone has remarkable purifying properties, which I need not stop to describe. It has also the effect, when breathed in large quantities, of irritating the mucous membrane of the air-passages. While M. Schönbein was engaged in examining its chemical relations, he found that the inhalation of strongly ozonised air produced a painful affection of his chest—a sort of asthma, with a violent cough, which obliged him to discontinue for a time his investigations. Reflecting on this circumstance, he began to suspect that certain catarrhal disorders might be caused by atmospheric ozone. He got several physicians at Bâsle to compare their lists of catarrhal patients with his tables of atmosphero-ozonometric observations; and he and they were struck by the occurrence of an unusual number of catarrhal cases, on the days, or during the periods, when M. Schönbein's test papers showed that ozone was unusually abundant in the air.

This presumable explanation of epidemic catarrhs deserves, and doubtless will receive, a searching scrutiny, whenever the community may again be afflicted with influenza.

Another hypothesis, more fanciful perhaps, at first sight, than these, yet quite as easily accommodated to the known phenomena of the distemper, attributes it to the presence of innumerable minute substances, endowed with vegetable or with animal life, and developed in unusual abundance under specific states of the atmosphere, in which they float, and by which they are carried hither and thither. Myriads of these animalcules, or of these vegetable germs, coming in contact with the mucous membranes, and especially with that of the air-passages, irritate (it is imagined) those surfaces, and exercise a poisonous influence upon the system. Now the sporules of certain fungi, which ruin the health, and destroy the vitality of larger plants, on which they prey, are inconceivably small. I shall prove to you presently, that vegetable effluvia are capable of producing, in the human body, symptoms not very dissimilar from those of influenza. Again, that the waters of this globe swarm with living creatures, which are invisible by our unaided eyes, the microscope has taught us. Others, too minute to be estimated even by that wonder-showing instrument, in all probability exist. We cannot doubt that the gaseous fluid which surrounds this planet, equally teems with living atoms. We know that multitudes of insects, and of cryptogamous plants, infinite in number with respect to our finite powers of computation, *are* sometimes suddenly hatched or developed, in places which were previously free from them. It is easy to conceive that atmospheric infusoria (so to speak) may rapidly congregate, or vivify, in masses sufficient to render deleterious the very air we breathe. If this be so, we can understand how such a cause of disease may first act here and there, and presently overspread large districts; how it may move, or be wafted, from place to place, or be carried about by persons; how its course and operation may be circumscribed and definite; and how some germs or ova may remain after the visit, retaining their vitality, and ready in future seasons again to start into life and activity under favouring circumstances. Taking the insect hypothesis, and knowing as we do, that some animal poisons, (that of small pox, for example,) have the singular property of multiplying themselves in the human body, like yeast in beer, we may conceive that diseases, produced by animalcules, may thus infect the fluids of the body, and become contagious in the fullest sense of that term. Lastly, the uniform duration of these epidemics has been supposed to add probability to the notion that they result from the operation of some organic principle, which has its definite periods of growth and of decay. All this is sheer hypothesis: but I have nothing better than hypotheses to offer you. You may choose from among them, or you may reject them all, as the bent of your minds may incline. The prevalence of the disorder in a city or town while a village a mile or two off remains untouched, is quite conclusive, in Dr. Parkes' opinion, against the hypothesis that ozone, or any other *gas*, may be the cause of the Influenza.

The character of debility, which is so conspicuously impressed upon this disease, bears closely upon the treatment required for its cure, or its safe conduct. As in all other epidemics, the severity of the complaint is extremely variable in different persons. In some it proves a very trifling malady, which soon passes off, and requires little or no assistance from medicine. In others it is a very distressing affection, and lays the foundation for other and still more serious, though more chronic diseases: and in some, and more especially in the old and the unsound, it shows itself a very fatal disorder. The absolute mortality under the recent epidemic has been immense: the daily newspaper obituaries have been unusually long; and you may have remarked that the ages of the persons whose deaths they announced were in almost all cases great. The funerals have been so frequent, that difficulty has been found in performing them without indecent hurry and confusion. One undertaker, of whom I was informed, had at one time seventy-five dead bodies to inter—*above ground*, as he expressed it; and mourning coaches, and black horses, could not be procured in sufficient numbers to meet the demand for them. The absolute mortality, therefore, I say, has been enormous; yet the relative mortality has been small. You will hear people comparing the ravages of the influenza with those of the cholera, and inferring that the latter is the less dangerous complaint of the two; but this is plainly a great misapprehension. Less dangerous to the community at large (in this country at least) it certainly has been; but infinitely more dangerous to the individuals attacked by it. More persons have died of the influenza in the present year than died of the cholera when it raged in 1832; but then a vastly greater number have been affected with the one disease than with the other. I suppose that nearly one-half of those who were seized with the cholera perished: while but a very small fraction indeed, not more probably than 2 per cent., of those who suffered influenza have sunk under it. The only fatal cases that I have seen have been in persons advanced in life, or in persons whose lungs were previously known to be unsound.

Now the *treatment* of the influenza is pretty well understood. The chief risk of mistake is that of being too busy with what are called lowering measures. Certainly those affected by this disorder do not well bear active depletion. Of course no one would think of blood-letting except the symptoms were severe, and the distress great; but even in such cases, much caution would be requisite in adopting that remedy. If you find that the inflammation has extended to the pleura, or to the substance of the lungs, it may be necessary to apply cupping-glasses over the chest, or between the shoulders: but this is a very unpleasant necessity. Such is the result of all that I have seen, and heard from others, of the present epidemic; and such is the result of the recorded experience of nearly all previous epidemics. You will find abundant evidence of this collected in a summary view by Dr. Hancock. In 1510, Dr. Short says bleeding and purging did harm. In 1557, bleeding was said to be so fatal, that in a small town near Madrid, two thousand persons died after it in the month of September. In 1580, Sennertus, after stating that where blood-letting was omitted, the mortality was not greater than one in one thousand, adds, ‘Experientia enim *hoc* comprobavit, omnes fere mortuos esse, quibus vena aperiebatur.’ Dr. Ash observes that, in 1775, it was never necessary to bleed at Birmingham; and that, in a neighbouring town, three died who were bled, and all recovered who were not bled. And a great deal more evidence to the same purpose you may find in the article I have referred to.

I believe the best plan of management—as far as any general plan can be laid down—is to keep the patient in bed, and after clearing the bowels by two or three grains of calomel, followed by a mild aperient, to give a couple of grains of James's powder every six hours, with a saline draught, and slops, till the first brunt of the disorder is over; and then, if the cough be troublesome and the breathing laborious, and much rhonchus, or sibilus, or crepitation be audible in the chest, to apply a blister, and to give expectorants and diuretics. What I prescribed a great many times was something of this kind: half a drachm of oxymel of squills, a drachm of the sweet spirit of nitre, and sometimes another drachm of paregoric, in almond emulsion. With respect to full doses of opium, when the feverishness is abated and the headache gone, I should recommend the same practice as that described in the last lecture. If there be any lividity of the skin, or of the mucous membranes, it is hazardous to give a full dose of opium. On the other hand, if there be no visible indication in the complexion that venous blood is circulating in the arteries, opium given at bed-time will have sometimes a magical effect in relieving distress, and (by giving rest and refreshing sleep) in recruiting the strength also. The newly discovered remedy, chloral, would be still more eligible for the same purpose. In cases in which the powers of the system are prostrate, and the face and lips are livid, and the patient is tugging to expectorate the mucus that is filling up his air-passages, you should have recourse to ammonia, to nourishing broths, and it may be to wine and water: and when all danger from the disease is over, but the patient remains feeble, languid, and out of spirits, then is the fit time to administer tonic medicines; and although snake-root and cascarilla are well spoken of by many practitioners, I know no tonics so good as the sulphate of quina, or of iron, for such patients.

Dr. Richard Pearson, and others, have denounced the use, after the first three or four days, of means calculated to promote perspiration. Although strong diaphoretic remedies, to force much sweating, may not be expedient, I cannot but think that we should rather encourage than check the free action of the skin, through which, in all probability, the poisonous element of the disease is both most readily and most safely let off.

As to external applications, mustard poultices, blisters, and the like—and to the inhalation of the steam of hot water,—these may each and all be very useful; but I have nothing to add concerning the time and manner of their employment to what I said upon the same subject at our last meeting.*

There is a singular variety of catarrh produced by a peculiar local cause, and therefore requiring to be briefly noticed. I have now seen several unequivocal instances of it; and it has been observed and described by many writers. Dr. Bostock, in the 'Medico-Chirurgical Transactions,' gives an account of this complaint as it was apt to attack himself. It is called the *catarrhus æstivus*, and by some the *hay-fever*, or the *hay-asthma*. In Dr. Elliotson's lectures also, as published in the 'Medical Gazette,' there is a good deal of curious information upon this malady, contained in letters addressed to him from practitioners in various parts of the country, in consequence of some previous remarks made upon it by him in a clinical lecture, which had also been printed. Dr. Elliotson speaks of it as a combination of catarrh and asthma. It consists in excessive irritation of the eyes, nose, and the whole of the air-passages; producing, in succession, itching of the eyes and nose, much sneezing occurring in paroxysms, with a copious defluxion from the nostrils; pricking sensations in the throat; cough, tightness of the chest, and difficulty of breathing, with or without considerable mucous expectoration. This complaint affects certain persons only, and in them it always takes place at the same period of the year, in the latter end of May, or in June, when the grass comes into blossom, or when the vernal hay-making is going on. It seems, in fact, to be produced by some kind of emanation from certain of the grasses that are in flower at that season, of the irritating qualities of which emanation some persons only,—and a very few persons in comparison with the entire community,—are susceptible. The disorder happens only at that one particular season; and it then attacks persons who are not remarkably subject to catarrh at other times, nor from the ordinary causes of catarrh; and if they avoid meadows and hay-fields, and the neighbourhood of hay-stacks, they escape the malady. Hence going to the sea-coast,—and especially to those parts of the coast that are barren of grass,—offers a means of protection: and when this cannot be done, such persons obtain refuge, in some measure, from the cause of the irritation, by remaining within doors, and shutting out, as much as possible, the external air, during the hay-crop. One lady, who suffered annually from this strange affection, states that a paroxysm has been brought on by the approach of her children, who had been in a hay-field; and once this happened when the hay-harvest had been for some time over, upon their joining her at tea, after playing in a barn in which the hay of that year had been deposited. She was in the habit of flying to Harwich, or some other part of the coast, as the dangerous season came on. On one occasion, while walking on the shore at Harwich, she was suddenly attacked by the complaint, to her great surprise, as she was not aware of any grass being in the neighbourhood; but the next day she discovered that hay-making was in progress upon the top of the cliff at the time when she was walking under it. In another year, she being at Cromer, and an attack that she had suffered having quite subsided, and all the hay-making thereabouts being over, she was suddenly visited by the well-known symptoms, and on going into her bed-chamber perceived that they were building a large stack of hay in a yard near the house, having transferred it from a field five miles distant.

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* Influenza again prevailed in this country towards the end of 1847, and extended into the beginning of the next year. According to Dr. Farr it killed, directly or indirectly, not less than 5,000 persons in six weeks. Of this epidemic Dr. Peacock has given us a very full and interesting account.

I was asked by Mr. Cheyne to see with him the wife of a stable-keeper near Regent Street. I found her suffering under what is popularly called 'a crying cold:' pain in the situation of the frontal sinuses, streaming eyes, sneezing and defluxion from the nostrils, and very urgent dyspnœa, which was accompanied by loud wheezing. Symptoms of this kind had come on, suddenly, some days before: and her distress was then so great, that her husband proposed to drive her in a gig to consult a medical friend of his who lived in Islington. On their way thither, every symptom disappeared, and she felt at once quite well. She subsequently stayed a night or two, in comfort, with some relations in the city. Immediately upon her return home, the same symptoms recurred, with all their former severity, and resisted the means adopted for their relief by Mr. Cheyne, who had now been called in. He was soon led to suspect the cause of the attack, and of its obstinacy. There was a strong odour of hay in the house. The husband told him that his lofts were filled with a lot of hay which had recently arrived, and which had an unusually powerful smell. We learned that our patient was always worse at night, when the house was shut up; and better in the morning, when a free current of air blew through the open windows. We advised a temporary change of residence: but our advice was not followed until two days afterwards, the disorder meanwhile continuing, and increasing in intensity. Then the patient removed to lodgings not one hundred yards distant; and immediately all the catarrh and distress again ceased, and she passed a perfectly tranquil night. Afterwards she went into the country, and did not return till the odoriferous parcel of hay had been consumed, and a new stock laid in. She was however revisited by some slight cough, and occasional dyspnœa—neither of which troubled her much or long.

Avoidance, then, of the ascertained source of the complaint is the best thing that can be recommended to these persons. You may read almost every year in the newspapers that one of our English dukes has gone to Brighton to escape the hay-fever. But it is not in the power of every one to leave home for that purpose; and it has been found that the system is capable of being fortified in some degree against the pernicious effects of these vegetable effluvia. Mr. Gordon, of Weldon, in Yorkshire, had communicated some interesting observations to the profession on this subject, before those of Dr. Elliotson were published. You may find Mr. Gordon's paper in the fourth volume of the 'Medical Gazette.' He supposes that the aroma of the sweet-scented vernal grass, the *anthoxanthum odoratum*, is the principal exciting cause of the complaint. He found the symptoms more speedily and effectually removed by the tincture of lobelia inflata, than by anything else that he had tried at that time; and he recommended the cold shower bath as the best preservative against the attack. But in a subsequent communication to Dr. Elliotson, he states that the sulphates of quina and of iron, given in combination, had proved completely successful in emancipating from their tormenting disorder the two patients, from whose cases he had principally drawn up his account; although they had, in spite of all previous treatment, suffered an annual return of it for fifteen or twenty years.

The susceptibility of this troublesome affection of the respiratory mucous membrane, from a peculiar cause, which to most people occasions no uneasiness, appears sometimes to run in families; and this is nothing more than one might expect.

Dr. Elliotson, thinking it possible that the chlorides, which have the power of decomposing, and disarming of their noxious qualities, certain *animal* effluvia, might exert a similar control over the *vegetable* emanations that excite the hay-catarrh, suggested to one of the sufferers a trial of the chloride of lime, or of soda. He desired him to have it placed in saucers about his bed-chamber; to have rags dipped in it, and hung about the rooms of the house; to wash his hands and face with it night and morning; and to carry a small bottle of it with him, to smell from repeatedly in the course of the day: and this plan gave so much relief—either by destroying the emanations, or by lessening the irritability of the mucous membranes—that it was tried in other cases; and though it did not succeed in all, it did in most of them. Three patients out of four derived advantage from it. This expedient, therefore, is worth carrying in mind.

The son of an old acquaintance of mine was thought to have been benefited, in an attack of hay-fever, by the *Tinctura nucis vomicæ* of the Dublin pharmacopœia, taken in ten minim doses three times a day.

But the most hopeful preventive and remedy of this vexing disorder is that which has been lately suggested by Dr. Mackenzie. Led, as he has informed me, by the apparent analogy between certain irritable conditions of the skin, and of the mucous surfaces, and by the ascertained beneficial influence of *arsenic* upon some of the former, he tried that drug in the case of a gentleman suffering from hay-fever, in whom previous disappointments had produced despair of obtaining relief from any treatment. The use, however, of from three to five minims of the *liquor arsenicalis*, in distilled water, immediately after each of his three daily meals, was attended with wonderful and speedy success. A like encouraging result followed the adoption of the same plan in other instances by Dr. Mackenzie, and by professional friends to whom he had mentioned it. Since the year 1850 I have recommended this remedy to several persons who complained of being annually harassed by the hay-fever: and from most of these I have subsequently received very favourable reports of its good effects. Dr. Mackenzie believes it to be most serviceable when the symptoms are of a catarrhal rather than of an asthmatic character.

There is another vegetable substance, better known to us, which produces, in some few individuals, symptoms very like those of the hay-asthma: I mean the powder of ipecacuan. I recollect a servant employed in the laboratory of St. Bartholomew's Hospital, when I was a pupil there, who had the peculiar ill-luck to be liable to this affection. Whenever that drug was under preparation, he was obliged to fly the place. This idiosyncrasy is not very uncommon. A very small quantity of the ipecacuan dust is sufficient, in such persons, to bring on a paroxysm of extreme dyspnœa, wheezing, and cough, with singular anxiety, and great weakness. The distress usually terminates by a copious expectoration of mucus. There are persons who are similarly affected by the odour of cats.

These effects of a powdered root, and of certain emanations from grass or hay, lend weight to the hypothesis which ascribes the influenza to subtle vegetable matter floating in the atmosphere.

I would suggest a *trial* of the Respirator, as a defence against the particles of ipecacuan, and against the volatile exciting cause (whatever it may be) of hay-asthma.

Catarrh is very often met with in a chronic form; in other words, the mucous membrane of the air-passages is very liable to be affected with chronic inflammation. The accounts which you may read of this are exceedingly puzzling. Authors have endeavoured to draw nice distinctions between different *species* of chronic catarrh; sometimes according to varying qualities in the matters expectorated: thus you have *chronic mucous* catarrh—*pituitous* catarrh—*chronic pituitous* catarrh—and *dry* catarrh, which, after all, is *not* dry, but only accompanied by less expectoration than some of the others; and then again there is *symptomatic* catarrh. You will find all these enumerated by Laennec; and the majority of writers since his time have trodden with too submissive reverence in his footsteps. There are by no means such differences in the symptoms or in the proper treatment of the several varieties of chronic inflammation of the membrane in question, as to make these numerous subdivisions of any practical utility. Chronic or moderate catarrh is often a sequel of acute bronchitis; it is a very common accompaniment of disease of the heart; it frequently arises during the course of the febrile exanthemata; it is seldom entirely absent in cases of continued fever; and it is a form of complaint that is full of interest on this account, if on no other, that it has so often been mistaken, and is so liable to be mistaken still, for tubercular consumption; of which indeed it is very frequently the companion.

The *constant* symptoms of chronic catarrh, or bronchitis, are cough, some shortness of breath, expectoration of altered mucus. The *variable* symptoms, those which are oftentimes of the most importance, as determining the slight or the serious character of the disorder, consist in the quantity and quality of the matters expectorated, and the presence or absence of wasting, and of hectic fever.

You will continually be meeting with cases of *this* kind. A person advanced in years has what he calls a slight cold, in the winter. He coughs, and expectorates a certain quantity of gray or transparent mucus. In the summer his cough diminishes, or ceases altogether. The next winter the same thing happens again; and each successive return of the colder seasons of the year brings back in increasing severity the cough and the expectoration: and if you listen to the breathing of such persons, while the cough is on them, you will find crepitation at the lower part of their lungs. Now these are examples, I believe, of a chronic state of slight inflammation of the membrane—or it may be of passive congestion and effusion—depending upon slowly advancing *cardiac* alterations. Peripneumonia notha is very apt to supervene on this condition.

But chronic bronchitis may take place at any age, as a sequel to the acute: just as active inflammation of other parts of the body is liable to degenerate into the chronic form; and such cases are sometimes very equivocal and deceptive. Several years ago, a lady became my patient, having cough, expectoration of puriform matter, night-sweats, and diarrhœa. She had had hooping-cough a short time before; but though the hooping and other symptoms proper to that disease had ceased, she continued to cough, and to waste. Gradually she got thinner and weaker, her pulse became like a thread, and beat 120 times in a minute; she took to her bed, the diarrhœa was scarcely restrained by astringents and opiates, and I thought she could not live a week. And upon being pressed by her brother for my opinion, I said so. She had scarcely allowed me to listen to the sounds in the thorax: but I had once done so fairly, and I could find no morbid sounds, except at the lower part of the lungs. If I had trusted to that circumstance alone, I should have said that she had not pulmonary consumption; but I had not then so much faith in the indications afforded by auscultation, nor in my own accuracy of ear in such matters, as I might have now; and I concluded that she *was* dying of pulmonary phthisis. Almost on the day, however, on which I ventured to give this prognosis, some slight amendment began: and she did gradually recover, and is alive and quite well at this time. Now it is in cases of this kind that cures are performed by those who boast that they can cure consumption.

In truth, chronic bronchitis is, in some cases, as incapable of recovery, and as surely and progressively fatal, as tubercular phthisis itself, and even more so than some of the forms of phthisis. So long, however, as *no organic change* has taken place in the air-tubes, or in the mucous membrane lining them, these chronic forms of bronchitis that simulate phthisis in their general symptoms, are within the reach of cure. They are to be treated by counter-irritants to the chest—and by such measures as are calculated to relieve the most urgent symptoms. Opiates for cough, or for diarrhœa. Sometimes the patients bear steel well, and then it is almost sure to have a beneficial effect. Sometimes sarsaparilla appears to do good; but, as far as I have observed, one of the most effectual restoratives in these cases is to be found, when the weather and the strength permit, in frequent change of air and place; in gentle gestation in a carriage, or in a boat; and in a nourishing but bland and unstimulating diet. When the membrane, and the tubes which it lines, become *altered in structure*, and pour forth a fluid which has all the qualities of pus, hectic fever generally is present, and the chronic disease tends, slowly perhaps, but surely, to death.

There are certain cases of chronic bronchitis which are especially remarkable, on account of the great abundance of the bronchial secretion: so great that the patients appear to die principally from the daily exhausting drain thus made upon the system. There are sometimes no other evident signs of inflammation; so that, as Andral observes, one might be led to separate these fluxes from the truly inflammatory affections. They differ from them apparently in their nature, and certainly in the treatment which they require. Andral has detailed two or three instances of this kind in his ‘Clinique Médicale.’ The patients expectorated every day large quantities—a pint or more—of frothy fluid, resembling weak gum-water in colour and consistence. They had no fever; neither frequency of pulse nor heat of skin; but they were exceedingly pale, like persons blanched by hæmorrhage, and their emaciation and weakness were also extreme. Very little appreciable deviation from the healthy state was detectable when the lungs and heart were examined after death.

It does occasionally happen that even larger quantities—three or four pints daily—are, for a considerable period, spat up, without much wasting.

Andral asks, whether, in such cases as these, which certainly occur, although they are not very common, the first indication of treatment should not be to check and diminish the excessive bronchial secretion; to treat it as you might treat a gleet of the other mucous membranes, with balsams, administered either by the stomach, or in the shape of vapour. He conjectures that it might have been in cases of this nature that the vapour of tar, and tar-water, were once thought to be so useful. Probably the creasote would be well adapted to such cases. Certainly I have seen the excessive expectoration diminish, and the patients gain strength, under the use of the balsams; the compound tincture of benzoin, for example; a form of medicine much employed formerly, and too much neglected, I apprehend, at present. Another remedy from which I have derived great advantage in some cases of the same kind, is the sulphate of iron, given in two- or three-grain doses, in the compound infusion of roses, thrice daily. When there is any fever present, these remedies may perhaps augment it: but when the pulse is quiet, and the skin cool, I am quite sure that they are often of the greatest service; and this you will likewise find to be the opinion of various practical authors.

There is another very remarkable condition of the pulmonary mucous membrane, constituting also, I imagine, a species of chronic inflammation, and characterised chiefly, like the last, by the matters expectorated. I mean that state in which a firm substance, resembling a false membrane, forms in the smaller bronchi and in their ramifications, and is coughed up, from time to time, in fragments. I mentioned in a former lecture that the false membrane of diphtheria sometimes descends a long way into the bronchi; even to their extremities. But I am speaking now of a different and less acute form of disease, in which the trachea being unaffected, concrete masses, evidently moulded in parts of the hollow bronchial tree, are spat up; somewhat like bunches of worms, or the branching roots of a small plant. This I presume to be uncommon; for I have met with it twice only in my life. It has been described, however, by several observers. The first Dr. Warren has a paper upon it in the first volume of the 'Medical Transactions,' where he gives representations of the substances coughed up, which he calls *bronchial polypi*. Dr. Paris told me that a patient of his coughed up considerable quantities of these branching casts of the ultimate air-tubes, now and then, for a long period. An interesting paper of Mr. North's, on the same subject, was read at one of the evening meetings at the College of Physicians. That gentleman possesses some beautiful specimens of these miscalled polypi. Sir R. Carswell gives a figure representing them. When the affection is extensive, it is attended with great distress, and dyspnœa, and violent fits of coughing; and the symptoms are wonderfully calmed upon each expulsion of the solid matter. The surprise is that such patients should ever recover; but I have never heard of an instance in which the complaint proved fatal.

The two examples of it which have fallen under my own observation, were invested, by the circumstances attending them, with a peculiar interest. They occurred, within less than a twelvemonth of each other, in the persons of two brothers, of middle age, the one a barrister, the other well known to you all as one of my most valued colleagues in this place. Both of them were, and are, remarkably stout, strong, and healthy men.* In both cases the expulsion of the so-called polypi was preceded by hæmoptysis, which came in considerable gushes, and was repeated at intervals of a few days, until the solid matters began also to be expelled, and then the hæmorrhage soon subsided.

The barrister, after having been annoyed for nearly a year by some huskiness of the voice, spat up, all of a sudden, a small quantity of bright blood; and soon afterwards expectorated several ramifying masses of tolerably firm consistence, resembling fibrinous coagula of blood, deprived of most of its colouring matter. Some of them, which I saw and examined, were solid; others, I understood, were hollow. I found slight circumscribed crepitation in the lower and posterior part of his left lung. This trivial degree of hæmoptysis, with the expulsion of what looked like casts of the interior of a bronchial tube, was once or twice repeated within a few days. He had no fever—no dyspnœa. Mercury, *inter alia*, was prescribed; but as the patient did not feel in any way ill, I believe he soon became tired of physic, and of medical restraint. Whenever I have since seen him, he has appeared to be in perfect health.

Of the Professor's illness I saw more. In the midst of health which had been uninterrupted, save by a solitary fit of gout some years before, he also spat some mouthfuls of florid blood. He had no cough, but the hæmoptysis was accompanied by a rattling sensation in the right side of his chest.

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* Since this was written, Professor Daniell has been suddenly cut off, to the great grief of all who knew him, by a stroke of apoplexy.

For about three weeks he continued, at intervals varying from three to six days, to expectorate blood, in gushes. The smallest quantity brought up on any one occasion was two ounces; the largest, eight. Just above the right nipple the respiratory murmur was mingled with large crepitation, which was always sensibly increased, and quite perceptible by the patient himself, during the attacks of hæmorrhage. In the intervals between them his breathing and his pulse were perfectly tranquil and regular.

With the florid blood came up, in general, some black coagula; and at the end of three weeks, or thereabouts, in these black masses ragged shreds of a different and firmer material began to be visible: and presently afterwards, complete branch-like casts of the ramifying air-tubes were expelled; and the bleeding ceased.

Of these casts there were two kinds: the one solid, somewhat coloured, evidently fibrinous, and resembling the branching coagula that may sometimes be drawn out of the arteries in the dead body; the other white, membranous, tubular, but ramifying also. None of them were of very firm consistence.

Till these substances made their appearance, our anxiety about the patient was extreme, and he underwent some rigorous discipline at our hands. He was confined to bed, forbidden to speak, kept strictly to the slenderest slop diet, several times bled, and extensively blistered. Lumps of ice were given him to swallow, and pounded ice was applied to his chest whenever the blood broke forth afresh. He took mercury till his gums were tender, and afterwards the acetate of lead, and other reputed styptics.

To most of this I was a consenting party; but looking back upon the case now that its nature and result are known, I must confess that the treatment, though fairly justifiable at the time, was unnecessarily active.

Mr. North, in the paper to which I have alluded (you may see it in the twenty-second volume of the 'Medical Gazette'), draws a distinction, of which he gives the credit to Dr. Cheyne, between the hollow, membranous concretions, expelled without any blood; and the solid branching masses which accompany or succeed hæmoptysis, and are obviously mere coagula of blood moulded in the smaller air-tubes, where it had stagnated. He points out the comparatively harmless character of the cases in which the first occur; and the far more serious import of the second: the hæmorrhage denoting the presence of some organic mischief within the thorax, and the 'polypous concretions' being simply an accident of the hæmorrhage.

I doubt the accuracy of this distinction. The brothers of whom I have spoken continue to be, as they were before, free from any symptom or suspicion, either of cardiac or of pulmonary disease. Moreover, in hæmoptysis depending upon tubercles in the lungs, or upon organic disease of the heart, these concretions are very rarely observed. I have never seen them in such cases, common as such cases are. The barrister had a husky voice, and the Professor was noticed to have been often 'clearing his throat' for some time before the first eruption of blood: from which circumstances I infer a previous unhealthy state of the mucous membrane. Upon the whole I incline to the views expressed by Dr. Todd, with whom I had the advantage of consulting in the latter case—that a chronic and limited inflammation of certain of the bronchial tubes first occurred, disclosing itself by no marked symptoms, but leading to the formation of tubular membranes; that, after a while, these membranes began to be detached; that hæmorrhage resulted, and continued till the separation was complete; and that, at the same time, some of the extravasated blood coagulated in, and took the shape of, the air-tubes, and was afterwards expectorated.

I understand that the barrister has since had a recurrence of this strange complaint, which he treated very lightly, and soon got rid of.

He entertains a fixed belief that his attacks were attributable to the presence of one of Dr. Arnott's stoves in his chambers; the heated atmosphere of which always produced a slight feeling of constriction and distress within his chest. Whether this notion be well or ill founded I cannot pretend to say; but it is curious that the Professor also had been using a similar stove, which, placed in his sitting-room, warmed both it and his bedroom adjoining it.

In the fifth volume of the 'Transactions of the Pathological Society of London' is contained an interesting summary by Dr. Peacock, of thirty-four cases, recorded by various authors, in which fibrinous casts were expelled from the air-tubes.

From this summary it would appear:—

That the affection occurs more frequently in men than in women.

That though it is not limited to any period of life, it is most common in persons of middle age.

That generally the moulds proceed from bronchial tubes of the third or fourth magnitude, and from their branches; the trunks having the average size of an ordinary goose-quill.

That hæmoptysis is quite as often absent as present; and that the only pathognomonic sign of the complaint is the expectoration of the membranous shreds or casts.

That the attacks generally cease after a week or fortnight, but are sometimes protracted over several weeks; and that they are apt to recur, from time to time, for months or years.

That in itself the complaint is not a dangerous one; and that when death takes place during its progress, the fatal issue results from some disease of which the membranous exudation is merely a complication. Some of the patients who died were manifestly consumptive; and most of those in whom this peculiar expectoration has been noticed were originally of delicate constitution; or had shown previous tokens of pulmonary weakness, or of pulmonary disease.

In the sixth volume of the 'Pathological Transactions' are communications from Dr. Bristowe and Dr. Wilks, showing that solid casts of the bronchial tubes are occasionally associated with acute pneumonia.

It would appear then that these fibrinous casts are sometimes, and most often, the result of *exudation from* the mucous membrane lining the air-passages, that membrane being in some unsound state—of what has been called *plastic* bronchitis: sometimes the result of the coagulation of matters *poured into* the air-passages in the course of some other complaint, and independent of any inflammatory or other morbid condition of the mucous membrane itself.

A word or two more, before we separate, as to the morbid anatomy of these tissues.

Chronic inflammation of the aërial mucous membrane may lead to changes in its colour; or to thickening of the membrane; or to ulceration; or to dilatation of the bronchi, and of their ramifications. And it is proper that you should be informed respecting these morbid conditions.

In general, when chronic inflammation has existed during life, the mucous membrane is found to be red: but it is not a bright redness; it is rather a livid, or violet, or brownish tint. And what is very curious, in some instances in which all the symptoms of inveterate bronchitis, with *puriform* expectoration, had been present, the inner membrane of the air-passages has been found scarcely rosy—or even perfectly white—throughout its whole extent. Of course we are not to infer from this that there has not been *inflammation*; for the same thing is known to occur in the intestinal mucous membrane, in that of the bladder, and even in serous membranes. Where pus is poured forth there must have been inflammation.

One effect of inflammation, as I formerly showed you, is a softening of the membrane: but this is a much less common result of inflammation in the mucous membrane of the bronchi, than in that of the digestive organs. In regard to ulceration likewise there is a great difference between the two mucous surfaces: in that of the air-passages it is comparatively rare.

Thickening of the mucous tissue occurs also in various degrees: and in connection with the various morbid conditions of this membrane, I must bring briefly under your notice some remarkable changes, produced by disease, in the tubes which it lines; and especially the dilatation of those tubes.

There are two or three varieties of this dilatation. In the first of them, one or more of the bronchi present, throughout the whole or the greater part of their extent, an increase of capacity which is often very considerable: so that tubes which result from the fourth, or the fifth, or even the sixth division of the principal bronchus of each lung, may equal or exceed in diameter that bronchus itself. Tubes that ought to be no bigger than a crow-quill may become as large as the finger of one's glove. The parietes of the dilated tube are thickened, and its circular fibres hypertrophied, as you may see in this preparation, and in Sir R. Carswell's plate. Sometimes this kind of dilatation is seen in a single branch only, sometimes in many branches. It may affect the bronchial ramifications of an entire lobe. It is more common in the branches of a bronchus than in the bronchus itself.

With respect to this sort of dilatation, it is observable that the pulmonary tissue lying in immediate contact with the enlarged tubes is usually, if not always, impermeable by air; it may be from some bygone inflammatory process; it may be from mere collapse and its attendant atrophy. Sir Dominic Corrigan, and after him Dr. William Gairdner, have shown the strong probability that the dilatation is a consequence of this unnatural condition of the surrounding tissue; and that it is gradually produced by 'the expansive forces of inspiration acting upon the bronchi of atrophied lung.'

In the second variety of this change, instead of the uniform dilatation of one or more bronchial tubes, throughout their whole extent, we find a bellying, or globular expansion, at the extremity of one of them; and the walls of the tube, instead of being thick and hypertrophied, are wasted, and in a state of atrophy. The tissues composing the tube are often so thin, that when the cavity, for such it must be called, is laid open, the colour and structure of the pulmonary tissue may be seen through them. These cavities are generally found filled with a thick, tenacious, straw-coloured, muco-purulent fluid. They are seldom met with except after those affections which are characterised by considerable secretion from the membrane, and by much and repeated cough: as after some forms of chronic bronchitis, and after hooping-cough.

A third variety of dilatation is that in which the same bronchus bellies out in different places; is dilated at intervals; so as to present in its course a series of successive enlargements and contractions. Here, again, the walls of the bronchi, though they may be traced in the parts dilated, do not appear to be thickened, but rather are diminished in thickness. This variety of dilatation is more frequent in children than in adults.

Dr. William Gairdner gives a plausible account of the formation of these globular or sacculated expansions. He states that, after bronchitis, pus is sometimes found imprisoned in the central air-branches of a collapsed lobule. The coats of these tubes, injured and softened by disease, gradually give way and ulcerate; and the pus, which accumulates, is at length surrounded by a false membrane exactly similar to that of an abscess in any other part. The continuity of this membrane with that of the ministering bronchus may either exist from its first formation, or be established subsequently. These 'bronchial abscesses' communicating at length freely with the tubes leading to them, may afterwards be expanded beyond their former dimensions by the respiratory forces.

In whatever way dilatations of the bronchi may take place, it is to be expected, when they are numerous or extensive, that they should be attended with some habitual shortness of breath.

But the most important consideration arising out of these conditions of the bronchi, is this; that the signs both general and physical, by which they are accompanied, are apt to be exactly those which are most distinctive of phthisis. And it is on that account that I have now described these changes. I shall revert to them again when I come to the symptoms, revealed by auscultation, of tubercular disease of the lungs.

LECTURE XLIV.

Hooping-cough: symptoms; duration; complications; pathology; treatment.—Pneumonia: its stages and morbid anatomy; auscultatory signs.

I HAVE yet to consider one very important disorder, which is usually classed among the catarrhal affections, but which is marked by features so peculiarly its own, as to distinguish it effectually from every other form of disease. I allude to *hooping-cough*: a remarkable complaint, well known everywhere, I believe, and much dreaded by parents. It has received a variety of names: chin-cough; kink-hoast; coqueluche; tussis convulsiva; tussis ferina; and *pertussis*. This last name, which Sydenham bestowed upon it, and which was adopted by Cullen, is the technical appellation of the disease in this country, as hooping-cough is the popular.

The phenomena that characterise hooping-cough are, I say, remarkable. It begins with the symptoms of an ordinary catarrh arising from cold. The child (for it is most especially a disease of children) has coryza, and coughs: and mothers and nurses are aware that the disease commences in this way, and express their apprehensions lest it may *turn* to the hooping-cough. After *this*, the *catarrhal* stage, has lasted eight or ten days, or a fortnight, or sometimes a day or two longer, that kind of cough begins to be heard which is so distinctive. It comes on in paroxysms, in which a number of the *ex*piratory motions belonging to the act of coughing are made in rapid succession, and with much violence, without any intervening inspirations; till the little patient turns black in the face, and seems on the point of being suffocated. Then one long-drawn act of *in*spiration takes place, attended with that peculiar crowing or hooping noise, which denotes that the rima glottidis is partially closed, and which gives the disease its name. As soon as this protracted inspiration has been completed, the series of short expiratory coughs, repeated one immediately after the other till nearly all the air appears to be expelled from the lungs, is renewed; and then a second sonorous back draught occurs: and this alternation of a number of expiratory coughs, with one shrill inspiration, goes on, until a quantity of glairy mucus is forced up from the lungs, or until the child vomits, or until expectoration and vomiting both take place at once. During the urgency of the paroxysms the face becomes swelled, and red or livid, the eyes start, the little sufferer stamps sometimes with impatience, and generally clings to the person who is nursing him for support, or lays hold of a chair or table, or of whatever object may be near him, to diminish (as it would seem) the shock and jar by which his whole frame is shaken. As soon as expectoration or vomiting have happened, the paroxysm is over. The child may pant a little while, and appear fatigued; but commonly the relief is so complete, that he returns immediately to the amusements, or the occupation, which the fit of coughing had interrupted, and is as gay and lively as if nothing had been the matter with him. When the fit terminates by vomiting, the patient is in general seized immediately after with a craving for food, asks for something to eat, and takes it with some greediness.

Each paroxysm may consist of several alternations of the gasping coughs, and the characteristic hoop or kink; but Cullen remarks, that the expectoration or the vomiting usually takes place after the second coughing, and puts an end to the fit.

The number of paroxysms that occur in the twenty-four hours is variable also: and they come on at irregular intervals. The nocturnal paroxysms are commonly more severe than those of the day. When the complaint is uncomplicated, the child, during the intermissions, appears to be quite well. This is another striking feature of the disorder. In the earlier paroxysms the mucus expelled is scanty and thin; and in proportion as this is the case, the fits are the longer and the more violent. By degrees the expectoration becomes more abundant: and sometimes it is very copious: at the same time it is thicker, and more easily brought up; and on that account the fits of coughing are less protracted.

The ordinary duration of the disease is from six weeks to three months; but it may run its course, I believe, in three weeks; and it may continue for six months, or more.

In an uncomplicated case, if you listen at the chest during the intermissions, you will probably hear the sounds that are proper to catarrh—some degree of rhonchus or sibilus: and in some parts there may be puerile respiration; and if you percuss the thorax, you get the natural hollow sounds. But what happens when you apply your ear to the chest during the paroxysms of coughing? Why, the information given us in this case by auscultation is very curious. You may perhaps hear, between the short explosive shocks of the cough, some snatches of wheezing, or of vesicular breathing; but during the long-drawn noisy inspiration that succeeds, all *within* the chest is silent. This is supposed to result from the slow and niggardly manner in which the air passes towards the lungs through the chink of the glottis, which is spasmodically narrowed. It may also depend, in part, as Laennec supposed, upon a spasmodic condition of the muscular or contractile fibres of the bronchi and their branches. When the fit is at an end, the ordinary sounds of healthy, or of catarrhal respiration are resumed.

Children are very susceptible of this complaint; and it is a complaint which spreads by *contagion.* Hence it follows that few children escape an attack of it. It is also one of those contagious maladies which do not in general affect the same individual twice; and hence again it follows that it is rarely met with in adults. Such is the fact; and such, I apprehend, is the explanation of it. It is not that adults are unsusceptible of hooping-cough: for adults that have not had it during their childhood are readily affected when exposed to the contagion. But it is that the disorder, with very few exceptions, protects the system somehow from its future recurrence; and that most adults have *had* it when they were young, and for that reason do not take it afterwards.

During the very early periods of infancy, i.e., within the first two or three months, hooping-cough is said to be rare: I mentioned a case, however, before—and I have read of others—in which the disorder appeared to have been contracted before the patient was born. My bedmaker's daughter in Cambridge had a child ill with hooping-cough in the house with her during the last week of another pregnancy, and the new comer hooped the first day he appeared in the world.

As long as this disease is uncomplicated — unmixed with inflammation, and therefore, unattended with fever, or only with that slight inflammatory condition proper to mild catarrh —it is seldom a *dangerous* disease. Probably it *will,* under the most favourable circumstances, run a certain course. By degrees the violence and the frequency of the paroxysms diminish; they occur only in the morning and the evening, then in the evening alone, and at length they cease altogether. But for some time after the disorder has apparently come to an end, if the child take cold, and get a cough, the cough is apt to assume a spasmodic character, and to be attended with a hooping noise in inspiration.

Nevertheless, when it is very intense, the disorder *may* have a fatal issue, without any organic complication. Before the little patient has fairly rallied from one paroxysm, another succeeds it; the spasmodic closure of the larynx is more and more complete and lasting, until, at length, little or no air can pass; the characteristic hoop becomes short and faint, or ceases altogether; and the child dies of apnœa.

Unfortunately, too, hooping-cough is, in a great many cases, not simple—not uncomplicated. It becomes mixed up with other kinds of disease, in the chest, or in the head. In the chest severe bronchitis supervenes upon it, or inflammation of the substance of the lungs; and then fever is lighted up, and permanent dyspnœa is present. When the disorder has been long drawn out, and has at last terminated fatally, dilatation of the bronchi, such as I described in the last lecture, is often found upon dissection; still more commonly what is called *emphysema* of the lungs—a change which I have yet to bring before you; but most frequently of all, pulmonary collapse of the lobular kind.

Collapse of the lung I have already shown you to be ordinarily the result of obstruction of the air-tubes by mucus accumulated within them. The risk of such obstruction is always present in hooping-cough. But there are co-operating causes; to most of which these young patients are also liable. Whatever impedes the free and full indraught of air may be a co-operating cause: the spasm therefore which, narrowing the inlet, diminishes the supply of air; a tumid abdomen, hindering the contractions of the diaphragm; weakness of the muscles of inspiration, arising from general debility. And there is yet another accessory cause, which is peculiar to the early years of life. In the full inspiration of an adult the thorax is enlarged by the separation and the upward movement of the unbending ribs, and by the simultaneous descent of the diaphragm. But in young children, when the inspiratory act is difficult and forced, the ribs yield under the power of the contracting diaphragm, which drags them inward, and thus the full expansion of the lung is stinted. In this way permanent deformity of the chest is sometimes produced. We need not wonder then that more or less of pulmonary collapse should be a nearly constant phenomenon after death from hooping-cough.

Neither can we be surprised that the disease frequently leads to cerebral disorder. During the fits there is a great and visible determination of blood towards the head, or rather a detention of the blood in the veins that proceed from the head; —passive mechanical congestion: the transmission of the blood through the lungs being obstructed, and its return from the head interrupted. Hence, the face becomes turgid, the eyes are prominent, the superficial veins full and projecting, the lips and cheeks turn livid; sometimes hæmorrhage takes place from the nose or ears; or the eyes become blood-shotten; or the patient actually falls into convulsions; nay, apoplexy is occasionally the result of the straining; and when life is not thus suddenly cut short, chronic mischief is apt to be set up in the brain, and the child may ultimately die hydrocephalic.

All this is the more to be feared in proportion as the child is the younger. Head affections are particularly to be dreaded in scrofulous children; and in any children during the first dentition. When the disease occurs within the first two years of life, it is usually attended with convulsions: and many more die within that period than afterwards. And Cullen's remark is undoubtedly true; that the older children are, the more secure they are, *cœteris paribus*, against an unhappy event.

Hooping-cough may be complicated also with a disordered condition of the bowels; and with infantile remittent fever. This complication is more accidental, and less a *consequence* of the hooping-cough than the former; but it may very materially add to its peril.

Dr. Cullen was of opinion that the complaint may exist in even a milder form than that which I have called simple hooping-cough. He thought he had seen 'instances of a disease, which, though evidently arising from the chin-cough contagion, never put on any other form than that of a common catarrh.' Others again believe that adults may have it without hooping. But all this seems to me very doubtful. Catarrh is an exceedingly common malady; and I should be slow to consider any case a genuine case of pertussis, unless the characteristic paroxysms of coughing, and the stridulous inspiration, were present.

Divers opinions have been held respecting the seat, and respecting the nature, of hooping-cough. Some suppose it to have its seat in the brain: others that it is a specific variety of catarrh, and has always therefore more or less of an inflammatory character: others again, looking to its spasmodic symptoms, ascribe the disease to some morbid influence exercised upon the pneumogastric nerve: and this last I believe to be the truest view of the matter. The disorder belongs to a very remarkable group of blood-diseases, of which I shall have much to say hereafter, and is produced by an animal poison. Certainly the simple form of the disease is often unattended with any appreciable fever: and that is a strong ground for concluding that its peculiar phenomena are not *necessarily* connected with inflammation. They who have ascribed the complaint to a morbid condition of the brain have deduced that opinion, I presume, from the cerebral symptoms that are sometimes so plainly marked in hooping-cough. But these symptoms are oftener, to all appearance, the consequence, than the cause, of the paroxysms of coughing. I would suggest it as an interesting point for your future enquiry, whether the pathology of hooping-cough may not receive some elucidation from the researches of the late Dr. Ley respecting the crowing inspiration of infants. You remember his suggestion, that mere inflammation of the mucous membrane of the air-passages might cause swelling of the absorbent glands of the bronchi, or of the neck. This is a circumstance which I have myself long thought probable, from having found enlargement of the cervical glands springing up during the existence of pulmonary irritation. Take notice that the spasmodic fits of hooping-cough are always preceded for some days by mere catarrhal symptoms. Observe further how the parts supplied by the pneumogastric nerve are affected in these paroxysms: the larynx, the lungs, the stomach. This conjecture, that the crowing inspiration of infants, and the crowing inspiration of hooping-cough—though quite distinct affections—may both depend upon irritation of the recurrent nerve, or of the pneumogastric nerve generally; and that even the irritation might in both cases arise out of enlargement of the glands that lie in the course of that nerve: this natural conjecture had presented itself to Dr. Ley's mind; for, towards the end of his book, I find this note:—‘ Recently four children have been brought to my house, labouring under hooping-cough. In all, the glandulæ concatenatæ near the trachea were very considerably enlarged. Is this (he says) merely an accidental combination? or is there any essential connection between the two? May it not be that an enlargement of these glands, from a specific animal poison, similar to that of the parotid glands in mumps, is, after all, the essence of hooping-cough? The subject at least deserves enquiry, and further observation.'

In corroboration of this conjectural view of what *may* ultimately prove to be the true pathology of hooping-cough, I may remark that among the morbid appearances described as being met with after death from that disease, 'an unusual swelling of the bronchial glands' is set down. It is also stated, by some of the Germans, that that portion of the pneumogastric nerve which lies in the cavity of the chest has been sometimes found red. Yet I should lay no stress upon this; for others have asserted that they have looked in vain for this redness: and even supposing it to exist, it is no sure or safe token that there had been inflammation of the nerve. The nerve, all things considered, would be likely to become tinged of that colour soon before, or even after, death, from the gorged condition of the lungs. In some cases, as you may well believe, serous fluid is met with in the ventricles of the brain, or in the meshes of the pia mater: in others the consequences of inflammation are traceable in the bronchi, the lungs, or the pleuræ. Portions of what is called hepatised lung are not unfrequently seen in the fatal cases: though less frequently than portions of collapsed lung.

The object of rational treatment in hooping-cough, supposing the disease to be simple, is to *keep* it simple: to keep it *mere* hooping-cough: to obviate serious inflammation, or mischief, in the chest and head: and, if possible, to mitigate the severity and shorten the duration of the fits of coughing. I have no notion that anything we can do in the beginning will materially abridge the duration of the complaint as it appears in its unmixed form. It *will*, I say, in all probability, run a certain course; and our business is to conduct it evenly and safely to the *end* of its course. For this purpose the diet must, in the first place, be regulated and reduced. The child should be allowed to eat but little meat; it may be nourished as well, and more safely, upon milk, and unstimulating farinaceous matters. The bowels should be kept moderately open: and the patient in cold weather should be confined to the equable temperature of the house, or protected by warm clothing; and care should be taken to keep the internal temperature equable. The air of the bedroom should not be colder than that in which the child has passed the day. It should not be much above nor much below 60° Fahrenheit. You will find different persons employing and praising different plans of treatment; the object in all cases, however, being the same, viz., to *ward off inflammation*, and to *quiet irritation*. One very good plan, as I believe, is that of giving a grain, or a grain and a half, of ipecacuan, three or four times a day. This generally keeps the bowels sufficiently open, and seems to have a beneficial operation on the mucous membrane of the air-passages also. Or a few grains of rhubarb and of ipecacuan may be given every night: and if the cough be very troublesome and urgent, small opiates may be administered: syrup of poppies: or the extract of hyoscyamus; as many grains *per diem* as the child has years. There is a method recommended many years ago by a namesake of mine, which some people swear by. Sir William Watson's prescription was one grain of tartarised antimony and twenty drops of laudanum in an ounce of water. A tea-spoonful, or a dessert-spoonful, of that mixture was given every evening, or every other evening. I have heard the late Dr. Gooch say that his mother became famous as a village doctress by the help of that prescription. Fothergill's method was to give an emetic every day; or three or four times a week: and this plan is the more suitable when there is much wheezing, and the phlegm is brought up with difficulty. The best emetic substance in such cases is, doubtless, ipecacuan: and the best time for giving it is the evening. Mr. Pearson—who has had, I fancy, many imitators—used to prescribe, after the operation of an emetic, one drop of laudanum, five drops of ipecacuan wine, and two grains of carbonate of soda, in a draught, every fourth hour, for several days. Under some such treatment as this, the disease will reach its termination in from six to twelve weeks: and it frequently happens that when the child is quite well in all other respects, it still continues to *cough*. The cough would almost seem to be kept up by the mere influence of *habit*. Now, under these circumstances, change of air will often remove the cough, as if by magic: and the shower-bath, and iron in some shape, will sometimes succeed, if change of air be not practicable.

There is a great variety of medicines lauded as *specifics* against hooping-cough: but they are not to be trusted to. Many persons think highly of the prussic acid, as a remedy for the paroxysms of coughing. Others employ and praise the extract of belladonna. But these are gigantic remedies to employ upon such young subjects. If you give them at all, you must give them in very small quantities, and watch their effects. Dr. West thus records the result of his own experience in respect of the hydrocyanic acid, administered in minute doses—

'This remedy sometimes exerts an almost magical influence on the cough, diminishing the frequency and severity of its paroxysms almost immediately; while in other cases it seems perfectly inert; and again in others, without at all diminishing the severity of the cough, it exerts its peculiar poisonous action on the system, so as to render its discontinuance advisable.' The artificial tincture of musk is another substance which some have found useful. Three or four minims of it may be given in the outset, and the dose increased till some sensible effect is produced; and then the dose that has been so reached should be persisted in, without further augmentation. I have been assured, by a most intelligent practitioner, that he had got considerable credit by prescribing this medicine, after other persons, with other modes of management, had failed. Alum, in doses of three or four grains every four or six hours, has been given, with apparent benefit, when there has been much expectoration, and no fever. Digitalis, and cantharides, are other, and, I think, hazardous remedies. Safer drugs recommended, and, for aught I know, equally efficacious with these poisons, are cochineal, oil of amber, musk, camphor, and the meadow narcissus. Of late the carbonate of iron has been greatly commended by some of the continental physicians. The bromide of potassium is sometimes very useful; and so, probably, would be the hydrate of chloral.

External applications are also much in fashion in the treatment of hooping-cough. Frictions to the spine and to the chest; and as counter-irritants, they probably are of some service. The tartarised antimony is the least innocent of these applications. It will often cause foul and very troublesome sores upon an adult skin: and till I am better advised than I am at present of its certain efficacy as a remedy for hooping-cough, no one (however authorised professionally *ludere corio humano*) should rub it upon a child of mine. Mothers are many of them fond of using Roche's Embrocation for the hooping-cough. This (Dr. Paris tells us) consists of olive-oil, mixed with half its quantity of the oils of cloves and of amber.

Such is the plan of management which you will do well to enforce—and such are the expedients which you may, if you please, make use of as auxiliaries to that plan—when the disease is *mere* hooping-cough. But when it becomes complicated with symptons of inflammation within the chest, or with head symptoms—(and for such symptoms you must jealously watch)—then you must employ *remedies* (in addition to the prescribed *regimen*) adapted to the circumstances of the case. Now we have reason to believe that the *bronchi*, or the *lungs*, are affected with inflammation, when we find that the child has *fever*, and that there is *permanent dyspnœa* between the paroxysms of spasmodic cough. In such a case we must have recourse to the treatment required in such inflammation: leeches to the surface of the chest, tartar emetic, small doses of nitre, the warm bath, and blistering; and to these measures, modified and combined according to the particular emergency, it may sometimes be well to add small and repeated doses of mercury; of the hydrargyrum cum cretâ, or of calomel; the state of the bowels determining which. Some have recommended friction with the tartar emetic ointment upon the *chest* in such cases: but I have the same objection to it there, in patients so young, as upon the spine.

Permanent dyspnœa, unattended with fever, is more likely to depend upon collapse, than upon inflammation, of the pulmonary substance. Under such circumstances any active lowering measures would be out of place, and even hurtful. You must search therefore for auscultatory evidence as to the condition of the lungs, and adjust your treatment accordingly.

When any head symptoms come on, threatening hydrocephalus, or apoplexy—such as squinting, convulsions, stupor—those remedies must be adopted which I endeavoured to describe to you when I spoke of those diseases; leeches to the head, cold applied there, purgatives, the warm bath: but, except in very young children, I believe there is more danger of fatal *pulmonary* changes in this disagreeable, and sometimes intractable disorder, than of cerebral mischief.

I might pass, by a very natural transition, from the consideration of hooping-cough, to that of *spasmodic asthma*. But this last complaint is found to exist in connection with *various* organic changes within the chest, few of which have yet been treated of in these lectures. I shall therefore postpone what I have to say respecting asthma, till I have gone through some other thoracic diseases. And I now proceed to *pneumonia*, or inflammation of the *substance of the lungs*. Questions have been raised as to the precise part and texture in which the inflammation begins; and to these questions I may briefly advert as we go on: but I hold that in pneumonia *all the textures* composing the pulmonary substance in the part inflamed are involved in the inflammatory process.

Now of pneumonia it is especially true, that we ascertain its situation, its extent, and every step of its progress, by means of the ear. All the symptoms that give us the most sure information respecting the nature of the disease, its increase and aggravation on the one hand, or its abatement and diminution on the other, spring out of the actual changes wrought in the pulmonary substance itself; and these changes are disclosed to us by the method of auscultation. It is necessary, therefore, that you should understand, first of all, what those changes are which are produced by inflammation of the substance of the lungs: that you should know the morbid anatomy of pneumonia, as an indispensable groundwork for a knowledge of its pathology.

There are three well-marked, and very constant conditions of the lung, corresponding to different degrees and periods of its inflammation. I will describe them in succession, in the order in which they take place.

The first stage or condition is that of *engorgement.* All modern observers agree, I believe, both as to the nature and as to the name of this condition. The substance of the lung is gorged with blood, or bloody serum. It is of a dark red colour externally, and crepitates less under pressure than sound lung does. We feel that there is more liquid than air in its cells. It is heavier also than natural, and inelastic, and retains, in some degree, the impression of the finger. When the engorged portion is cut, we find it red, and we see a great quantity of a reddish and frothy serum flow from it. Its cohesion is at the same time diminished: it is more easily torn; more, in that respect, like the spleen; and accordingly the term *splenisation* of the lung has been given to this stage of its inflammation, as hepatisation has to that which succeeds it. In this stage of engorgement the mucous membrane of the smaller bronchial ramifications is of a deep red colour. The portions most engorged, although their specific gravity is increased, will nevertheless almost always float on water.

Now it is necessary to caution you in the outset, against a very frequent source of fallacy with respect to this condition of inflammatory engorgement. Such a state of the pulmonary substance as I have been describing, you will meet with in half, at least, of the dead bodies which you may have to examine; and you must not necessarily infer therefrom that the persons deceased had *inflammation* of the lungs. There is almost always some degree of *mechanical* engorgement of the back part of the lungs; or of that part which has been undermost during the last hours of life, or after death; and the two kinds of engorgement can scarcely be distinguished from each other by their physical characters alone. Andral at one time held, indeed, that if the engorged part were more friable, more easily torn or broken down under pressure than natural, that was sufficient evidence of its inflammation: but he afterwards saw reason to change that opinion. We judge by the *situation* of the engorgement sometimes; if it be not in a depending part of the lungs, it is surely inflammatory. We judge also by the antecedent symptoms.

If the inflammation continue, the lung undergoes a further alteration, and presents the following characters. It is still red—externally and within: but it crepitates no longer under pressure; and it sinks in water: it contains in fact no air. Its cut surface presents sometimes a uniform red colour; sometimes a slightly mottled or variegated appearance, produced by an intermixture of specks of the black matter of the lung, and of the interlobular areolar tissue, which is less red than the other parts, and more than naturally obvious to the sight: but the spongy character of the organ is lost; it is evidently solid; and the cut surface very much resembles the cut surface of the liver. Hence Laennec, and after him most other writers, have applied to this altered condition of the lung the term *hepatisation.* There still flows out, under pressure, from the surface, when a fresh incision is made, some red fluid, but it is much less in quantity than in the former degree; and it is not foamy; and if the surface be gently scraped with a scalpel, you may often perceive in the red fluid so collected, some traces of a thicker and yellower matter, the first indication of commencing suppuration. The hepatised lung is denser and more solid than before, but it is also more friable; more easily crushed and broken: and this results from the softening of the areolar tissue which holds its component parts together.

If you tear a portion of hepatised lung, and examine the torn surface with a magnifying glass, the pulmonary tissue will appear to be composed of a crowd of small red granulations, lying close to each other. These are, I presume, the air-vesicles clogged up, thickened, and made red, by the inflammation. As no air is contained in the lung in this stage of the inflammation, it follows that if the entire organ be involved in the disease, it will not sink down when the thorax is laid open; and will therefore appear to be increased in bulk. It is swelled, in fact,—just as other inflamed parts are swelled—by the congestion of its vessels, and by the effusion of blood, or of some of the constituent parts of the blood, into its hollows and interstices. The marks of the ribs are frequently visible on the surface of the distended lung. The texture of the lung in this condition is sometimes so rotten, that a moderate degree of pressure between the fingers will suffice to reduce it to a state of pulp; and this diminution of consistence has made Andral quarrel with the term hepatisation: and he proposes to call this second stage of pneumonia, red softening, *ramollissement rouge.* All this is very unimportant, provided that you recollect the sense in which either nomenclature is employed. But as Laennec and Andral are both great authorities, and both have their disciples in this country, it is well that you should understand their language.

In a degree still further advanced, the pulmonary tissue, dense, solid, and impervious to air, as in the last stage, undergoes an alteration of colour: it presents a reddish yellow, or straw, or drab, or stone colour; or it is of a grayish hue, sometimes mottled with red, or with the black pulmonary matter. The little granulations which I just now mentioned are whitish or gray, instead of being red; and the texture of the lung is still more rotten and friable than before. It is full, in fact, of puriform matter, which is sometimes so abundant that it oozes out plentifully when incisions are made into the lung: or it may be made to exude by gentle pressure. The gray pus shows itself upon the cut surface in the form of minute drops. The more the pulmonary texture is soaked or drenched with this fluid, the softer and more friable it becomes. When crushed between the thumb and fingers, it is reduced to a yellowish gray pulp, exactly like the fluid itself, only rather more consistent. And by gently forcing the finger into any part of the parenchyma in this state, a small cavity may be made, which soon fills with pus, and which might readily be mistaken for a recently formed abscess.

Laennec has called this third stage of the process of inflammation in the lung, *gray hepatisation*, or *purulent infiltration.* Andral denominates it *gray softening—ramollissement gris.* In fact, it consists in *diffused suppuration* of the pulmonary texture. And it is a very remarkable circumstance, and one which the researches of modern times have brought to light, that, in the lung, inflammation going on to suppuration, does not lead to the formation of a circumscribed abscess, as it does when it affects the areolar tissue, or the parenchymatous tissue, in other parts of the body. Abscess of the lung used to be spoken of as a very common thing; but it is a very rare thing. In several hundred dissections of persons dead of pneumonia, made by Laennec during a space of more than twenty years, he only met with five or six collections of pus in the inflamed lung. Once only did he find a *large* abscess of that sort. Once only has Andral seen a real abscess of the lung form as a consequence of pneumonia. You may find collections of pus in the lungs sometimes, occurring in connection with the inflammation of veins. Several instances of that kind have happened very recently in patients who have died in the Middlesex Hospital. But these are not ordinary cases of pneumonia. I need scarcely caution you not to take tubercular vomicæ and cavities, containing pus, for genuine abscesses of the lung. These, and the deposits of pus in pyæmia, are not *exceptions* to the general statement: they arise from different forms of disease: and you will find a circumscribed collection of pus, surrounded by hepatized lung, as a consequence of common uncomplicated pneumonia, to be an exceedingly rare event.

Can we account for this in any way? I do not know that any satisfactory explanation of the fact has ever been offered. But I would submit to your consideration what has occurred to my mind on this subject. When I was speaking of inflammation in general, I pointed out to you the remarkable influence which the presence of atmospheric air in contact with the inflamed part has in accelerating, or determining, the event of suppuration. In a recent cut through the skin, the admission or exclusion of the air to the cut surface will make all the difference between the adhesive and the suppurative inflammation; and so in other cases which I then mentioned, and will not now trouble you by repeating. Now it seems to me that the same principle obtains in inflammation of the lung. First, there is an effusion of serum and blood, then of lymph and blood; but the air, passing into the surrounding sounder tissue, and penetrating for a time even the inflamed portion itself, causes the suppurative process to supersede the adhesive; and so no wall of circumvallation is formed by the coagulable lymph, as is the case in areolar tissue when not accessible by the air. Whether this be a sufficient explanation of the *fact* (all explanations being the resolving a given fact into a certain class of other facts more general and comprehensive), I say, whether it be a reasonable and satisfactory explanation, you will judge: at any rate it may serve to impress upon your memory that fact which it endeavours to elucidate.

Gangrene is sometimes, but very seldom, the result of acute inflammation of the lung. It is almost as uncommon as the formation of an abscess. Yet it certainly does now and then occur, as a consequence of acute inflammation of the pulmonary substance. It is somewhat more common (though under any shape rare) as an independent and primitive affection. Sometimes it occupies a large portion of the lung, and is uncircumscribed; and sometimes it is more limited. The colour of the part which has thus perished under inflammation, is dark, of a dirty olive, or greenish-brown colour. The gangrenous portion is moist and wet; sometimes of the consistence of the engorged lung; more commonly softer, and even diffluent; and it stinks most abominably. This horrible odour is in truth, during life, the most distinctive character of gangrene of the lung. It sometimes renders the room in which the unhappy patient is lying, scarcely endurable. I should have stated before that the puriform infiltration of the third stage of pneumonia is attended with no fœtor.

There are some other points, connected with, or learned from investigating, the morbid anatomy of pneumonia, which I may as well take this opportunity of telling you, before we go on to consider the symptoms, physical and general, of that disease.

There are two lungs, just as there are two tonsils, and two eyes; and in the one case as well as in the others, inflammation may affect both organs at once, or it may affect one of them alone. Technically speaking, pneumonia may be either double or single. Again, the inflammation may occupy a part of one lung, or the whole of it: in other words, it may be partial or general; but it does not affect all parts, or both sides, indifferently or capriciously. In the first place, it is (why I know not) greatly more common on the right side of the body than on the left. I will give you some statistical statements collected by Andral, with respect to this point. Of one hundred and fifty-one cases of pneumonia, noticed at La Charité, ninety were of the right lung alone; thirty-eight only of the left alone; seventeen on both sides at once; and in six the situation was uncertain. He was at the pains of collecting the particulars of fifty-nine other examples of pneumonia, from different authors, so fully described as to leave no doubt about the nature and situation of the disease. Among these, the inflammation existed in the right lung alone in thirty-one patients; in the left alone in twenty; and on both sides at once in eight. Hence, taking both series of observations together, we have two hundred and ten cases of pneumonia; and there were one hundred and twenty-one in which the right side was solely the seat of the disease; fifty-eight in which the left; twenty-five in which the pneumonia was double: and six in which the seat was uncertain. So that, at this rate, pneumonia is more than twice as common on the right side as on the left; and does not occur on both sides together so often as once in eight times.

Again, with regard to that *part of the lung* which is most obnoxious to inflammation, there are remarkable differences. It is well known, and it is a very important fact in respect of diagnosis in some cases, that the lower lobes are more liable to inflammation than the upper. I speak, of course, of active idiopathic inflammation. But this circumstance, much insisted on by Laennec, and quite true in the main, has perhaps been somewhat exaggerated. I have not had leisure to frame any numerical statement of the cases that have come under my own observation, but the general impression which they have left upon my mind is in favour of the correctness of Laennec's statement—that pneumonia generally commences in the lower lobes, and spreads upwards frequently to the superior lobes. But I may adduce Andral's statistical representation respecting this question also. Of eighty-eight cases of pneumonia, he found that the inflammation affected the inferior lobe forty-seven times, the superior lobe thirty, and the whole lung at once, eleven times.

Inflammation of the bronchi constantly accompanies inflammation of the parenchyma. The mucous membrane presents a red colour both in the large and in the small branches of the air-passages. And when a single lobe is inflamed, it has been observed that the redness of the mucous membrane existed in those bronchial tubes alone which were distributed to that lobe. You may have bronchitis without pneumonia: but pneumonia without a corresponding extent of bronchitis, is perhaps never seen.

The majority of cases of pneumonia are attended also with a degree of inflammation of the investing membrane of the lung: there is some pleurisy. So frequently indeed is this the case, that certain writers, Andral among others, call the disease by the compound name of *pleuro-pneumonia*. However, pneumonia may and does sometimes occur without any concurrent pleurisy.

Of the latter complaint I must speak by itself; and I merely notice now the frequent combination of the two—the occurrence of a slight degree and extent of pleuritis in most cases of pneumonia—that you may the better understand some of the general symptoms of pneumonia.

Now, such being the changes which the lungs undergo when inflammation affects the pulmonary texture, we may next inquire what signals of its existence the inflammation holds out; and how far we, not having the power of *seeing* what is going on within the cavity of the thorax, may nevertheless ascertain the important processes which are there transacted.

If the ear be applied to the surface of the chest, with or without the intervention of the stethoscope, and the portion of lung subjacent to that surface happen to be in the first stage of inflammation, that of engorgement, what does the lung, so suffering, *say*? what audible notice does it give of its morbid condition? Why it speaks very plainly. You hear a peculiar crackling sound: the smallest and finest-possible kind of crepitation: which has been happily illustrated by saying that it resembles the multitudinous little crackling explosions made by salt when it is scattered over red-hot coals. Andral has another resemblance for it, and not a bad one; he says the noise is often like that which is produced by rumpling a very fine piece of parchment. Dr. Williams observes that a pretty correct idea of this sound may be obtained in a ready way, by rubbing between the finger and thumb a lock of one's own hair, close to the ear. Laennec calls this *crepitant rhonchus*: I would speak of it as *minute crepitation*; or the *crackling of pneumonia*. This may be heard in a very limited spot in the beginning. And what an important sound it is! It is a direct symptom having immediate reference to the structure of the part. And if we consider what the part is, and what the disease; the part the lungs, and the disease inflammation: we can scarcely value too highly this single and simple symptom which gives the earliest and surest intimation that a disease so grave and important as pneumonia has begun.

At first, when you catch the inflammation in its earliest stage, this minute crepitation, which announces *commencing* engorgement of the part, is heard mingling with the ordinary vesicular breathing; obscuring the natural sound, though it does not yet entirely cover it. But as the inflammation advances, the crackling becomes more and more pronounced, until at length it totally supersedes the natural sound. So long as the natural vesicular breathing overcomes the crackling, we may conclude that the inflammation is slight. But if the crackling should, in its turn, become predominant, if it should ultimately mask the murmur of respiration entirely, that infallibly denotes the advance of the pneumonia, and teaches us that it tends to pass from the first into the second degree. But the crackling sound does not *long* remain in any part. As the case proceeds, the sound is less and less heard, and at length is not heard at all, in that spot; and it may be succeeded by one of two very different things. Its place may be taken by the natural respiratory murmur again. When this is so, it denotes the *resolution* of the inflammation. But the crackling may cease, and either no sound at all be heard in its stead, or another morbid sound which I shall presently describe: and this teaches us with absolute certainty, that the disease is growing more severe and serious; that the lung is becoming, or has become, *hepatized*.

Let us inquire, for a moment, before we go any further, what is the nature and where the seat of this minute crepitation, so characteristic of the commencement of pulmonic inflammation. With respect to its *seat*, I apprehend, there can be no question. It proceeds from the very smallest ramifications of the bronchi, and from the air-vesicles themselves. The common opinion is, and such, I confess, is mine, that the sound is the same in cause and kind, only different in degree, with the large and the small crepitation described in a previous lecture: that it results from the passage of air through liquid; from the formation and bursting in quick succession of a multitude of little air-bubbles. The bubbles are necessarily minute, for they are formed, and they explode, in very slender tubes. This is Andral's view of the matter. Laennec does not appear to have formed very clear notions on the subject. But a different explanation has been offered by a well-known and able writer on the auscultatory signs of disease, in this country: I mean Dr. C. J. B. Williams. He holds that the distended blood-vessels, and the interstitial serous effusion, press upon the minutest bronchial ramifications, and obstruct, without wholly preventing, the passage of the air through them: that these small tubes are lined by a viscid secretion, such as is expectorated, and such as I shall have to describe: that the sides of the tubes stick together in consequence of the presence of this viscid matter; and that it is the separation of these adhering sides by little portions of air, which successively pass in and out, that gives rise to the characteristic sound. However, what it is important to remember is, that the crackling sound proceeds from the minutest divisions of the air-tubes, and from the ultimate vesicles of the lungs.

Sometimes, I say, when this crackling ceases, the ear applied to the corresponding surface of the chest, feels it heave up in inspiration, but catches no sound at all. Much more commonly, however, a *new* sound reaches the ear. It is not the vesicular rustle; it is not the minute crepitation: but a whiffing sound is audible, like that produced by blowing through a quill. Little gusts of air are puffed in and out; most distinct, often, at the termination of a slight cough or hem. This is the sound to which the term *bronchial respiration* has been given; and the name expresses well the fact. I mentioned before that in the healthy state we do not hear the air pass through the larger bronchi during inspiration and expiration: the sound doubtless is made, but it is obscured and hidden by the smooth rustle of the vesicular breathing, which comes from the spongy lung surrounding the large divisions of the bronchi, and intervening between them and the ear. But that spongy structure is now filled up. The hepatized lung admits air to pass through the larger bronchi, which are still patent, but it admits none into the vesicles and smaller tubes. It crepitates not when pressed between the thumb and finger; in fact, it is converted into a solid substance, and conducts the sound, in the living body, as any other solid substance might: and therefore the whiffing, blowing, gusty sound of the breath, as it enters and departs from the larger bronchial tubes, which still remain open, is conveyed to the ear, and *bronchial respiration* is heard. At the same time, and in the same place, another auscultatory phenomenon generally arises, and admits of a similar explanation. The *voice* of the patient descends into the pervious bronchi, and is conveyed to the ear of the listener through the solid lung: and it is quite altered by that circumstance. The tone of it is modified; it sounds like the voice of one speaking through a tube. It is totally different from the same voice heard through the healthy lung at the corresponding point on the other side. It approaches in distinctness and quality, but it does not reach, the sound of the speaker's voice heard through a stethoscope placed over his trachea. A humming and muttering are audible, but the words are not distinctly articulated into the ear. It is hard to describe these things in words. Three minutes at the bedside of a patient in whom the bronchial breathing and the bronchial voice were tolerably well marked would put you in possession of them for ever. They are striking sounds, requiring no fine tact to distinguish; and they are exceedingly informing sounds. But I must resume this subject when we meet again.

LECTURE XLV.

Pneumonia continued: its general symptoms; pain, dyspnœa, delirium, cough, expectoration. Course of the disease. Prognosis. Treatment. Lobular pneumonia.

I WAS describing, at the close of the last lecture, the auscultatory signs which lead us to the knowledge that the inflamed lung, in a case of pneumonia, has passed from the first into the second stage of inflammation, and become solid, or hepatized. The altered condition of the organ gives rise to altered sounds. Instead of vesicular breathing, which is the natural sound; or of minute crepitation, which is the sound belonging to the first stage of the inflammation; we either hear no sound at all, though we feel the chest heave up against our ear, or we hear what I described under the denomination of *bronchial respiration*; that is to say, a blowing sound, which is conveyed to the ear from the larger and still pervious branches of the bronchi, through the solid portion of lung around them, and through the solid walls of the chest. This is what the listener hears when the patient *breathes*. And when he speaks his *voice* is heard, much more resonant than is natural, much more resonant than in the corresponding spot on the opposite healthy side of the chest, entering the same open air-tubes, and conducted to the ear by the dense and solid lung. We thus become acquainted with two entirely new sounds; sounds which are never heard in the healthy state of the lungs; *bronchial respiration*, and *bronchial voice*, or *bronchophony*: and you will do well to remember these two sounds, and to familiarise your ear with them; for they speak a most significant language in *other* pulmonary diseases, as well as in pneumonia.

But I say, sometimes we hear these morbid sounds, in the case in question, and sometimes we hear *no* sound at all during the breathing. How is that? Why the existence and degree of the bronchial respiration, and bronchial voice, vary according to the place and extent of the inflammation. These morbid sounds are most plainly marked, where the number and size of the bronchial tubes involved in the hepatization are the greater. They are most distinct, therefore, when the inflammation occupies the upper part of the lung; or the central parts, what are called the roots of the lungs; and when it extends thence to the surface: but when the lower portions alone are inflamed, or the inflammation is merely superficial or partial, they may not be heard at all. Again, if the hepatization should be so general and complete as to prevent the chest, on the affected side, from expanding—you will, in that case, hear no bronchial *respiration*; for the air in the large bronchi must be stagnant. This condition, however, rarely happens: when it does happen, *bronchophony* may remain, unless the bronchi are full and obstructed.

When we have bronchial respiration, usually also we have dulness on percussion. The degree in which this is present will depend upon the circumstances of the case. If a portion of crepitant and permeable lung, even a thin portion, should intervene between the inflamed parts and the walls of the chest, there will still be resonance on percussion, though it will not be exactly the natural resonance. If the hepatized part come close up to the ribs, the sound elicited by mediate percussion will be flat or dead. With all this, you will generally hear, in the sound lung, if the whole of the other be engaged in the inflammation;—or in those parts of the inflamed lung that are healthy;—you will hear, I say, *puerile respiration*: and this is a strong confirming symptom that a part of the breathing apparatus is spoiled, and that the remaining part is endeavouring to compensate for its deficiency.

Now this period in pneumonia, when no sound but bronchial breathing is audible during respiration, is a period of anxious and painful interest. We cannot tell whether the lung will revert gradually to its healthy state; or whether it is passing into the third stage, that of purulent infiltration. I shall tell you presently of some symptoms not auscultatory which may aid in determining that question. But taking first the most favourable of these two suppositions—what *auricular* information do we get to that effect? Why, *there*, where for a while we heard nothing but bronchial respiration, a slight crepitation begins again to be distinguishable, especially at the end of each act of inspiration: gradually this increases in extent and intensity, and as it increases, the bronchial breathing, and the bronchial voice, become proportionately less distinct, because the texture of the lung is again becoming permeable by air, and therefore a worse conductor of sound. By degrees, the bronchial breathing and voice disappear altogether; the vesicular murmur begins again to mix with the crepitation, and at length supersedes it; and the lung is restored to its previous fitness for the purposes of respiration. The same symptoms therefore recur, over again, but in a reversed order; the *returning* crepitation is however coarser and larger, and less regularly diffused, than that of the *advancing* pneumonia:—and even when nothing is heard in the ordinary condition of the breathing but the natural vesicular rustle, some crepitation is found for some little while to mingle with it towards the end of a deep-drawn inspiration. Next, let us take the *worst* of the two suppositions. Auscultation has traced the disease *through* its stage of engorgement, and *into* its stage of hepatization. Can it trace it further? I believe not with any certainty. We cannot say whether the lung remains in the state of hepatization (as it may remain), or whether it has passed into the third stage. But at last, if the structure of the lung break down, and a portion of it be expectorated, air finds its way into the vacant spot, and gives rise to large gurgling crepitation. But other signs sometimes come to our aid when this state has been reached.

We often find, after death, the three degrees of pneumonia existing in different parts of the same lung; and therefore it is not to be wondered at that the different parts of the chest should during life yield sounds indicative of each of those degrees, or at least of the two first; minute crepitation *here*, bronchial breathing, and bronchophony, and dulness on percussion *there*, and in another spot, *no* sound at all, or on the other hand, *puerile* respiration.

Again, it must be confessed—and I am desirous of confessing it, for I am sure that the method of auscultation is brought into undeserved suspicion and disrepute by attempts made to assert its all-sufficiency in all cases—it must be confessed that in some instances, although pneumonia exists, the ear is able to collect nothing of it: nothing indicative of its situation, or of its extent, or even of its existence. The pulmonary expansion is clear, all over the thorax; nay, much more strong than is natural; and this circumstance justifies the belief that, from some cause or other, not *necessarily* from pneumonia, a portion of the lung has ceased to discharge its function, and the other portions have taken it up. This failure on the part of auscultation happens when the inflammation occupies a small portion only of the lung, and that portion is central, or deeply situated—at a distance from the walls of the chest.

Such are, then, the physical signs that accompany and reveal the successive changes of texture, destructive and reparative, which take place in inflammation of the lungs. I do not know whether I have made them clear to you; but I know that no very long apprenticeship, if I may so speak, in the wards of a hospital, will be sufficient, with a little guidance, to render you master of them. There are indeed varieties, and modifications, and exceptions, which nothing but such an apprenticeship can ever teach you. Of these it would be idle and unprofitable for me here to speak: and I go on to consider the *general* signs of pneumonia; some of which, either in themselves, or in combination with the *physical* signs, are of no less importance than these.

In the majority of cases the commencement of inflammation of the lung is marked by shivering, followed by heat and increased frequency of pulse; in one word, by inflammatory fever: and at the same time, or presently after, a stitch in the side comes on, with cough, and a sense of oppression in the chest. In other instances the disease steals on more insidiously, and succeeds to bronchitis; the inflammation appearing to propagate itself by little and little from the larger to the smaller bronchi, and ultimately to reach the air-vesicles themselves, and the interstitial textures; and this may be accomplished with or without the sharp pain or stitch in the side. At first the cough may be dry, but it soon is attended with a very characteristic sort of expectoration. The dyspnœa is sometimes but slight in the outset; sometimes considerable.

Apart, therefore, from the physical signs, we may say that the usual symptoms of pneumonia are pain, more or less severe, on one side, sometimes on both sides, of the chest; dyspnœa; cough; a peculiar kind of expectoration; and fever.

The pain in pneumonia appears to exist only in those cases in which the inflammation of the lung is accompanied by some degree of pleurisy. But these are the most numerous cases. It is most commonly experienced on a level with, or a little below, one or other breast; but it may exist in almost any other part of the thoracic parietes. Generally it is most severe at the beginning, declines by degrees, and ceases altogether for some time before the *pneumonia* ceases. It is aggravated by cough; by a full inspiration; often by sudden changes of posture; by pressure made upon the ribs or intercostal spaces; or by percussion of that part. For the same reason the patients cannot lie on the painful side. Andral declares that in all the individuals in whom he had noticed this pain, and who died, he found the pleura inflamed, and covered more or less with coagulable lymph; and, on the other hand, that he had constantly known the absence of pain coincide with a sound condition of the pleura. When there is no sharp pain, there is, however, some morbid sensation, of trouble, or tightness, or weight, or heat, on the affected side. He quotes, with approbation of its justness, the ancient observation respecting pneumonia—'Affert plus periculi quam doloris.' When I come to speak of pleurisy as a distinct and substantial affection, I shall revert to this pain.

It is, or it was, a common doctrine, that one of the general symptoms of pneumonia relates to the posture which the patient assumes; that the *decubitus*, to speak technically, is on the side affected. The truth, however, is what I have just now stated. The *breathing*, indeed, is more impeded when the patient lies on the sound than when on the diseased side; but in point of fact, patients labouring under this disease almost all lie upon their backs; the decubitus is dorsal. The disturbance of the breathing deserves some notice. In general it bears a direct proportion to the extent and severity of the inflammation. But there are many exceptions to this. In some persons the inflammation of even a small portion of one lung is attended with great constraint or hurry of the respiration. In others, who have a much larger portion of the pulmonary tissue intensely inflamed, the dyspnœa appears to be but slight. So that the degree of difficulty of breathing is not a *certain* measure of the seriousness, or rather of the extent and the degree, of the inflammation. It is probable, that if we knew of what kind was the ordinary breathing of the individuals thus differently affected, we should find that they whose respiration is generally indistinct, or noiseless, who do not seem to *want* all their lung for the purpose of breathing, would best bear to have a part of it inflamed; and *vice versâ*. *Cœteris paribus*, inflammation of the upper lobe causes greater dyspnœa than inflammation of the lower. I may observe further, with respect to dyspnœa in general, that you must not trust implicitly to what patients tell you on that head. They will often deny that they have any shortness of breath, when one may see them respiring with unnatural rapidity, or observe that in their discourse they pause between every three or four words to take breath.

However, the dyspnœa that occurs in pneumonia varies greatly both in degree and in kind in different cases. Sometimes it is so slight that the patient is not conscious of it, and the physician scarcely perceives it. Sometimes it is so extreme that the patient, entirely regardless of what is going on about him, seems wholly occupied with respiring; is unable to lie down; can scarcely speak; his face becomes lividly red or pale, and is expressive of the utmost anxiety; his nostrils are expanded, his shoulders elevated, and all the muscles which are auxiliary to the diaphragm and intercostals, in full and evident action. In one word, the breathing is *laborious*. Now this is the sort of dyspnœa which I mentioned before as being characteristic of obstructive bronchitis. When it accompanies inflammation of the lungs, we may conclude that bronchitis is superadded to that disease. Sometimes again the respiratory movements are simply frequent and very short or shallow, as if the air were not able to penetrate beyond the primary divisions of the bronchi. Dr. William Gairdner, who has closely studied these differences and their meanings, expresses them clearly in the following sentences:—'The dyspnœa of *pure* pneumonia is a mere *acceleration* of the respiration, without any of the heaving or straining inspiration observed in bronchitis, or in cases where the two diseases are combined. So much is this the case, that I have repeatedly observed patients affected with a great extent of pneumonia in both lungs, and in whom the extreme lividity, and the respirations, numbering fifty or sixty in the minute, showed infallibly the amount to which the function of the lung was interfered with; and who nevertheless lay quietly in bed, breathing without any of the violent effort, or the disposition to assume the erect posture, so constantly accompanying the more dangerous forms of bronchitis. If this freedom from dyspnœa and laborious breathing be not uniformly characteristic of true pneumonia, it is because that disease comparatively seldom exists uncomplicated by some degree of bronchial affection.'

Between these states of extreme rapidity or extreme labour of breathing, and the slightest hurry or embarrassment of respiration, there are of course many degrees.

Delirium is a symptom which very frequently occurs in the course of an attack of pneumonia; and a very ugly symptom it is: whether it depends upon a morbid condition of the blood, which has preceded and excited the pulmonary disease—or whether it is a secondary consequence of the pectoral mischief, which is thus telling upon the *brain*, by the circulation of imperfectly aërated blood.

The cough, in pneumonia, has no particular character; and affords but little information. It does not usually take place in paroxysms; and its severity and frequency are not always proportioned to the intensity and extent of the inflammation. It is usually dry in the outset; but in a few hours it is accompanied by the expectoration of peculiar sputa, which constitute one of the most certain indications of the presence of pneumonia: and as this is a symptom which every one can easily recognise, I will describe this characteristic expectoration, and endeavour to explain its cause.

The expectoration of pneumonia, when well marked, consists of transparent and tawny or rust-coloured sputa, uniting, in the vessel containing them, into one jelly-like and trembling mass: and of such viscidity that the vessel may be turned upside down, and strongly shaken, without their being detached from its bottom or sides. It cannot be said that when there is no such expectoration as this, there is no pneumonia: but it may be affirmed that where we do find such expectoration, there almost certainly we have pneumonia. At the outset of the disease, either nothing is spat up, or simply some bronchial mucus: but on the second or third day generally, the matters expectorated assume the characteristic appearance: *i.e.*, they come to be composed of mucus, intimately united and combined with blood. It is not that the sputa are *streaked* with blood, as often happens in *bronchitis*: nor have we the *unmixed* blood of *hæmoptysis*. But the blood and the mucus are amalgamated together: and in proportion to the quantity of the former, the sputa become of a yellow colour, or of the colour of rust, or of a decided red: and at the same time they become glutinous and tenacious; they adhere together, so as to form one transparent homogeneous mass. So long as this mass flows readily along the sides of the vessel when it is tilted, so long have we reason to *hope* (judging from that circumstance alone) that the inflammation of the lung does not pass its first degree. But, as I said before, the sputa often acquire an extraordinary degree of viscidity: so as no longer to separate themselves from the vessel when it is inverted: you cannot even shake them out. When this happens, we are obliged to *fear* that the pneumonia reaches its second degree. In fact, when the sputa become thus rusty and very viscid, the stricken chest almost always returns a duller sound, and the vesicular breathing is abolished, and bronchial respiration has taken its place. The pneumonia is then at its acme; and the expectoration remains for some time stationary. At length, if the inflammation recede, the sputa become again less tenacious, less red or yellow, and more like the expectoration of mere catarrh. But if the disease go on from bad to worse, the rust-coloured sputa may continue to the end. Commonly there is *less* expectoration in that case, or even none at all. Not that the mucus ceases to be secreted, but that its *ex*cretion is no longer possible: either on account of its extreme tenacity, or on account of the patient's debility. The sputa then accumulate in the bronchi, trachea, and larynx, in succession: they fill up the air-passages, and suffocate the patient. In some instances the expectoration, in the advanced stages of the disease, consists of a fluid having the consistence of gum-water, and of a brownish red colour: like (as Andral says) liquorice-water, or plum-juice. He states that the mere occurrence of this kind of expectoration has led him to announce the existence of the third stage of pneumonia; and that the subsequent examination of the dead body has seldom failed to justify his diagnosis. Sometimes again, during the third stage, very perfect pus is excreted.

That the colour of the sputa peculiar to pneumonia depends upon an intimate union of *blood* with the altered mucus, is perfectly obvious when that colour is deep. And even when this transparent mucus is yellow, you may satisfy yourselves by the following simple experiment that the source of the colour is the same, and that the yellowness does not result, as some have fancied, from an admixture of *bile* with the matter expectorated. If to water, rendered viscid by dissolving a certain quantity of gum in it, you add blood, drop by drop, you will obtain in succession, all the shades of colour that are presented by the pneumonic sputa: first a yellow tinge; then a tawny yellow which loses itself in a red, and comes to represent the colour of the rust of iron; and lastly an intense red. The sputa may, indeed, sometimes, but I believe *that* does not often happen, be coloured by bile: but bile is not the source of the yellowness which they assume in cases of pure pneumonia.

Sputa composed of very red mucus, indicate pneumonia less surely than such as are tawny. The very red masses, in which there is more blood than mucus, often belong to pulmonary hæmorrhage.

Although these rust or orange-coloured sputa are commonly present during the more active period of pneumonia, and, as far as my experience goes, are peculiar to that disease, you ought to be aware that they do not *constantly* accompany it. Sometimes the matters expectorated are like those of catarrh: and sometimes there is scarcely any expectoration at all.

When the pneumonia passes into gangrene—which I repeat is an exceedingly rare consequence of inflammation in that organ—the expectoration becomes of a greenish, or reddish, or dirty gray colour; is more liquid, and exhales a fœtid smell, resembling that which proceeds from the gangrene of external parts. So again the puriform expectoration which at length ensues in the rare cases of circumscribed abscess of the lung from pneumonia, is horribly offensive.

I have now described, *seriatim*, the main symptoms, general and physical, which mark the existence and the progress of pneumonia. And in order to give you a just notion of each, I have spoken of them separately. But they *exist together*; and they must be *studied* together: and some will be found to confirm or to correct the indications that might be drawn from the others. I must briefly therefore run over the phenomena of the disease we have been considering, as, in most cases, it actually presents itself.

The first symptom felt is commonly pain in the side; which may or may not have been preceded by rigors. At the same time the breathing is constrained; and the patient coughs without expectorating. At this period, the ear may generally detect a slight degree of minute crepitation, which is not strong enough to mask entirely the vesicular rustle; and the stricken thorax still sounds well; and there is fever withal. This assemblage of phenomena constitutes the first period of the disease. From the second to the third day, new symptoms appear. The expectoration, hitherto absent, or merely catarrhal, becomes characteristic; being at first moderately viscid, and having a degree of colour proportioned to the variable quantity of blood which it contains. The minute crepitation increases, and drowns or supersedes the natural respiratory murmur: the clear sound produced by percussion begins to diminish on that side on which the crackling is heard and the pain is felt; and that pain is commonly less sharp than in the beginning. The dyspnœa augments, as is quite apparent from the short and frequent inspirations made by the patient. If the pain be acute, he cannot lie, on that account, on the side affected; neither can he place himself on the sound side, because in that position his respiration becomes more embarrassed; he remains, therefore, almost constantly, lying upon his back.

In this condition of pneumonia, though the disease may be severe, the inflammation is as yet in its primary stage. It often remains stationary for a while, and then recedes, and terminates by resolution. The dyspnœa diminishes, the slight dulness of sound disappears, the crackling is gradually displaced by the natural murmur of the pulmonary expansion, the sputa again become those of simple bronchitis, the fever subsides, and ceases; and all is well again.

At other times, instead of retreating towards resolution, the pneumonia becomes more intense, or rather more extensive, without passing beyond its primary stage; and the patient may die whilst it is still in that stage. But this is unusual. Ordinarily, if the inflammatory engorgement do not cease by resolution, and the symptoms that announce it are exasperated, we must expect that the second stage will be established. And we may be certain that it exists when we observe the following phenomena:—the breathing becomes more and more constrained, short, accelerated; the speech ceases to be free; the patient can do no more than pronounce a few interrupted words in a panting manner. The sputa acquire such a degree of viscidity that they can no longer be detached from the vessel by shaking it; the sound afforded by percussion, on the side affected, is decidedly dull; generally there still is crepitation, less fine in its character however than in the outset, without the admixture of any pure vesicular breathing; sometimes the crepitation ceases entirely, and either no sound at all is perceived by the ear, or, in the part where the percussion is dull, bronchial respiration is heard, and this is almost always accompanied with bronchophony. The patient continues to lie on his back.

In this degree of the disease the prognosis is always uncertain. The patient sometimes sinks rapidly, and dies from apnœa. Yet even in this degree resolution may still take place. In that case the dulness on percussion diminishes; the bronchial breathing disappears; we hear afresh a coarse kind of crepitation, at first alone, then mixed with the natural respiratory murmur, which, in its turn, becomes alone audible. The sputa return to their catarrhal character. In the meanwhile the dyspnœa and fever diminish, and then cease entirely.

It would doubtless be very interesting to determine, in a given case, whether the lung of our patient was in the second or the third stage of inflammation. But there are no certain means for making this distinction. We may *guess* that the third stage is established if the face become exceedingly pale and corpse-like; we may be more confident of it if the prunejuice expectoration, or if puriform expectoration, should occur; and our presumption will be strengthened if the disease have existed for a certain *time*. However, this last circumstance will not help us *much*; for sometimes the lung has been found to be in a state of suppuration on the fifth day of the disease, and sometimes it has been found still in a state of red hepatization after fifteen or twenty days.

I told you just now that sometimes other symptoms, irrespective of those revealed by auscultation, may help to solve this anxious question.

It is a curious and very interesting fact that the chloride of sodium—common salt—which is always present in the urine of health, diminishes in quantity during the first advance of pneumonia, and ultimately disappears from the urine upon the occurrence of hepatization of the lung, but begins to reappear when the absorption of the solidifying material is about to commence. You will at once perceive the diagnostic and prognostic value of this remarkable fact. The fact itself may be ascertained by a rough and easy test, which, though open to an occasional source of fallacy, is sufficiently trustworthy for all practical purposes. Acidulate with a drop of nitric acid a small portion of the patient's urine collected in a test-tube. Then add a few drops of a solution of the nitrate of silver, and a white cloud of precipitate will form, if there be ever so little of the chloride of sodium in the urine. This would show that the inflammation was receding.

The use thus made of the test in question is not affected by the circumstance that there are other forms of disease in which the chlorides vanish from the urine. I would refer you for a complete examination of this subject to a paper by Professor Beale in the thirty-fifth volume of the 'Medico-Chirurgical Transactions.'

Again, there are reasons for thinking that mere uncomplicated pneumonia, instead of being the starting-point of the whole disturbance of the health, may spring from some previous contamination of the blood, arising, perhaps, from a check to the action of the skin through exposure to cold. Certain it is that for a day or two before the shivering which is considered to be initiatory of the local inflammation, careful observation may often discover that the patient is unwell, has lost his appetite, is languid, restless, out of sorts, and suffering that sort of *malaise* which not seldom precedes and ushers in the local signs of inflammation. Immediately after, and even during, the shivering fit, the temperature of the body rises considerably. Now it is stated by Dr. Johnson, in one of his printed lectures, that frequently in his experience, the high temperature has abruptly and rapidly subsided upon the occurrence of exudation in the inflamed lung. He likens this to the breaking-out of the eruption on the skin in small-pox, which often brings with it great relief to the pre-existing feverish distress. When therefore this fall in the bodily temperature is noticed concurrently with the manifestation of the physical signs proper to the solidification of the lung in pneumonia, we may reasonably conclude that the inflammation has not yet passed, and is not likely to pass, into the less hopeful condition of gray hepatization.

Whether, when the lung has reached the third stage, it is still susceptible of repair, is a question which no one can answer. We have not the materials for its solution, inasmuch as we have no sure sign of the existence of this third stage during life. I should *think* that recovery from diffused suppuration of the lung is not possible. The rarer form of circumscribed abscess certainly is not of necessity fatal.

The disciples of Hahnemann have enabled us to form a tolerably correct opinion as to the *natural duration* of uncomplicated pneumonia. A French physician, Dr. Bourgeois, after abstaining for twenty-five years from all active therapeutic treatment in pneumonia, affirms that in cases which terminate favourably, a marked tendency is manifest to a recession of the disease on the eighth day; the sputa become less tawny and less viscid, respiration is easier, there is no longer pain in the side, the tongue cleans, and natural sleep returns. On the ninth day the appetite is restored, the cough is looser, and the urine becomes again plentiful: in a word, the *symptoms* of pneumonia disappear, while the *physical signs remain in their plenitude.* On the tenth day the patient enters upon complete convalescence; at the end of a fortnight he is able to resume his usual avocation, if it be not of a very fatiguing character. Nevertheless there still remain, often for long, auscultatory traces of the extinct or retreating disease.

I have very little to add to what I have stated already of the morbid anatomy of pneumonia. Of the changes which the *lung itself* undergoes you are now I hope fully apprised. The pleurisy which often attends the disease is seldom accompanied by much liquid effusion; indeed, when the whole of one lung is solidified by inflammation, it fills the cavity of the pleura, and *prevents* much effusion. The heart is found to be in that condition which I formerly described to you, as being both a consequence and an index of death by apnœa. Its right cavities especially are full of black coagulated blood; and a remarkable degree of venous congestion is frequently met with in the liver, spleen, and intestines. The amount of this varies according as the process of dissolution—what the French call the *agony*—has been more or less protracted, and the breathing more or less difficult.

Neither need I enter upon any formal discussion of the *causes* of pneumonia. Sometimes *no* cause can be traced; often the disease is clearly the consequence of exposure to cold, especially under those circumstances which were formerly described as aiding the injurious operation of cold upon the human body. Why, in one person, such exposure causes peritonitis, in another pleurisy, and in a third inflammation of the substance of the lungs, we can give no satisfactory account.

It remains, then, only that I should speak, first of the *prognosis*, and secondly of the *treatment*, of pneumonia; and of the first of these matters—of the prognosis—I have already, incidentally, told you nearly all that is made out, or worth knowing. It is almost superfluous to say that the first degree of the disease is less dangerous than the second, and the second than the third. There is no doubt that pulmonary inflammation may still undergo resolution, although a great part of one lung should be hepatized; but there are no facts which prove—indeed there is no possibility of proving—that the lung may recover from the state of purulent infiltration—the third degree.

Something will depend upon the *extent* of the inflammation; I mean that pneumonia in the first degree, and of great extent, is generally as serious as pneumonia in the second degree, but much more circumscribed. Inflammation of the upper lobes is also more perilous than inflammation, to the same extent and degree, of the lower.

Of the *general* symptoms, those which we learn independently of auscultation, the *respiration*, as a prognostic sign, is the most important. Laboured breathing, and shallow and frequent breathing, are both of them symptoms of bad omen. We get less help from the state of the *pulse*. If, however, a feeble pulse go along with great difficulty of breathing, and if it do not develop itself under the abstraction of blood, we must conclude that the case is a serious one, and deduce an unfavourable prognosis. The supervention of *delirium* is also a discouraging circumstance. You will have inferred already the information which may be gleaned from the character of the expectoration, in respect of the probable issue of the disease. Great viscidity of the sputa, and a deep rusty colour, announce continued intensity of inflammation: their return to the catarrhal condition indicates that resolution is going on. Watery and brownish sputa, more or less like plum-juice, should induce us to suspect suppuration of the lung, and are therefore of evil augury.

To dictate the treatment of pneumonia is not an easy task. It may sound like a paradox, but concerning this disease I believe it to be true, that the very perfection of modern diagnosis has helped to bring uncertainty and vacillation into our practice. Inflammations of the lung, which might escape all other modes of investigation, reveal themselves infallibly to the ear. By the same sense we learn, as surely, that many of these otherwise latent inflammations run their course without any great commotion of the general system, whether they kill, or whether they pass gradually away. These forms of pneumonia neither require, nor would they endure, nor have they had addressed to them, so far as I am aware, the active measures which, prior to the use of auscultation, were enjoined as proper in unmixed inflammations of the lungs. On the contrary, the current has set (too strongly I conceive) in the opposite direction. A most distinguished French author, M. Louis, endeavoured to show that venæsection had not much control over the progress or the issue of pneumonia in any of its forms: and in our own country that doctrine has been adopted by at least one very accomplished physician—adopted and extended, for Dr. Hughes Bennett maintains that antiphlogistic remedies in general, and blood-letting in particular, are unsuitable, and even hurtful, in all acute inflammations. I believe that I might ascribe similar opinions to physicians and surgeons of eminence in this town.

I must here make a short digression.

Great praise is given, and justly, to the observance of what is called the *numerical method*, in the cultivation of medicine as a science. Vital statistics are indeed of infinite value to us for many important purposes: but I wish to caution you against the error of putting absolute trust in them for other purposes, for which they are not adapted, for which they are worthless and likely to mislead, especially when applied to medicine as an art. Even when the best ultimate result is the attainment of a high degree of probability, I would have you substitute, *whenever you can*, actual numbers for such vague phrases as 'sometimes,' 'generally,' 'occasionally,' 'for the most part,' and the like: phrases which, besides being open to objection for their inexactness where exactness is of moment, and more or less obtainable, are apt to vary even with the varying temperament of those who use them. 'The *sometimes* of the cautious (says Professor Guy) is the *often* of the sanguine, the *always* of the empiric, the *never* of the sceptic: but the numbers 1, 10, 100, 1000 have but one meaning for all mankind.'

Now, without going into detail, I may say that statistics are available, conclusive, and most valuable for discovering the *causes* of disease—the length of the incubation of contagious disorders—their duration, their fatality; the comparative salubrity of different places and climates. Again, to take an example from forensic medicine, we ascertain by statistical enquiries the earliest and the latest periods at which a poison begins to give evidence of its operation. In social matters, the mean duration of human life, and therefore the probable period of death, are estimated by statistics with sufficient accuracy to settle the scale of payments for the insurance of lives. In all cases, the larger the number of facts noted, the greater will be the probability reached, or the approach to absolute certainty: but the facts noted must in all cases be strictly *identical.*

For this last reason, statistics may mislead when they are relied upon as guides in the treatment of actual disease; for here the requisite identity fails to obtain. Some *general* guidance, no doubt, may thus be gathered; but to be governed by statistical results in the management of separate cases of disease, is to adopt the irrational and dangerous rule of prescribing according to the *name* of a disorder; whereas each individual case requires its special study, speaks its proper language, furnishes its peculiar indications, and reads its own lesson. There can be no *average* treatment of a nominal disease.

I remember being present when a schoolboy, during a war panic, at the drill exercise of a raw company of militia, and amused by the *bad fit* of their regimentals, which had apparently been contracted for wholesale, and were all of the same *average* size. Here stood a short recruit, with his coat-tail dangling nearly on a level with his knees, next him a tall one whose coat-waist rose almost to between his shoulders. Possibly there was not in the squad one man of the exact average size, and therefore not one whose dress fitted him accurately. Just as inapplicable must all average treatment be to individual cases of multiform disease.

Now—to return to our subject—the very eminent physician already mentioned, Dr. Hughes Bennett, the Professor of the Institutes of Medicine in the University of Edinburgh, has appealed strongly in support of his treatment of pneumonia to the evidence of statistics. But his appeal does not convince me that every case of pneumonia, as ascertained and measured by its auscultatory and its general symptoms, ought to be treated in precisely the same manner. All that his exhibition of statistics proves is, first, that blood-letting is very rarely requisite, and, secondly, that the patient needs to be well nourished; and these points are indeed *much* to have established. But there are minor points which we cannot wisely neglect; nor does Dr. Bennett himself appear in practice to have neglected them. The great majority of cases of pneumonia are, by common consent, treated without any abstraction of blood; and very much perhaps as Dr. Bennett might treat them. Some, in my judgment, are properly treated by stimulants even; some by opium, or by chloral; some with mere 'expectation.' To use or to withhold a remedy simply because it is found by numerical calculation that, in cases nominally the same, recoveries have been more frequent when that remedy was employed on the one hand or omitted on the other, would be to sacrifice the plain and perhaps pressing indications of a particular case, to the statistical average of diseases having a common denomination. To repeat what I have said before—we do not necessarily take the same symptoms as indications of treatment, which we trust to as signs of disease. We treat, indeed, not the so-called disease, but its accidents; the vital manifestations which proclaim its character and intensity, foreshow its tendencies, and illustrate its course. For directing the management of individual cases, it is far more profitable (as some one has well expressed it) to *watch* than to *count*.

For myself, I must say that I have never been a partisan of extreme views on this and cognate matters—have neither advocated copious blood-letting on the one hand, nor the large and indiscriminate use of brandy (which has been equally applauded and practised) on the other. My own experience has taught me that the more sthenic and flagrant form of uncomplicated pneumonia is very rare among us. Years have passed by since

I have met with any instance of that disease which has required phlebotomy. In this special case, not only must you decline to be guided by the mere name of the disorder, you must not be guided even by the *thing*, *pneumonia itself*, as disclosed by the evidence of auscultation. The general or constitutional symptoms must direct the treatment, while the local signs identify the disease. The question must always be, what detail of treatment will soonest bring this particular case to its best possible issue.

When, with the physical evidence of pulmonary inflammation, you find your patient breathing with extreme labour and difficulty, and you notice at the same time the tokens of enormous venous congestion, the veins of the head and neck turgid with dark blood, while the pulse is very feeble as well as frequent, you may conclude that the right side of his heart is so distended with blood as to be unable to contract, and you must take blood by venæsection, with the chance of so saving your patient's life, and with the certainty of prolonging it, and of giving immediate relief to his exceeding anguish. But I do not advise you to push the bleeding beyond the advent of this manifest relief. I believe this to be the only accident of pneumonia that requires or warrants a recourse to general bloodletting.

Again, if at the outset of the illness there is sharp pain in the side, announcing the presence of *pleuro*-pneumonia, you will do well, especially if the patient's health have been previously sound, to apply cupping-glasses, or leeches, over the painful spot: and you need have no dread, in my opinion, of ultimately damaging him by this local diversion and removal of blood.

Diaphoretic medicines are, in all cases, proper and serviceable: and in the severer forms of pneumonia I would recommend you to adopt the treatment by *antimony*, as directed by the Italian physician Rasori; but I would limit and regulate that treatment in accordance with the rules laid down by Dr. William Gairdner in his instructive volume on 'Clinical Medicine.' They are simply these: to give the antimonium tartaratum in doses varying from one-twentieth of a grain to one grain, every hour, withdrawing or suspending the remedy as soon as it produces any distressing effect on the patient—continued vomiting, purging, or great general depression; withdrawing it also as soon as the fever appears to have received a decided check. I said *continued* vomiting, because a single act of vomiting may be beneficial. After that the stomach is generally found to *tolerate* the drug, as our Continental brethren express it.

I should add that this antimonial treatment is chiefly useful and fit for the very early stage of the disease, when the skin is hot and dry, and the expectoration scanty and viscid.

It will be right, in the commencement of the disease, to administer, for once, a mercurial purgative—two or three grains of calomel, for instance, with ten or twelve of rhubarb. But multiplied experience has at length wrought a widespread belief that to give mercury in this disorder, with the view of exciting ptyalism, is not commendable practice.

Among what may be called the routine remedies of pneumonia, we must rank counter-irritation by means of blisters; but they are often applied to the chest much too early in such cases. In the outset, while there is yet considerable fever present, they add to the irritation, and distress the patient; and tend to aggravate the existing inflammation. But when the fever is no longer high, and the skin no longer burning, though the expectoration is still difficult, the dyspnœa considerable, and a sensation of pain, or tightness, or oppression, is experienced in the chest, then a large blister is often productive of very sensible benefit; but it should *be* a large one. The patient should have a waistcoat almost, or at any rate a breast-plate, of blistering-plaster. I have never seen such good effects from placing blisters upon distant parts in this disease, upon the thighs or arms for instance, as would lead me to plague the patient with them in those situations.

What I have said of blisters applies to the other methods of stimulating the surface. Take a case in illustration. A worthy physician, a friend of mine, suffering in his own person an attack of pleuro-pneumonia, was persuaded, against his better judgment, to lay a mustard-poultice to his painful side. The pain, as soon as the mustard began to tell, was increased to a pitch that was scarcely endurable. He had the mustard removed, and the part (after it had been washed) covered with leeches; with the speedy effect of bringing ease to his suffering, as decided as had been its previous aggravation. The proper appliance under such circumstances is a soft and warm *linseed*-poultice.

This, then, is the kind of treatment which is in my judgment suitable for acute idiopathic pneumonia. Different cases will require different modifications of it; for which, I repeat, no particular rules can be laid down.

All that I have hitherto been saying relates to *acute* pneumonia, occurring in a previously healthy person. But pneumonia, having that character, and so occurring, is (I repeat) a much less common disorder than most persons appear to suppose, or than I formerly thought it to be. I have been surprised to find how few cases of pure idiopathic inflammation of the lungs present themselves among my hospital patients. Five or six in the year are as many as I see there. Intercurrent pneumonia, however—pneumonia engrafted upon some other pre-existing disease—is abundantly frequent; and requires, in general, a much more guarded plan of treatment. Inflammation of the pulmonary substance is apt to supervene insidiously upon various disorders which are of every-day occurrence: upon bronchitis; upon phthisis; upon disease of the heart; and upon fevers, especially the exanthematous fevers. In these cases, while the physical signs are necessarily the same as in the unmixed acute disease, the general symptoms are often but slightly pronounced. During the progress of continued fever of a low type, inflammation may steal upon the lung, and run quickly through all its stages, and spoil the organ irrecoverably, without giving any notice of its presence: unless, indeed, you suspect it, and search for it with your ear. The pneumonia is said, in such cases, to be *latent*. It seldom needs, the associated disorder would seldom bear, any active depletion. Much benefit often follows the abstraction of small quantities of blood, but they should be taken from the surface of the chest by the cupping-glass, and not by the lancet from the arm; and it is good practice thus to aim at reducing the local mischief with one hand, while with the other we support the patient's strength by means of ammonia, wine, and nourishing broths. Blisters are also of service: more so than in the sthenic forms of pure pneumonia; and they may be applied at an earlier period.

When the convalescence from acute pneumonia is decided, and real, it is shorter than might have been supposed. From the period when the pulmonary inflammation is fairly over, the strength returns with unexpected facility, even though large bleedings may have been practised and repeated. But we have to guard, more perhaps in this disease than in most others, against false or merely apparent convalescences. A patient can never be pronounced perfectly secure so long as any trace of crepitation remains in the affected lung, and this may often continue long: nay, it not unfrequently ceases only upon the supervention of another more surely fatal though less rapid disorder—viz.

pulmonary consumption ; of which, however, I must treat as a distinct disease.

Before I take leave of the subject of pneumonia, I must again mention, however briefly, an unmanageable variety of pulmonary inflammation, which has been called *lobular pneumonia* ; the consequence of pyæmia, or of the absorption of septic material from a wound, or from the interior of the uterus after parturition. The absorbed matters get entangled in the capillary blood-vessels of both lungs, where they frequently provoke inflammation, resulting in the formation of small scattered abscesses.

The physical signs of this condition are few and doubtful. There is no dulness under percussion. If the inflamed lobules lie near the surface, moist sounds may be audible, large crepitation, and even gurgling ; and puriform expectoration may occur when the lung-tissue at length breaks down.

This disorganising process is accompanied by a low form of fever, frequent shiverings, a very rapid and feeble pulse, profuse perspirations, and early death by asthenia.

In the next lecture I shall speak of pleurisy.

LECTURE XLVI.

Pleurisy. Its anatomical characters; false membranes; liquid effusion; effects of these upon the shape and contents of the chest, and upon its healthy sounds. Symptoms of Pleurisy.

I PROCEED this afternoon to the subject of *pleurisy*; having in the last lecture concluded what I had to say on that of pneumonia: that is, I pass from inflammation of the *substance* of the lung to inflammation of its *investing membrane.* The two frequently exist together: but when that is the case, the one, in general, predominates greatly over the other. Pleurisy, however, without pneumonia, is much more common than pneumonia without pleurisy. I told you that when both are present, but the pneumonia forms the main disease, the term *pleuro-pneumonia* is applied to the compound malady. The whole interest of such a case merges in the pneumonic inflammation. Again, when both are present, but the pleurisy predominates, the compound affection is sometimes called *pneumo-pleuritis.*

The pleura, as you know, is one of the serous membranes. Its inflammation is attended therefore with those *events* which I formerly took some pains to describe as belonging especially to that particular tissue. The inflammation is of the adhesive kind: it is accompanied by pain; by the pouring out of serum, of coagulable lymph, of pus, or of blood. I think it will be best, in this instance also, to lay before you some account of the morbid anatomy of the disease, before I consider its symptoms.

The alterations that take place in the inflamed membrane itself are not very striking or important. Experiments upon living animals, made by introducing some foreign substance, or injecting some slightly irritating liquid, into the cavity of the pleura, have proved that, as in other cases, inflammation is attended with *redness* of the part affected. But it is scarcely ever that we observe this effect *alone* of inflammation, in the pleura of a dead person; unless, indeed, he has died of some other complaint while he happened to have *incipient* pleurisy. The pleura has been said to be thickened by inflammation; but that I apprehend to be a mistake. It often *appears* to be thickened, in consequence of the superposition of a false membrane—a layer, or several layers, of plastic lymph. But actual thickening of the pleura itself seldom or never happens. Neither does the pleura easily soften, or readily ulcerate, under inflammation. It peels off, in some cases, from the lung, or from the ribs, with more facility than in the sound state.

The most remarkable effects of pleurisy result from the effusion of coagulable lymph, or of serous liquid, or of both, into a shut sac, having peculiar anatomical relations. One part of the membrane lines the firm walls of the chest: the other part envelopes the soft and compressible lung. The opposed surfaces of this closed and empty bag being *ap*posed also, but freely movable one upon the other, very different, and even contrary, effects may be produced by its inflammation. The pulmonary pleura may be glued to the costal pleura, so as to prevent all lateral movement between them, and to obliterate the pleural cavity: or the two surfaces of the membrane which are naturally in contact, may be forced unnaturally apart by a pouring forth of serum between them: or the opposite surfaces of the pleuræ may be united by coagulable lymph in some places, and separated by effused fluid in others. And great differences will arise in the symptoms, and in the gravity and tendency of the complaint, according as one or another of these different conditions of the contents of the thorax is established.

Let us first consider the effect of the throwing out of coagulable lymph only; or, of what comes to the same thing, the effusion of coagulable lymph with a certain quantity of serum, which last is soon reabsorbed.

One consequence of this is the formation of *false membranes.* These, indeed, are formed whether there be much or little serum poured out. We continually meet with them, sometimes when we least expect to do so, in the dead body. They vary greatly, in different cases, in respect of their thickness, situation, extent, organization, and effects.

When the lymph is first deposited upon the free surface of the inflamed pleura, it is soft, and of a grayish white colour, like paste somewhat. It soon, however, acquires an increase of consistence, and shows marks of vitality; becomes, in short, organized. Red points begin to appear in it, few in number and widely separated at first; but they presently multiply, and lengthen into reddish streaks, which run along the surface of the effused matter. Soon these red streaks may be perceived to be slender vascular canals; and at length they inosculate with the vessels of the pleura, and the lymph, converted into a false membrane, becomes a constituent part of the living frame.

It is curious, and useful too, to know how rapidly this work of organization may go on.

Andral made experiments upon the pleuræ of rabbits, by injecting acetic acid into them. He sometimes found, at the end of nineteen hours, soft and thin false membranes, traversed by numerous anastomosing red lines. In other rabbits, placed under circumstances which appeared to be exactly similar, no such result had taken place at the end of a much longer period; but the pleura contained only a serous or puriform liquid, mixed with unorganized flakes of lymph. Now similar differences have been remarked in the human subject, under disease. False membranes, already vascular, have been found in the bodies of persons who died of pleurisy after a very few days' illness: while in other patients, who had lived for many months after the invasion of the disease, there has been no trace of such vascular membranes. It is clear, therefore, that the organization of the lymph does not depend solely upon the length of time that has elapsed from the period at which it was poured forth. It has much more to do with the previous state and habit of the patient. *Cæteris paribus*, plastic lymph and early adhesion are more to be expected in young, strong, and healthy persons; curdy unorganized lymph, granular deposits, with copious and abiding serous effusion tending to become puriform, in such as are old, feeble, cachectic, or scrofulous.

The extent of these false membranes varies, according to the extent of the inflammation which has produced them. When that has been general, they cover the whole lung, and line the whole costal surface, and spread themselves over the diaphragm and mediastinum of the same side. Supposing that there is no serous liquid effused, or that it is absorbed, the lung then becomes everywhere adherent to the sides of the cavity which contains it. The medium of adhesion, which is soft and tender while it is recent, grows firm, and assumes the characters of areolar tissue, when the union is of old standing.

The thickness of the false membranes is also extremely variable. Sometimes it is not more than that of the pleura itself, and the lymph might then, in the absence of adhesion, be almost overlooked. But in the majority of cases their thickness is much greater than this. Frequently several distinct layers or strata are seen, superposed one upon another, to a considerable depth.

Are there any auscultatory signs of this process of adhesion, when it occurs? Yes. There is a morbid sound, not hitherto mentioned by me, whereby it is sometimes disclosed: the sound, namely, of *friction*; the sound produced by the rubbing together of the dry, or inflamed and roughened surfaces. You doubtless are aware that every time a tolerably deep inspiration takes place, the relation between the ribs and the lung undergoes a change. While the ribs are being elevated, the lung descends a little: and consequently any given point of the surface of the lung no longer touches the same point of the thoracic wall as it did before the elevation. You may convince yourselves of this fact by carefully making a small incision through an intercostal space, in a living animal. Now the pulmonary pleura, when that membrane is inflamed, does not slip and glide over the costal pleura in its usual smooth and noiseless manner; but it makes a creaking or rubbing sound, which the ear, applied to the corresponding surface of the chest, readily catches. I have very many times heard this; yet it is not at all a familiar sound: indeed I had heard it, in one instance, some time before I knew what the noise meant. The sound has, mostly, an interrupted character, occurring in a series of three or four jerks. The patient is often made aware of the harsh movement, by some internal sensation; and a bystander, who places his hand flat upon the corresponding part of the thorax, may sometimes feel this grating of the membrane upon itself. You may wonder, as adhesions are so common, that this sound, and these sensations, are not oftener heard, and felt. In truth, they are transitory phenomena, and cease, of necessity, so soon as adhesion prevents any further motion of the opposed pleuræ upon each other. If we do not happen to listen during that period, usually a short one, in which the pleuræ, roughened by inflammation and effused lymph, but not separated by liquid, still chafe against each other, we lose the opportunity of hearing the sound at all. This *rubbing* sound, this noise of *friction*, we shall find to be of even greater importance in relation to certain diseases of the heart, than in cases of acute pleurisy. In pleurisy the *liquid* matters poured into the membranous sac have far more interesting consequences: and to these I now ask your attention.

In some instances we find, after death, a clear, serous, or watery fluid, without colour, or of a pale lemon-colour, and perfectly limpid and transparent. This may occur, independently of inflammation of the pleura, from some mechanical obstacle to the circulation. It then constitutes a species of dropsy; a true *hydrothorax*: and this, though less common than ascites, is by no means an *un*common consequence of disease of the heart. When the effusion does not proceed from a cause of that kind, it is almost always the result of inflammation of the pleura itself, although we may find only a slight degree of redness upon its surface, or a few patches of coagulable lymph. More frequently, besides this clear liquid, with flakes of albuminous matter floating in it, there is also a coating of lymph on the inflamed membrane. Very often the thinner fluid is turbid, or whitish, like whey; sometimes it is distinctly puriform; sometimes it is tinged more or less deeply with blood; sometimes it consists of nothing else but blood, which has separated into serum and crassamentum. There being no wound, nor visible rupture, of large or of smaller vessels, we conclude, in such cases, that the blood has exuded from the membrane by what I have previously spoken of as capillary hæmorrhage.

The different kinds of fluid effused into the pleuræ are always, or almost always, without smell;—provided that it has remained a closed bag: I mean when no communication has been established between the cavity of the pleura and the external air, either through an opening in the walls of the chest, or through a pulmonary fistula leading to the trachea, or through some breach in the œsophagus. I have met with but one exception, and that a doubtful one, to this rule. A patient died in the hospital, who, some years before, had nearly killed himself by swallowing, in mistake for beer, a solution of caustic potass. The result of this had been ulceration, and subsequently stricture, of the gullet. His left pleura was perfectly full of most stinking pus; and we were unable to detect any channel of communication with the outward air, although the circumstances of the case rendered it not improbable that such a channel might have existed.

Sometimes air, or gas, is found in the cavity of the inflamed pleura; either alone, or (what is much more common) together with a liquid. We ascertain this fact, in the dead body, by the hissing sound that takes place as soon as a penetrating incision is made between the ribs: or by opening the thorax under water, and noticing the escape of air in the form of bubbles. It is probable that these gases are sometimes secreted or exhaled from the diseased membrane; sometimes they are the product of decomposition within the cavity; but, for the most part, they are admitted from without, the sac of the pleura communicating somehow with the external air.

Such being the fluid matters that frequently occupy the cavity of the pleura when that membrane has undergone inflammation, let us next examine the necessary effects of their being collected in that part. These effects will obviously vary considerably according to the quantity of the fluid that accumulates.

Now the quantity of fluid may vary from less than an ounce to several pints. At first it is lodged in the cavity of the pleura solely at the expense of the yielding lung, which is compressed to make room for it. But if the quantity continue to augment, other parts are at length displaced by the increasing pressure, the boundaries of the chest on that side are stretched, and even the abdominal viscera are thrust out of their natural position. The lung is pushed back towards the mediastinum and vertebral column, and flattened and brought to lie in the smallest possible compass; the diaphragm is forced downwards, which sometimes gives rise to a considerable prominence of one or the other hypochondrium, the spleen and stomach being displaced on the left side, or the liver on the right. The ribs are separated too; the intercostal spaces become wider, and are pushed out to the level of the bones, and the whole of the affected side is smooth and obviously larger than the other. The mediastinum also undergoes some change of position, being driven more or less towards the side opposite to that on which the effusion exists. If the liquid happen to fill and distend the left side of the thorax, the heart may be moved out of its natural place, and be heard, and felt, and seen to beat on the right of the sternum. Andral mentions having met with only one instance of that kind. I suppose that I cannot have witnessed less than a score such. So again the heart may be carried beyond its proper place, to the left, by a large effusion into the right pleural cavity.

I say when the liquid is accumulated in very considerable quantity, the lung is pressed into the form of a thin cake, which occupies a very small space alongside the vertebral column: and if it happen to be covered over and concealed, as it often is, by a strong layer of adventitious membrane, we might fancy, at first examination, that it had completely disappeared. It was in cases of this kind—especially when the effused fluid consisted of pus—that the lung was erroneously represented by the older observers as having been *destroyed* by suppuration. However, you will always find the lung there if you take the pains to look for it, and to divide the false membranes that bind it down: and, in many instances, it is sound also. Its surface may, indeed, be wrinkled, but the lung itself may be capable of being restored to nearly its former volume by insufflation, as it is called; by blowing air into it through the principal bronchus of that side. In this compressed state the lung does not crepitate under the finger; it is dense, and sinks in water; in fact it is wholly void of air, and has been brought, by the pressure of the fluid around and upon it, into nearly the condition of the lung of the fœtus that has never breathed. But its firmness, its resistance to being torn, and its capability of being again inflated, prevent our confounding it with hepatized lung. Sometimes its cellular texture is obliterated; the opposite surfaces of the vesicles and smaller air-tubes adhere together; the lung will not admit air; it looks like a piece of muscle, and is then said to be *carnified.*

Such is a general account of the anatomical characters of pleurisy, as they are disclosed to us by an examination of the body after death. We may now inquire what effect these changes are capable of producing on the *sounds* which are heard when the healthy chest is percussed, or listened at. We shall then be the better prepared to appreciate the several symptoms, general and physical, which are known actually to occur in pleurisy. Now it is clear that when the lung is pushed away from the walls of the thorax by fluid between the pleuræ, it will be compressed also; its capacity must be reduced; less air will be able to enter it. There will consequently be a proportional diminution in the intensity of the respiratory murmur; and this murmur will, moreover, be less audible in consequence of the distance, from the ear, of the structure in which it takes place. The lung is attached by its roots (so anatomists speak) to the spinal column. A moderate amount of effusion will, therefore, cause it to recede upwards and inwards: and a certain quantity of the liquid will ascend between the lung and the ribs, compressing the spongy pulmonary tissue around the larger and more resisting bronchial tubes. We might expect, in this condition of things, that the passing breath, and the voice, would be audible in those tubes, through the partially condensed lung, and through the circumfused layer of liquid: and it is so. We do hear bronchial respiration, and bronchial voice and cough; with some modification, indeed, to be noticed presently. In this respect, therefore, you will observe that pneumonia, which solidifies the spongy texture of the lung around the bronchial tubes by filling it with lymph or with blood, has the same effect, so far as acoustic principles and results are concerned, as pleurisy, which solidifies a portion of the lung by expressing air from it, and pours round the bronchial tubes a fluid which readily transmits sound. Hence bronchial respiration and bronchophony are not always indicative of the same condition of parts within the chest, but derive their true value and meaning from the context, if I may so say; from the circumstances under which they occur, and with which they are associated.

When the effusion is so copious as to squeeze all the air out of the spongy part of the lung, to pack the organ up along the vertebral column, to distend the thorax, and to compress strongly the bronchial tubes themselves, *no* respiratory murmur *can* then be heard, nor any tubular breathing; for the dilated chest can neither expand nor collapse, and, therefore, no air can pass along even the larger air tubes; neither can these compressed tubes vibrate with the patient's voice; wherefore bronchophony also ceases, or is but faintly audible.

Again, if percussion be made over a portion of the chest, beneath which, instead of healthy and spongy lung, there is incompressible serous fluid, a dull flat sound will be rendered. But a dull sound is rendered also when percussion is made over a solidified lung. Hence the mere dulness of the part struck does not inform us whether we have pneumonia or pleurisy to deal with, or some other disease that has had the effect of making the lung solid, without plugging up the larger bronchi.

But an expedient presents itself, by which we may in some cases render this experiment of percussion conclusive. The dull sound occasioned by hepatization or other solidification of the lung occupies the same spot in every posture of the patient. Not so, necessarily, the dull sound produced by the presence of liquid in the cavity of the pleura. The liquid will gravitate to the lowest part of the cavity, and will carry with it the dull sound. We place the patient, therefore, in different attitudes; and if we find the chest, when struck, resonant always in the higher, and dull always in the lower portions of the thorax, whatever its position may be, then we may be sure that the cavity of the pleura contains liquid. In such a case, when the patient sits up, the dull sound will be elicited from the lower part of the chest, on one side, from the spine round to the sternum. When he lies on his back, the anterior of the thorax sounds hollow; the posterior dull: and when he reverses that posture, and lies with his face downwards, these sounds change places also: the hollow sound is still uppermost, in the posterior part of the cavity; the dull sound still undermost, in its anterior portion.

There are just two states which may interfere with the true interpretation of the sounds produced by percussion in the manner now described; and these are, first, partial adhesions of the pleuræ, which may confine and isolate the effused liquid, and prevent its sinking from one part of the chest to another under the influence of gravity: and, secondly, so large an amount of effusion as to fill entirely the cavity and fix the compressed and empty lung in one position; for it is necessary, in order to obtain the shifting sounds in different attitudes of the body, that there should be light spongy lung to ascend, as well as heavier fluid to sink down, according to the posture of the patient. In this last case, that of excessive effusion, the whole surface of the affected side will yield a dull sound. It is seldom so in pneumonia: it is seldom that the entire lung on one side is so blocked up, in consequence of inflammation, as to give rise to universal dulness on percussion. But the diagnosis of these two conditions is an important one, and apt to puzzle a student. I hope to elucidate it as we go on.

We may now consider, with a better chance of understanding some of them, the *symptoms* which are generally met with in a case of pleurisy under its ordinary form and progress. The general tokens, then, of that complaint are rigors, pain in the chest, dyspnœa, cough, difficulty or impossibility of assuming certain postures, and fever. Very much the same, therefore (as Cullen truly stated), with those of pneumonia, and, it may be added, with those of pericarditis: but auscultation differentiates these diseases. The physical signs I will examine presently. The general symptoms will bear, each of them, a short comment.

The *pain* which the patient feels—or the *stitch* in the side, as it is expressively called—is one of the most striking and characteristic symptoms of the disease. *Point de côté* the French name it. It occupies a point or spot; and patients feel as if some sharp stabbing instrument were driven in at that spot every time that the act of inspiration goes beyond a certain limit. The Latin medical writers, attending chiefly to this prominent symptom, call pleurisy 'morbus lateris.'

This pleuritic stitch is subject to considerable variety in regard to its situation, its severity, and its duration. Most commonly it is felt on a level with or just beneath one or other of the breasts, in the part corresponding with the lateral attachments of the diaphragm: and this, even when the inflammation which occasions it is of much greater extent. Why is this? What is the cause of the pain? Wherefore should it be restricted to one small spot, when the inflammation pervades, it may be, the whole of the pleura? Pathologists have made attempts to explain these matters; but perhaps their explanations are not very much to be trusted to. They say that there is a larger amount of motion, at the lower part of the thorax, of the pulmonary over the costal pleura; and that the pain resulting from that friction, when the membranes are inflamed, is therefore felt where the friction is the greatest. However, the pain is not always confined to that spot. It is occasionally felt in other places, as in the shoulder; in the hollow of the axilla; beneath the clavicle; along the sternum; and sometimes it is complained of as extending over the whole of one side of the thorax. Andral states that he has observed the pain to prevail especially along the cartilaginous border of the false ribs, when the inflammation has attacked that portion of the pleura which covers the upper surface of the diaphragm. He says, too, that in such cases the pain often affects the hypochondrium, and even extends as far as the flank, so that it might be mistaken for a symptom of abdominal inflammation. This observation is worth remembering. Sharp pain, occupying the right hypochondrium, belongs oftener to the pleura than to the peritoneum. I have known several instances in which such pain was erroneously ascribed to hepatitis, when in truth it resulted from inflammation of the pleura. Cruveilhier observes, also, that he has known the pain to affect the loins, and to simulate lumbago.

Whatever may be the situation of the pleuritic pain, it is generally increased by percussion, by intercostal pressure, by lying on the affected side, by a deep inspiration, by cough, and by different movements of the body.

In many patients the pain is exceedingly sharp, whether it be continued, or whether it occur only at intervals: the more *circumscribed* it is, generally the more *acute* it is. The patients are then in a state of great anxiety: they make very short and imperfect inspirations, through fear of aggravating the pain; they dread the least effort of coughing, or of sneezing, and suppress the desire to cough which the disease may occasion. There are other patients in whom the pain is moderate, is felt only when a *deep* inspiration is made, and is scarcely augmented by pressure or by percussion. And there are even some cases of pleurisy which are unattended with pain from first to last.

The pain commonly exists from the very outset of the pleurisy. It is sometimes vague and fugitive at first, and becomes fixed and permanent after a day or two. In that case it may be mistaken for simple rheumatic pain; for muscular soreness; for pleurodyne, or for what is thought to be merely a nervous pain. When the pain is increased by slight pressure made *upon* the ribs as well as between them; when it extends over a large space; when it is unattended with fever; when it is inconstant or fugitive—we may *suspect* that it is situated in the fibrous and muscular tissues; but these circumstances do not afford any *certainty* that such is the case. In fact I have long been of opinion that some at least of the cases which pass under the name of pleurodyne, are really instances of what has been called *dry* pleurisy. You are aware perhaps that adhesions are very constantly found to exist between the lungs and the ribs in persons dead of pulmonary consumption. Such persons are liable to pains in the chest, beneath the clavicles, in the axillæ, between the shoulders, at the upper part of the dorsal region; in short, in those situations where, after death, the adhesions are found most frequently and in the greatest number. The pains indicated, it may be presumed, the periods at which the slighter forms of circumscribed pleurisy, attended with no other effusion than that of coagulable lymph, took place. And it is probable that many cases of pleurodyne are really instances of the same kind of pleuritic inflammation. How often do we find, even when there are no tubercles in the lungs, firm adhesions between the pulmonary and costal pleuræ, in the bodies of persons who were never known to have had any pectoral disease! The pain alone marks the inflammation in those cases; adhesion presently ensues; there is no fever perhaps, or none that attracts much notice; the pain soon subsides, and is soon forgotten; but the adhesion, the consequence of the inflammation, remains: and this is a morbid condition which is neither revealed to the sense of hearing, nor in any other way. I am much disposed therefore to agree with Cruveilhier in thinking that '*pleurodyne is nothing else* (in many cases at least) *than adhesive pleurisy.*'

I need scarcely repeat the fact which has so many times before been mentioned in these lectures, viz., that the inflammation of membranous parts, and especially of serous membranes, is attended with much more pain than the inflammation of parenchymatous parts. We cannot have a better example of it than is afforded in most cases of pneumonia. Most cases of pneumonia are accompanied in the beginning with a stitch in the side; some cases are not. In those cases in which the stitch happens, the pleura also is inflamed to a certain degree, and the pain depends upon the coexistence of the pleurisy: they are cases of pleuro-pneumonia. In pure pneumonia, on the contrary, the pleurisy being wanting, the sharp pain is wanting also.

The respiration in pleurisy, at its outset especially, and while there is still pain, is considerably embarrassed: the movements of inspiration in particular are short, hurried, and often interrupted or jerking. And this depends evidently upon the pain, which forbids the free contraction of the muscles that dilate the thorax; and you may often observe that the dilatation is sensibly less on the affected side than on the other. Cruveilhier indeed denies this; or rather he states that he has never observed it: but it certainly is not an uncommon phenomenon. I have noticed it and drawn the attention of others to it, again and again.

When effusion has taken place—*that*, one can easily understand, will be likely to aggravate the dyspnœa; and it will aggravate it in a greater degree, or in a less, according to circumstances. Thus, if the *other* lung happen to be a diseased lung, then the compression of that which is on the side of the pleurisy will have a more injurious effect upon the breathing. The dyspnœa arising from the effusion and consequent pressure upon the lung will also be in proportion, first to the *amount* of the effusion; and secondly, to the *rapidity* with which it has taken place. When the effusion has been slow—or when it has long existed, and the case has become chronic—the circulation through the lung has had time to accommodate itself to the altered condition of the parts, the disturbed equilibrium between the quantity of air and the quantity of blood in the lung is restored, and the dyspnœa is consequently slight.

But there are very singular exceptions met with to all this. Andral states (and I have seen more than one instance confirmatory of his statement) that there are persons, with pleuritic effusion enough not merely to fill but to dilate that side of the chest on which it exists (and you will observe that we cannot doubt about the presence of the effusion in such a case), who appear nevertheless to be quite free from dyspnœa; and *that*, not merely while they are at rest, for they talk, get up, walk about, even take long journeys, without their respiration becoming so short as to make them complain of it. Now this is conceivable enough in old and chronic cases; but Andral further affirms that this absence of dyspnœa is not restricted to those cases in which the collection of fluid has taken place slowly; but sometimes happens even with patients in whom pleurisy has led to abundant effusion in a few days. He gives a case of this kind, in which the patient was not prevented by an enormous pleuritic effusion from carrying on, without fatigue, in the streets of Paris, his business as a carter. I remember having a butcher in the Middlesex Hospital in exactly the same predicament; and nothing could persuade him that he was otherwise than well, and fit to go out; and out accordingly he went. Remember, therefore, that there are great varieties in this respect. In some patients the dyspnœa never ceases to be urgent from first to last; and these are apt to prove fatal cases. In others the respiration is very much impeded at first; then the difficulty of breathing diminishes; and at length it ceases, long before the fluid is reabsorbed. In others again, by some unaccountable idiosyncrasy, the respiration remains at all times very facile, both at the outset and during the progress of the disease.

Cough is another of the ordinary symptoms of pleurisy. It does not occur in paroxysms. It is small, half-suppressed, ineffectual. In some few cases this symptom also is entirely absent, even though the inflammation is intense, and the effusion into the pleura considerable. When cough does exist it is dry; or it is accompanied by the expectoration of slight catarrh. If much frothy mucus should be expectorated, the pleurisy is complicated with bronchitis; if rust-coloured sputa be brought up, it is complicated with pneumonia: and in each case other signs, proper respectively to those two diseases, will be present.

A good deal has been said and written respecting the position which a patient assumes who is labouring under pleurisy. The manner of the *decubitus* has even been regarded as one of the pathognomonic signs of the disease. Yet, strange to say, observers are much at variance with each other in regard to this so-called pathognomonic symptom. Some affirm that the patient lies on the side affected; others that he can lie only on the sound side; others again that he lies neither on one side nor on the other; or even that he lies indifferently in any posture. But this dispute is an exact counterpart of the celebrated quarrel which took place about the colour of the chameleon: 'they all are right, and all are wrong.' I believe that, if you narrowly inquire into the facts, they will be found to be somewhat as follows:—In the outset of the disease, while there is yet pain, the patient cannot lie on the affected side on account of the pain, which that position exasperates; he lies therefore on the sound side, or on his back; sometimes he is obliged to sit up. At a more advanced period of the disease, when the pain has ceased, and considerable effusion has taken place, he cannot lie on the sound side, because of dyspnœa: the dilatation of the chest on that side would be impeded by such a posture; and what is more, the effusion, lying uppermost, would press upon the mediastinum, and so further tend to restrain the expansion of the sound lung. But he is no longer prevented by pain from lying on the diseased side, and consequently he does, in some instances, take that position: but more commonly still, he lies in what Andral calls a *diagonal* posture; *i.e.* the patient is not on his back, nor on his side, but between the two; on his back, we may say, but inclining towards the affected side. Again, however the fact may be explained, it is certain that there are some few persons who lie indifferently on the back or on either side, without augmentation of the dyspnœa in any of these positions, though one side is choke-full of liquid.

Now, of the symptoms that we have hitherto been considering, the pain, the dyspnœa, the cough, the accommodation of position, there is not one which, taken alone, can be said to be strictly or absolutely pathognomonic; or which indicates in a positive and certain manner the existence of pleurisy, or of pleuritic effusion. Yet when all, or several of them, occur together, they afford a degree of probability on these points almost equivalent to certainty. There are yet some other, and more conclusive signs, which either in themselves, or taken in conjunction with those already mentioned, render the diagnosis of pleurisy easy and sure. These signs are furnished by the size of the thorax on the affected side; by its form and motions; and above all, as you will have anticipated, by percussion and auscultation.

I have already stated that in some cases, that side of the chest which contains the effused fluid becomes evidently larger than the opposite side. The ribs and their cartilages present that position which they assume during a deep inspiration: the intercostal spaces are pushed outwards and brought up to the level of the ribs; and occasionally fluctuation may be perceived in those spaces, through the muscles. When these appearances are observable, no doubt (or *scarcely* a doubt) can remain concerning the nature of the disease. This dilatation of the thorax on the diseased side is more common in old chronic cases than in the earlier periods of acute pleurisy; yet it *may* take place in a very short time. Andral declares that he has known it sometimes reach a great degree by the fourth or fifth day of the acute disease. You may satisfy yourselves that the side *is* dilated by measuring it with a string. Carry the string round the chest, upon a level with the extremity of the xyphoid cartilage, then fold it upon itself, and you will find that the half of it will more than encompass the sound moiety of the chest, and will not reach round the diseased. The diseased side may measure an inch, or an inch and half, or even sometimes two inches, more than the other. But this measurement by a string is seldom necessary. The eye takes a very accurate estimate of the comparative volume of the two sides; and the obliteration of the intercostal depressions can only be ascertained by seeing or feeling for them. It is necessary to remember that, in most persons, the right side is naturally somewhat the larger of the two.

I say when this dilatation is noticed, *scarcely* a doubt can exist of the true nature of the case. Some time ago I should have said *no* doubt: but having myself mistaken such a case, and seen others mistake it, I introduce this slight qualification, although it is a thousand to one against another such instance occurring to puzzle or mislead the observer. My colleague Dr. Hawkins had a patient in the hospital, in whom this dilatation of one side of the chest was exceedingly well marked. It was the left side that was enlarged; the heart was evidently pushed over to the right of the sternum. This is another circumstance strongly corroborating our conclusion in such cases. The intercostal depressions were effaced, and the whole of that side was perfectly dull on percussion. The poor fellow had a very unhealthy aspect; and he had, some time before, suffered amputation of a leg, for what was understood to have been scrofulous disease of the knee-joint. It was not unnatural therefore that every one who saw him should have come to the conclusion that this was a case of empyema; of fluid, and most likely of pus, collected in the pleura, and very probably the result of the extension of scrofulous disease from the lungs. Under these circumstances, and inasmuch as his dyspnœa was not urgent, it was not thought right to take any steps for evacuating the presumed fluid. The case was pointed out to the pupils as a capital example of empyema. At length the patient died; and when his body was examined we discovered—what think you? not pus, nor serum, but a large red *solid* mass, in the centre of which, when it was divided, was still a red, but softer, pultaceous, half-fluid substance. At first it was thought to be cancerous degeneration of the lung; but it was soon noticed that the solider part was arranged in concentric layers, like those which are often seen in aneurismal tumours; and further research showed that the effusion had indeed once been liquid, for it consisted entirely of blood, which had coagulated in the manner I have just described. And the source of the blood was detected. A portion of two of the ribs had been destroyed by ulceration, and one of the intercostal arteries had thus been laid open. The lung was found uninjured, but totally empty of air, and pressed flat up against the mediastinum.

No precaution could guard against such a source of fallacy; and you are not likely ever to meet with just such another case: yet I have thought it sufficiently interesting to relate, in illustration of the subject immediately before us. An instance is referred to by Trousseau, in which the presence of an encephaloid tumour within the pleural cavity was mistaken, by a practised observer, for pleuritic effusion.

It is unfortunate, so far as the diagnosis is concerned (but not in any other sense), that dilatation of the thorax is far from being a constant symptom, even in cases in which the effusion is very considerable.

There is still a condition of the thorax to be described, which is the very opposite to this. When the effused fluid begins to be reabsorbed—and when some cause or other, generally the formation of false membranes, prevents the lung from re-expanding and approaching the ribs in proportion as the fluid is removed—then of course the ribs *must* sink in, and approach the lung, to prevent that void which would otherwise exist between the ribs and the lung. Consequently that side of the chest on which the fluid has existed becomes narrower than the sound side. And the actual difference between the two will be augmented by the circumstance that, in such cases, an amplification of the sound lung, and of the cavity in which it is lodged, a true compensatory hypertrophy, commonly takes place.

This partial or general retraction of one side of the chest is not so much a sign of disease actually in progress, as of disease gone by; and it may exist without evident disturbance of the health, of any kind.

Persons who are thus affected have the appearance of being inclined towards the diseased side, even when they endeavour to hold themselves upright: and the deformity, for such it is, becomes manifest to the eye when the chest is uncovered. You see that the side is narrowed and shrunken. All its dimensions are contracted. It measures less, in circumference, by an inch or more, than the other side. The shoulder is depressed; the hypochondrium is tucked up; and the ribs are drawn close together. A patient of mine, whose chest had been punctured (a remedial procedure to be spoken of presently), and who drew off daily, with a syphon, pus which did not otherwise find vent, had such difficulty at last in introducing the tube between his ribs, that excision of a piece of the bone was contemplated by the eminent surgeon who had performed the operation of puncturing. The effect of the atmospheric pressure is sometimes so great as to crook the vertebral column, and produce lateral curvature of the spine. This I have myself witnessed. And as one of the unseen walls of the cavity, viz. a part of the diaphragm, is carried permanently up under the ribs, so another of the unseen walls, the mediastinum, is liable to be influenced by the tendency to contraction. The heart which, when the *left* pleura is *distended*, is apt to be *thrust* over, beyond the sternum on the right, may thus, when the *right* pleura is *contracted*, be *dragged* into the same position. In the former case, the dull sound given out by the diseased side when struck, will transgress the mesial line and encroach a little upon the healthy side: in the latter, the resonance yielded by the healthy will transgress the mesial line, and encroach a little upon the diseased side.

The difference of the two sides is so striking, that, at first sight, an observer supposes it to be even greater than it is actually found to be by measurement. Yet I have met with this deformity, as Laennec declares that he also had, in persons who were not themselves aware of its existence. Some of them have not even known that they had suffered any previous thoracic disease.

The conditions I have just been describing are *physical* conditions; and the signs they furnish are *physical* signs. I have still to speak of the remaining physical signs which are also *auscultatory* signs. What I have already said upon this subject in the present lecture will, I trust, enable you almost to foresee the kind of information which these signs afford in actual practice.

As soon as even a slight amount of effusion commences in the pleura, it is announced by a diminution of the hollow sound which percussion elicits in the healthy state. In proportion as the effusion becomes more considerable, the chest, when struck, gives a sound more and more dull. At first this flat sound is rendered opposite the lowermost depending part only of the cavity ; and this, as I showed you before, forms one ground of distinction between the dulness on percussion in pleurisy, and in pneumonia. However, at length, the effusion augmenting, the dead flat sound may proceed from the whole of the affected side ; and this forms another ground of distinction : for it is very seldom that the whole lung becomes so solid in pneumonia as to yield a uniform dead sound over the whole of one side of the chest. Either the dull sound is universal on one side, or it is not. If universal, it is not likely to be the result of solidification by pneumonia ; or, I may add, by tubercles : if not universal, the dull sound will (except in some rare cases) shift its place as the patient alters his posture.

I may mention another ground of diagnosis, which may be of great assistance when the case is seen from the beginning. The dulness comes on much more quickly in pleurisy than in pneumonia. It has been noticed within twelve hours from the invasion of the disease. In living animals, a considerable quantity of serous effusion has often been very rapidly produced by injecting some slightly irritant matter into the cavity of the pleura. In pneumonia, the dulness is commonly later in its appearance. The induration of the lung is gradual ; and so is the pneumonic dulness on percussion : the effusion of serous fluid is early and rapid ; and so also is the coming on of the pleuritic dulness. Moreover, as I have just shown you, pleurisy may displace the mediastinum, and cause the *whole sternum* to give a dull sound. A hepatized lung will render *one-half* only of it dull.

The intensity or completeness too of the dull sound is generally greater in pleurisy than in pneumonia. In two days, or even in twenty-four hours, the whole cavity of the pleura on one side may be filled quite full ; and the whole of the corresponding surface of the chest, from its base to its summit, will yield a sound (to use one of Avenbrugger's strong expressions) *tanquam percussi femoris*. It is very uncommon for such total and universal *matité*, as the French call it, to result from inflammation of the lung.

Again, in the outset of the disease, while there is yet little or no effusion, but when the pain is acute, the vesicular breathing is heard more faintly and feebly on the painful side than on the other. On that side also the walls of the chest are less forcibly expanded. But percussion, when the pain will permit of its being practised, gives the same sound on each side. It is clear that the sharpness of the pain causes the patient instinctively to expand the chest on that side as little as possible: and, consequently, the quantity of air that penetrates the lung in a given time is diminished, and the respiratory murmur is feeble.

As soon as effusion commences, the vesicular rustle is heard still less plainly on the affected side; and in proportion as the fluid increases, that rustle or murmur becomes more and more faint: and at the same time it becomes more distinct and noisy than natural—*puerile*, in fact—on the sound side. And while the respiratory murmur is disappearing on the diseased side, and the spongy lung is becoming empty of air from the pressure of the augmenting fluid, and the larger bronchi are surrounded by compressed lung and by incompressible liquid, the bronchial sounds begin to be heard, which I formerly described—the bronchial voice, the bronchial respiration. But the sounds are not exactly the same as those which are heard in pneumonia. They are modified by the nature of the substances through which they pass. The voice, for example, is still bronchial, still the voice of a person talking into a tube: but it has a superadded character; it is trembling, quivering, thrilling, cracked, discordant. I strive in vain to convey to you by these epithets a notion of this remarkable modification of the voice. Laennec's happy similitudes may enable you to form a more exact conception of it. It is like (he says) the bleating of a goat; or, happier still, it resembles the voice of Punch. But when once you have heard the sound you will never afterwards forget it. I presume that this modification of bronchophony (for such it is, and such I would have you consider it) is caused by the rapid undulations communicated to the effused liquid by the vibrations of the bronchi and condensed pulmonary tissue. The sound is usually most distinctly heard near the inferior angle of the scapula, the patient being in a sitting position. It disappears, or merges into pure and distant bronchophony, when the liquid exceeds a certain amount, so as to compress the bronchial tubes themselves, and to *damp* their vibration.

I would have you recollect, therefore, that ægophony, which is the technical appellation of the sound I have just been describing (goat-voice),—ægophony is nothing more than a species or variety of bronchophony; and the two run each into the other by such fine gradations, that it is sometimes difficult to say which it is we are listening to. When the quivering is strongly marked we may be certain that it denotes effusion into the pleura; when bronchophony only is heard, we cannot be sure, from that sound alone, whether there be indurated lung between the ear and the bronchi; or a liquid, and a portion of compressed and condensed lung: but other phenomena complete the diagnosis.

Do not forget that when any modification of the voice is heard, or thought to be heard, on the suspected side, the sound of the voice in the corresponding part of the other side of the chest must be ascertained also. It is only by a *comparison* of the two sides that we can come to any safe conclusion; and that *comparison* becomes often a striking and most instructive *contrast*.

LECTURE XLVII.

Pleurisy continued. Recapitulation of Symptoms; of Diagnostic Signs. Causes of Pleurisy. Pneumothorax; its Conditions and Signs. Treatment of Pleurisy. Empyema. Paracentesis Thoracis.

In the last lecture I enumerated the symptoms, general and physical, which are met with, more of them or fewer, in cases of acute pleurisy. I then considered them singly; it may be well to take a rapid recapitulatory view of them as they exist together or in succession, and compose the actual disease.

The outset, then, of pleurisy is marked by sharp stabbing pain, most commonly situated beneath one of the breasts, and preceded or accompanied by rigors. These two signs, the stitch and the shivering, are sufficient of themselves to awaken a strong suspicion that pleuritis has set in. At the same time there are usually a dry cough; a dread of breathing; a check or catch in the inspiration, which is curbed, so to speak, by the pain; fever; often a comparatively feeble respiratory murmur on that side on which the pain is felt; and the patient cannot lie on that side. If no liquid effusion take place, these symptoms ordinarily disappear at the end of a few days, and the patient recovers. The case has been a case of dry pleurisy; and the chances are much in favour of the lung having become permanently adherent to the ribs.

I should have said, with respect to the *fever*, that at the outset of the pleurisy it is often high. And it was matter of observation long before the method of auscultation was thought of, as well as since, that in the acute period of the disease the *pulse* is remarkable for its hardness, and forms a contrast with the softer pulse of pneumonia, and with the small and contracted pulse of inflammation of the serous membranes of the abdomen. Indeed the older physicians laid great stress upon the quality of the pulse, in their endeavours to distinguish pleurisy from pneumonia.

But to resume the description of the symptoms of pleuritis. Where effusion takes place (and it does so very early, so as to form a part of the complaint, just in the same sense in which expectoration forms a part of catarrh), the sound elicited by percussion becomes dull on the side on which the effusion exists. While the effusion is moderate, the dulness shifts its place according to the posture of the patient, and is heard only when the lowermost part of the chest is struck. But the fluid may soon increase so much as completely to fill the pleura; and then the whole of that side is dull. Meanwhile the murmur of respiration becomes feeble and faint, and at length, as the effusion augments, ceases altogether; while on the sound side it grows noisy and puerile. Tubular breathing, and that modification of the bronchial voice which medical men have agreed to call ægophony, become audible during the early periods of the effusion. Ægophony is heard, however, only so long as the quantity of liquid poured out observes a specific limit. There must be a certain amount of effusion—and there must not be more than a certain amount. I have sometimes thought that the discordant sound might depend upon the propagation of undulations through successive media of different densities. It seems to be somehow connected with the presence of a stratum of liquid between the lung and the ear. When the lung is strongly compressed, and especially when the cavity is stretched and distended by the enclosed fluid, the side is necessarily motionless; no tubular or other breathing can any longer be heard, or even occur: nor is the voice conducted, except perhaps very faintly, to the listening ear of the physician.

When the effusion is great, that side of the thorax on which it has taken place becomes, often, more or less dilated; and I should add, that the integuments on the same side are frequently œdematous. The patient now cannot lie on the sound side: and the most common posture is that which is intermediate between the supine position and the lateral; he lies *towards*, but not, in general, *upon*, the affected side.

I observed, in the last lecture, that this inability, after the effusion has reached a certain point, to lie on the sound side, might be accounted for in two ways. Partly it may be owing to the impediment which lying on the sound side offers to that side's expansion. The muscles which dilate the healthy side have then to lift, as it were, the weight of the body, and are, some of them, pressed upon and incumbered in their action, by that posture. But the inability in question is chiefly attributable to another circumstance, viz. the pressure exercised by the effused fluid downwards, through the mediastinum, upon the only lung that is left to perform the function of breathing. Now disputes, or differences of opinion, have arisen as to which of these two circumstances is the most efficient cause in this matter: and therefore it may not be amiss to provide you with the facts which prove that the last-mentioned cause is, in reality, the most operative—I mean the weight of the superincumbent liquid, in the supposed position, upon the mediastinum and upon the healthy lung below it. This is shown by the fact, that patients, to whom the decubitus on the sound side had previously been impossible, on account of dyspnœa, have been able to rest in that position *immediately after* the artificial evacuation of the fluid. Now in such a case the obstruction to the dilatation of the healthy side, produced by placing it under the weight of the body, would remain the same as before, or nearly so. A hospital patient of mine, named Coggs, could not breathe if he attempted to lie on his right side. His left pleura was distended by liquid effusion. I thought fit to have paracentesis performed: and the poor man was greatly delighted to find himself at once enabled by it to assume the posture which his weariness had long made him wish for, but which he had not been capable of enduring. We found, by percussion, that the diseased side was now filled with *air*; the compressed lung had not risen at all; so that the necessity for the free expansion of the sound side was just as great as before the operation. You may find a precisely similar consequence of the same operation related in the fifth volume of the 'Dublin Transactions.'

The œdema that is sometimes observed on the diseased side is more or less connected, probably, with the habitual position of the patient.

There is yet another sign of pleuritic effusion, which, as it is very simple, and readily perceived by even the least instructed observer, is too valuable to be neglected. In most persons, one's open hand, laid flat upon the surface of the chest, feels the vibration or thrill which the voice occasions when the person speaks. Now, in a case of pleurisy with effusion, you will generally find a remarkable contrast between the two sides in this respect: i.e. the thrill is strong and evident on the sound side, and not perceptible at all on the other. Whereas, when the whole side is dull in consequence of the solidity of the lung, the thrill is, *in many cases*, much *augmented* on that side. But this thrill is not always present in the healthy state, and then we can infer nothing from its absence on the diseased side. Its presence, however, under such circumstances, would indicate consolidation of some kind.

That the vocal fremitus is not increased or produced in *all cases* of hepatized lung, but is even as often diminished as increased, I have been recently assured by Dr. Johnson, who, after careful watching and observation for many years past, has convinced himself of this strange, and to me new and inexplicable fact.

After a while, when the fever has ceased, the liquid begins to be reabsorbed; but as, in many cases, the lung is more or less bound down by adhesions, or overlaid by a membranous stratum of lymph, it cannot expand in proportion as the liquid is removed: and the necessary consequence is, *that* shrinking of the affected side in all its dimensions which I fully described yesterday.

Let me now briefly re-state the points of distinction between pleuritic effusion and pneumonic consolidation, when the one or the other of these two morbid conditions is proved to exist by dulness on percussion, extending over the whole of one side of the chest. The question is one which frequently arises; and it is one of much interest and importance.

First, then, we distinguish these different conditions, having some physical signs in common, by their *history*. In pleurisy, sharp pains and a dry cough, or perhaps no cough, precede the dulness: and we have not the crepitation, nor the rust-coloured sputa, which are antecedent to the dulness of pneumonia.

We cannot, however, always learn the previous history of a given case.

Secondly, a lung rendered solid by inflammation does not *distend* the cavity. Copious pleuritic effusion most frequently does. In the first case, therefore, we have not that separation of the ribs, that obliteration of the intercostal depressions, that protrusion of the corresponding hypochondrium, that mensurable enlargement of the side, that extension of the dull sound beyond the middle of the sternum, or that displacement of the heart, which are, some or all of them, apt to result from a collection of liquid in the pleura.

Thirdly, the solid lung transmits the voice from the pervious bronchi to the surface of the thorax; and if any motion of the affected side remain, it transmits also the sound made by the passage of the air through them. These phenomena are mostly awanting when the pleura is so stretched by its liquid contents as to make the side everywhere dull to percussion.

Fourthly, the vibration of the thoracic parietes, caused by the patient's voice, is sometimes augmented by consolidation of the lung; prevented when it is strongly compressed by imprisoned liquid. The increase of this thrill can sometimes be felt therefore in the one case; its diminution always in the other.

This simple test fails to be applicable when, from the feebleness or the high pitch of the person's voice, no thrill is perceptible on either side in the healthy state. Unless, indeed, disease should generate a vibration, which, in such a case, would certify solidity.

Fifthly, a patient having one lung solid, is generally indifferent as to posture. A patient having one pleura quite full of liquid, lies (usually) on or towards that side; and is distressed and suffers dyspnœa if he attempt to lie on the other.

It is of more importance that we should inquire into the *exciting causes* of pleurisy than into those which give rise to pneumonia. They are more numerous and complicated, and have a more direct bearing upon the prognosis and treatment in the one case than in the other. I do not desire to refine too much; and therefore I shall restrict myself to those causes which are obvious, and which you are likely to meet with in practice.

I merely say of exposure to cold, as an exciting cause of pleurisy, that it is a very common—the *most common*—cause. You know already all that I can tell you of the circumstances that are likely to render that cause effectual in producing internal inflammations, and, among the rest, pleuritic inflammation.

Again, pleurisy is frequently a consequence of blood-poisoning: as in cases of uræmia in what is called Bright's disease, and of pyæmia; conditions which I shall have to describe and explain to you in a future lecture.

But pleurisy is often occasioned by mechanical violence; or by the accidental extension of disease from other parts; and the course, and the event, of the disease, are liable to be considerably modified by the nature of its cause in such cases.

Pleurisy may be excited by the splintered ends of a broken rib; and if the pulmonary pleura be wounded in that manner, air may get into the pleural cavity, as well as into the areolar tissue beneath the skin; constituting the true and genuine *emphysema* of our forefathers. Pleurisy may be determined also by a penetrating wound of the thorax; or by a perforating ulcer of the pulmonary pleura, the extension of a tubercular excavation. In the one case air will enter from without, if the aperture be sufficiently large; in the other, air will pass from the lung into the cavity of the pleura. In all of these cases of air finding its way into this serous sac while in a state of inflammation, the event of that inflammation is much more likely to be the effusion of *pus* than when no communication exists between the inflamed membrane and the atmosphere. This I have mentioned, and offered some explanation of, before. But another very curious consequence results from the admission of air, and its coexistence with puriform or other liquids in the sac of the pleura. New auscultatory signs arise, very easily appreciated, very instructive, and therefore very necessary for you to be acquainted with.

You must know that when the pleura contains air alone, the patient is said to have *pneumothorax*; and when (what is infinitely more common) air is there in company with a liquid, he is said to have *pneumothorax with effusion*. This is the name given to that condition of the chest by Laennec; and it serves its purpose sufficiently well. I shall take leave to employ the simple term pneumothorax, in speaking of either condition; whether there be liquid also in the pleural cavity or not. Pneumothorax, then, often proceeds from one or other of those causes of pleurisy just mentioned. It is sometimes produced too by the operation of paracentesis thoracis; by the opening made into the thorax by the trocar of the surgeon, in order to let out its liquid contents; in plain English, by *tapping* the chest.

The modifications of sound that result are particularly curious. Of course the air occupies the higher portion of the cavity and the liquid the lower, in whatever position the patient may be placed. And this being the case, percussion will give a remarkably hollow sound when made upon the uppermost part, and a totally dull and flat sound when made upon the lowermost part: and the change from the hollow to the dull sound will often take place quite abruptly, so that you may trace out the exact level at which the surface of the effused liquid stands. And if you reverse the posture of the patient, the resonant and the dull sounds will interchange their respective places: the uppermost part always yielding the clear, and the undermost the flat sound. This is just what you would expect. The result of the experiment is the same whether you make it upon the human thorax, or upon a beer-barrel. The resonant part, you are to observe, will be much *more* resonant than it would be in health—more resonant (you have always the other lung to test it by) than the corresponding portion of the opposite side of the chest—tympanitic, drum-like; for the air is not involved in spongy lung, but contained in a free space: and the sound is not damped, as in a healthy chest it is damped somewhat by the *presence* of the lung. Moreover no respiratory murmur can be heard where this tympanitic resonance occurs: nor can any thrill be felt. Now I say all this is no more than you must have foreseen. But the sounds detected in this new condition of things by the ear applied to the chest, as the patient *breathes* or *speaks* or *coughs*, you would *not*, I think, have anticipated. You hear then a sound which I must endeavour to describe in words, but which you will scarcely form a right conception of till you have heard it, and then all further verbal description will be needless. I can describe it by similitudes only. The patient's breathing is like the noise produced by blowing obliquely into an empty flask; and so the French have given the sound the somewhat magnificent title of 'amphoric resonance.' I have heard, fifty times over, exactly the sound in question when I have been out shooting in a gusty day, and the wind has blown sideways into the gun-barrel. It is a ringing, metallic sound. When this is present during the *breathing*, the *voice* also has, even *more strongly* in general, this metallic character; and so has the cough; and each of them is apt to be succeeded by a tinkling echo. The voice, and cough, resemble those of a person who speaks or coughs into a deep bricked well: or with his head bent over an empty copper boiler. The same ringing quality is often heard when one speaks in a large vaulted room; or beneath the arch of a stone bridge. You may perhaps now have some idea of what these metallic sounds are. They are very singular: and they are perfectly decisive (so far as my experience has gone) of the presence of air in a considerable cavity, within the thorax; which cavity mostly contains liquid also: and of the presence of air and liquid in the cavity of the *pleura* in particular. I do not know that the liquid is essential: I do not believe it is; but commonly there is some liquid, and a good deal of air. Almost always, too—but *that* is not indispensable—the cavity communicates with the external air, either through the walls of the chest, or through the bronchi. Neither is it necessary that the cavity should be in the pleura, for it may be in the lung: and when we come to speak of phthisis, I shall point out the circumstances which may enable you to determine whether the sounds proceed from a tubercular or other cavity in the lung, or from the sac of the pleura. What you will please to remember is that, in actual practice, in ninety-nine cases out of a hundred, these sounds will be found to denote the presence of both air and liquid in the cavity of the pleura; and the probable existence of some passage of communication between that cavity and the *external* air: in a single word, they will reveal the existence of pneumothorax. The voice reverberates in the little cavern just as it does in a large empty room with a stone roof; and this is the best explanation I can give you of the phenomenon. Sometimes, as you are listening, especially if the patient have recently changed his posture, you will hear a sound just like that occasioned by dropping a pin's head into a glass vase, or into a metal basin: and to this sound the name of *metallic tinkling* has been given. It really often closely resembles the distant tinkling of a sheep-bell. This is supposed to result from the dropping of the liquid from the upper part of the cavity; or sometimes from the bursting of a bubble on the surface of the liquid during respiration. You may succeed now and then in hearing a species of the same metallic tinkling by applying your stethoscope over the stomach, when percussion has already taught you that it is distended with gas, and by getting the patient to swallow some drink in successive teaspoonfuls.

Another auscultatory sound, arising out of the same condition, viz. the presence of both air and liquid in the cavity of the pleura, and known even as early as the time of Hippocrates, is rendered audible by *succussion* of the patient's body. You lay your ear upon his side, and get him to give his body a sudden jerk or jog: or you get some one else to take him by the shoulders and shake him: and you hear the liquid splashing within; just as you hear it when you shake a cask that is neither full nor empty of water. This is an unequivocal indication of pneumothorax; and demonstrates beyond a doubt that there is both air and liquid in the pleural sac; for no sound would arise if there were liquid only. A moderate quantity of liquid will make a greater squash than a large quantity. Unequivocal I say it is, because one could scarcely be misled by the splashing which may sometimes arise from wind and water mingling in the *stomach*. I wish that a patient, who was under my observation for some months last year in the Middlesex Hospital, and could produce this splashing noise at will, were there now; for he was not a little proud of his fatal gift, and I should have brought him down here to-day, and given you an opportunity of hearing this sound for yourselves, worth a dozen descriptions of it.

It is surprising how long this state of things within the thorax may last, without any great declension of the patient's general health and strength, even when the disease is (as it mostly is) incurable. Two men, patients of mine, both of whom had well-marked pneumothorax in connection with tubercular phthisis, remained in the hospital for several months; till, in fact, I could conscientiously keep them there no longer: and each of them went away in very tolerable plight. I was unable to trace them afterwards, for they returned to their homes, the one in Ireland, the other in the north of Scotland.

In June 1853, I was consulted upon the case of a lady in whose right chest, near the angles of the ribs, just below the scapula, the physical signs of pneumothorax were strongly pronounced; amphoric breathing, amphoric voice, metallic tinkling, and a splash following succussion. This last sound I and others often heard, while standing near her, without any application, direct or mediate, of the ear to her chest. It was audible by herself when she was jolted in a carriage, and when she cantered on horseback. She first heard the internal splashing, while cantering, in June 1852. In November of the same year she had borne a child. Under cod's-liver oil she grew strong and stout, and could take a good deal of horse exercise. She died, rather suddenly, at a distance from London, in January 1855.

In this instance pneumothorax existed at least two years and a half; probably longer. The patient began to cough in 1851.

You see, then, that the conditions of pleurisy, and the symptoms of those conditions, may be modified by its causes. All those causes that imply the introduction of air into the cavity of the pleura, imply also a more serious state of disease than results from most other causes. The perforation of the pleura by the extension of a vomica, I have mentioned as one of those causes. But tubercles in the lungs are frequently, very frequently, the cause of pleurisy, when no such perforation has taken place. A tubercle, or a group of tubercles, approaches the surface of the lung, but does not break through. Generally the pleurisy so produced is slight and partial, and ends in the formation of adhesions: it is *dry* pleurisy. And this very common occurrence of adhesions between the costal and pulmonary pleuræ, in the course of tubercular disease of the lungs, is, in truth, one reason why *perforation* of the pleura, and pneumothorax from that cause, is comparatively so rare. The part where the perforation is likely to take place has generally, though not always, been secured, and clouted as it were, by previous adhesion. So that even here we find that inflammation has a conservative tendency, and helps to postpone the fatal ending of the specific disease.

Pleurisy may terminate in resolution and complete recovery; or in adhesion, which is its next best termination, and which obtains for the patient, at the expense of some trifling embarrassment of his breathing, complete security for the future against the dangers of pleuritic effusion. Again, acute pleurisy may end in chronic disease of the pleural cavity: i.e. in a shrinking inwards of the walls of the chest, attended with total uselessness, or a very imperfect and limited use, of the corresponding lung. Lastly, pleurisy may terminate in death. It may cause effusion so copious, that the patient will die of actual suffocation, unless the fluid be removed by art. On the other hand, he may die worn out and exhausted by the disease, especially if it be attended with suppuration. In that case he will suffer hectic fever, and all its wasting and mournful accompaniments; and death ultimately by asthenia. It is seldom that simple idiopathic pleurisy proves fatal.

As the matter from a pulmonary cavity may break *in* upon the pleural sac, and lead to the admission of air, and the establishment of pneumothorax; so the puriform fluid which has resulted from inflammation of the pleura, and was for some time imprisoned in its sac, may also break *out*, and the consequence will still be the admission of air, and pneumothorax. This is not a very frequent result of pleurisy, however. When it occurs, an abscess forms externally, generally in front of the chest; and either the abscess bursts, or it is opened by the surgeon, and then it is found to communicate with the cavity of the pleura.

Sometimes air is effused into the sac of the pleura, in consequence of the rupture of dilated air-cells on the surface of the lung; of this accident of disease I may refer you to an instance related by Dr. Lloyd, in a paper upon pneumothorax, contributed by Dr. Hughes to the eighth volume of the 'Guy's Hospital Reports.' Sometimes gas is generated within the sac, from the decomposition of effused liquids, or of a gangrenous lung; and in such cases the gas has a strong odour, like that of sulphuretted hydrogen: sometimes, again, gas is said to be secreted from the membrane itself. All these events are, however, uncommon. When air, from whatever source, is shut up in the cavity of the pleura, and goes on accumulating there, it will compress the lung, just as certainly and effectually as if there were a liquid extravasated. And such compression, if suddenly brought about, may cause speedy death by apnœa : and this is more apt to occur from a scratch of the pulmonary pleura by the rough edge of a fractured rib, than from any other cause.

Pleurisy is one of those internal inflammations, in which we fortunately have it in our power to adopt the most potent of all remedies for inflammation in its primary stage—I mean the withdrawal of blood from the part inflamed. We need not even enquire into the validity of the allegation that damage might ultimately accrue to the patient from the employment of *general bleeding*. The capillaries of the pleura may be eased of their superfluity of blood, either directly and absolutely, or by that process of diversion which was sufficiently dwelt upon in a previous lecture. But to be fully effectual, the easement must be given at the earliest period of the inflammation. Apply cupping-glasses and the scarificator over the painful side, or, what perhaps is better—especially in the present dearth of expert cuppers—cover the side with leeches, and when they have sucked forth what they can, let their bites continue to discharge blood into a soft absorbing poultice. You may confidently expect, as a result of these measures, a speedy abatement or cessation of the pain, and that the patient will soon be able to draw a full breath again without check or fear. It may happen that the inflammation shall thus be brought to an end, that whatever fluid had been thrown out shall be completely removed, and the chest be restored to its former state.

But though pain and fever may both have passed away, and the patient is not conscious of much dyspnœa, there may remain evidence, not to be mistaken, of effusion, more or less in amount, within the cavity of the pleura; dulness on percussion, bronchial respiration, ægophony: and the object of our treatment now is to get rid of the effused fluid. We seek to do this by putting our patient on slender diet. By keeping the vessels comparatively empty, we facilitate, *pro tanto*, the absorption of the liquid contents of the pleura. We pursue the same end by applying blisters, one after another, to the affected side; by purgatives, diaphoretics, and above all, if the kidneys be sound, by diuretics. A very good diuretic pill, suitable for this state of matters, may be made by mixing from one to three grains of blue pill with one grain of the dried powder of squills and half a grain of the dried powder of digitalis. This is called Dr. Baillie's pill. It may be given twice or thrice a day.

I have told you that blisters will do more harm than good, while the inflammation is yet recent and active: but they are both safe and beneficial in the later stages of the disease, when the events of the inflammation, and not the inflammation itself, have to be combated.

I will take this opportunity of remarking that in the present wholesomely sceptical mood of the medical mind, the question has been raised whether what we call counter-irritation is ever, under any circumstances, eligible as a remedial measure. Should any doubt trouble you on this point, I recommend you to read an admirable apology for counter-irritation, from the pen of Dr. Risdon Bennett, in the third volume of the 'Practitioner.'

Too often it happens that although the fever and the inflammation may cease, and absorption of the liquid takes place, the parts within the thorax do *not* revert to their original condition. This we know by that shrinking of its dimensions on the side affected, which was described in the last lecture. This shrinking and narrowing is the *necessary consequence* of the absorption of the liquid, *unless* the compressed lung dilates again in proportion as the fluid is taken up. In many cases of this kind the lung *cannot* rise; being bound down by thick and firm false membranes: and then the deformity is irremediable, and lasts for life. If the lung be completely emptied of air, and enveloped by strong bands of lymph, so that it is permanently unable to admit air again—in that case, as the bony framework of the thorax can yield to a certain extent only, there will always remain, I presume, some liquid in the pleural cavity. If, again, the lung recover a part of its lost volume, and *meet* the contracting parietes of the chest, adhesion may take place; and the cavity of the pleura be obliterated by thick layers of false membrane. And other changes are apt to arise in the lymph which is adherent to the pleura in these cases of imperfect repair. Sometimes tubercles form in it. Sometimes ossific matter is deposited. I show you a fine specimen of this kind of ossification of the pleura. There is yet another supposable case: the investing adventitious membrane may be thin and weak, and yielding; and though the lung may not expand to its full dimensions at first, it may gradually force its way against the binding power of the coagulable lymph, and then the external configuration of the chest may be restored, and the symmetry between the two sides return. That this sometimes takes place I cannot doubt: but I have only met with two cases in which the dwindling of the side was *entirely* recovered from. In May 1834, I was asked to see a child four years old, who had had cough, and had wasted to mere skin and bone, after scarlet fever. I found the whole of the right side of the chest perfectly dull on percussion, and no respiration could be heard on that side. He was taken by his parents into the country, and I did not see him again for some weeks. He then had ceased to cough, and, in a great measure, had regained his strength; but he presented on the side which had been dull, as marked and complete an example as I ever saw of that sinking in of the ribs, with flattening and contraction of the chest, and depression of the shoulder, which denotes bygone pleurisy, and diminished bulk of the lung. About a year from the occurrence of the original disease his father brought him to my house, that I might see the change which had again taken place. The boy was plump and rosy, and in perfect health; the right side of the chest was as full and round as the other: the symmetry of the two sides was completely restored; the breathing natural and perfect; and the sound on percussion hollow. His father, to whom the former shrunken state of the side had been pointed out, told me that he had watched, with deep interest, the process of recovery, and that it had been very *gradual*. The only other example of perfectly regained symmetry that I have seen, occurred in the person of an adult man. I shall tell you some particulars of this remarkable case presently.

There are yet other cases in which the effusion continues and increases, and the side, instead of shrinking, enlarges; the functions of the lung on that side are entirely abolished; nay, the use of the remaining lung is greatly interfered with, by the pushing over of the mediastinum; and the patient is in imminent danger of suffocation. In such cases, whether the effusion has taken place rapidly or slowly—whether the disease has been acute or chronic pleurisy—we must relieve the oppressed lung by *letting the fluid out*—by tapping the thorax; and the sooner that is done, when such a state of things exists, the better.

The operation is not difficult, nor formidable; but a mistake in the diagnosis may be *very* formidable. I have heard of two instances, one in Scotland and one in this town, in which the operation of paracentesis thoracis was determined on, to relieve the oppression caused by empyema: but the opening was made on the wrong side; and the patient in three minutes was, in each case, a corpse. There was effusion, which had already put a stop to the play of one lung; and upon air being freely admitted to the surface of the other, it collapsed also, and immediate suffocation took place. I do not mention these mishaps to deter you from performing the operation. They both took place some years ago. Such a mistake would be unpardonable now. But I mention them to show the necessity of our being sure of our ground before we proceed to open the thorax of a living person. A surgeon once told me that with the sanction and at the suggestion of a physician, who understood auscultation exceedingly well I believe, he passed a trocar into the chest of a patient; but no fluid followed, to the no small mortification of the physician. This proved to be a case of malignant disease of the lung; and fluid was let out afterwards by puncturing the thorax in another place, and much relief afforded; although of course the disease proved ultimately fatal. The surgeon informed me that he had suspected the true nature of the case, from observing a livid protrusion in front; which was, in fact, the specific disease making its way through.

You will take care, then, to survey the chest narrowly before you plunge a trocar into it. If you see by your eye, and ascertain by measurement, that one side is larger than the other; if the intercostal depressions be effaced, or converted even into elevations, on that side; if the whole surface afford a dull sound when percussed; if the side do not move at all, or scarcely move during respiration; if no vibration can be felt on that side when the patient speaks; if no breathing can be heard in the corresponding lung; if the heart be found beating in an unnatural place, down towards the left hypochondrium, or in the other direction on the right of the sternum; and if, at the same time, the other side of the chest shall move freely, sound resonantly, communicate a thrill to the hand while the patient converses, and be full of *puerile* respiration; then you may be sure that the larger side is distended with fluid.

But it does not follow that you should, therefore, open that side. The propriety of doing so will depend upon circumstances.

There are two objects for which the operation of paracentesis thoracis may be contemplated; and these it is well to keep distinct. It may be curative in its intention, or merely palliative. In cases of simple pleurisy it may be adopted with the view of saving life, and restoring health. Again, when pleuritic effusion is complicated with other and mortal changes, the operation, like that of tracheotomy, may sometimes be resorted to for the purpose of relieving urgent distress, and of prolonging, it may be, a doomed existence.

In simple pleurisy it ought never, in my judgment, to be performed unless the life of the patient is, or seems to be, in jeopardy, from the continued presence of the liquid within the thorax.

Now life is plainly in jeopardy when the vital functions of the lungs, or of the heart, are greatly hindered; when symptoms present themselves of approaching death by apnœa, or by syncope. If we discover no cause for those symptoms, except the increasing pressure of liquid, or of air, pent up in the pleura, we are warranted in ascribing them to such pressure, and bound to act upon that persuasion. Whenever, with the physical signs of abundant effusion, we have great hurry and distress of breathing; an anxious and livid aspect; a tendency to delirium—or extreme faintness, and a vanishing pulse —there is no time to be lost: it is our duty to propose and to urge the mechanical removal of the pressure which must else be fatal.

Again, when the patient, without suffering much dyspnœa while he lies quiet, is yet evidently losing ground from day to day, and early death by asthenia appears to be inevitable, without the operation; and when all other means for getting rid of the imprisoned liquid have failed; and when no other condition of disease, or of advanced age, exists to account for the progressive sinking; then also, in my opinion, the patient should not be denied the *chance* which the operation may afford.

Thirdly, whenever (no matter how we ascertain the fact) the effused liquid consists of *pus*, it should be let out.

In either of these three predicaments of simple pleurisy, and in no other, should we be justified (as I think) in making an opening into the living thorax.

But I wish to be understood as giving you simply the impression which my own experience has made upon my own mind. I know that some practitioners recommend the early employment of the trocar; while (they say) the false membranes, which are apt to prevent the compressed lung from expanding again, are yet tender and unorganized. But surely we should risk much, and gain nothing, by admitting air into the pleura while the inflammation is still in progress. Most cases of mere pleurisy with effusion do well. The mortality from uncomplicated pleurisy is exceedingly small. It would, I fear, be vastly augmented if every patient having manifest effusion were to be tapped. The danger of the operation is this;—that it may, and probably will, induce suppuration, or cause the effused liquid to become putrid. Generally the effusion consists of serous fluid, which is at length spontaneously reabsorbed; the lung expands again, or the walls of the chest shrink inwards: and the ultimate state of such a patient is as good as it probably would have been after a successful tapping.

To make assurance doubly sure, it is always right, before proceeding to the operation of paracentesis, to adopt the expedient first suggested and used, I believe, by Dr. Thomas Davies, of trying the chest by means of a grooved needle; making a tentative exploration of the nature of its contents in that manner. The passage of this little instrument—like the dismissal of a pilot balloon—affords information which is useful in guiding the particulars of the subsequent process. As a mere diagnostic measure it is highly valuable. It not only ascertains that there really is liquid within the pleura, but it discovers the kind and quality, and exact place of the liquid. If it be serous, it will flow readily along the groove, and trickle down the patient's side. If it be puriform and thick, it will not exude so freely, but a drop or two will probably be visible at the external orifice: and when the needle is withdrawn, its groove will be found to contain pus. In the former case it is possible that there may be no false membranes; in the latter they are likely to be thick. You would use a larger trocar to evacuate the thicker fluid.

The puncture thus made is quite harmless; and inflicts very trifling pain. Dr. Davies gives this useful piece of advice with respect to the trocar, that its point should be *sharp*: for otherwise, after the serous membrane has been penetrated, if there happen to be thick tough layers of coagulable lymph, not very closely attached to the costal pleura, they may be driven before the instrument, and so the liquid will not be reached, but the operator will be perplexed and baffled.

Connected with the operation itself there are some questions concerning which medical opinions and medical practice are scarcely yet settled. I do not pretend to decide these questions: yet I cannot pass them by. I must point them out to you; and I shall, at the same time, state what my own observation has suggested in regard to them.

1st. Should all the liquid be let out at once?

Some say yes; some say no. If we appeal to experience on this point, we obtain no satisfactory answer. I have known patients get rapidly and perfectly well, after as complete an evacuation of the liquid as was possible. On the other hand, I have heard of speedy recovery when, by a sort of accident, very little had been withdrawn: enough to relieve the pressing distress: but much less than the operator intended.

We must try the matter, therefore, by our reason.

I think it very probable that when the serous membrane is *stretched* by the pressure of its contents, its natural absorbing power may be lessened. But we have no reason to suppose that the mere relief of this tension will *often* suffice to renew the process of absorption, and to enable the flattened lung to re-expand.

The theoretic objection to the thorough emptying of the thorax in such cases is (I conceive), that the introduction of air is likely to be hurtful, by converting the adhesive into the suppurative form of inflammation, and by promoting decomposition of the extravasated fluids. No doubt there is this risk; but, in general, if you wish to empty the pleura, it cannot be avoided. Unless the lung rises freely at once, the liquid *cannot* all, nor even much of it, come out, without air getting in. But the mere admission of air into the pleura does not necessarily *create* inflammation of the membrane. This we know from what happens sometimes in emphysema produced by a fractured rib. In the only instance of *pure* pneumothorax which I ever saw, the sac of the pleura had become half filled with air, through a very minute opening in the pulmonary membrane, communicating with the air-passages. There was *no* inflammation of the pleura in that case. Except that it was preternaturally *dry*, it seemed perfectly healthy. Neither does the access of air necessarily superinduce suppuration in the membrane already inflamed. Certainly, if pus follow the passage of the instrument, as much should be removed as we can get. And, for my own part, I should take away as much as would come, whenever the inclosed liquid proved to be serous. If much be left behind, a repetition of the operation will commonly be required. Air disappears spontaneously with far greater readiness than serum; and opposes less resistance, while it remains, to the gradual re-expansion of the compressed lung. Should you desire however to take away, as some advise, so much of the liquid, and no more, as the expansion of the lung, and the elastic resilience of the thoracic parietes suffice to press out, without admitting air—that object may be insured by adopting a simple contrivance of Professor Schuh's, of Vienna, which was shown me by Mr. Spencer Wells. It consists of a sort of small trough, which is readily fitted to the end of the canula after the trocar has been withdrawn, and which is provided with a valve that prevents any reflux of liquid, or passage of air, from the trough into the canula. In this way the entrance of air into the pleural sac may be effectually obviated. But a method which I have seen employed by the late Mr. Stanley, seems to me more simple, and equally certain. He used a trocar furnished with a stop-cock. To the trocar thus shut he adapted a long flexible tube, the open extremity of which was immersed in a vessel containing water, and placed on the floor. A circumstance that happened on the last occasion of his operating on a patient of mine is worth relating, as being suggestive of a caution. While serum was rapidly issuing, and mixing with the water in the vessel, the patient, a lad of about fifteen, began to cough, and presently each act of coughing filled his mouth with blood. Thereupon the stop-cock was turned, the opening closed with adhesive plaster, and the operation concluded. In this case the accumulation of the serum within the pleura had been gradual, and there were probably no adhesions. The lung therefore rose as the pressure upon it by the effused liquid was lessened, and while it thus rapidly unfolded itself, some of its smaller vessels, as I presume, were broken. The caution which this sort of accident indicates is, that when the grooved needle has ascertained that the effusion is serous, a *small* trocar should be employed, so as to allow the liquid to escape more gradually.

This patient recovered without a single delaying drawback; and I saw him lately, eight or nine years after the operation, a tall and stalwart man.

Secondly. Is the orifice to be healed up, or to be kept open?

Here, also, practical men differ. I should say, if pus come out, by all means make the aperture large, and keep it open; and inasmuch as detention of the pus would be injurious, and the depending point is difficult to hit, and the orifice is apt to clog, I would do more than leave it open: I would draw the puriform fluid off twice a day by a syphon: or, what in some instances may be more feasible and efficacious, I would make a second depending opening, and suffer the pus to escape continually, as fast as it forms, and so to be kept sweet and innocuous, through a *drainage tube.* For a detailed account of this contrivance, and for some examples of its successful employment, I must refer you to a joint paper by Dr. Goodfellow and Mr. Campbell De Morgan, in the forty-second volume of the *Medico-Chirurgical Transactions.*

If serum be let out, by all means close and heal the wound. Then, if all go on well, our object is achieved. But should the condition of the patient fail to improve; should hectic fever, after a day or two, set in or even continue; should much constitutional distress or disturbance arise;—under such circumstances I would reopen the wound. There *was* mere serum, or liquor sanguinis; there now *is*, in all probability, puriform matter pent up in the pleura; and even stinking and poisonous gases.

On six occasions I have myself witnessed the withdrawal, by puncture, from the human pleura, of a clear transparent liquid. Some of the patients were under my own charge, some under the charge of others. Of these six patients one died the day after the operation; I can scarcely say why. She was an extremely timid and susceptible young woman; and I am inclined to attribute her death to the shock produced, by apprehension of the operation, upon her sensitive nervous system. A patient of Dr. Bence Jones' had a large quantity of fluid drawn off. He was a strong man: but twenty-nine hours afterwards, when turning in bed, he suddenly died. The lung had not expanded. Two others of the six recovered forthwith, and perfectly. The wound presently healed in the three remaining cases also; but in one of the three it soon broke out again, and a quantity of healthy pus was discharged daily. After some time, the expedient of keeping the cavity free from accumulated pus by the use of a syphon was resorted to. Under this plan the discharge became gradually less and less, and at the end of many months it finally ceased. The side at one time was so shrunk in, and the ribs were drawn so closely together, that the introduction of the syphon became difficult. Ultimately this deformity was removed, and the symmetry of the chest restored. When the patient presented himself to me, after two years' residence upon the Continent, I found both sides of his thorax alike in shape and dimensions, and the pulmonary expansion everywhere audible and natural. This gentleman continues now (1870) in the enjoyment of good health; holding a high office in the Civil

Service. This was the case to which I just now referred as forming the second example that I have seen of the complete re-expansion of a side contracted after pleuritic effusion. When the operation was performed, thirty-one years ago, the patient was gasping, livid, faint, and on the brink of perishing by suffocation; it gave him instant and lasting relief. I have been told of a man who, for fifteen years, had a similar thoracic fistula; and who nevertheless, during nearly the whole of that period, was actively engaged in the various labours of a farm-servant.

I have still two of the six patients to account for. They were both much relieved by the operation for a while; but after a few days they again fell off; and after many more days of gradual sinking and distress, they died. The cavity of the pleura contained, in both cases, much puriform liquid, and a quantity of most offensive gas, consisting in great part, as I judged from its odour, of sulphuretted hydrogen. I have since thought that both these patients would have had a much better chance for life, if this corrupt and corrupting mass had been duly removed.

Again, I have twice seen *pus* let out, by the *primary* puncture of the chest. One of these two patients sank, exhausted, some months after the opening, which never healed, was made. The empyema of the other had been occasioned by fracture of a rib. The discharge continued for a short time, then ceased, the orifice closed, and the lad got well.

This constitutes the amount, or nearly so, of my personal experience of the operation of paracentesis thoracis. You will see, in the statement I have been making, the grounds of those opinions which I have formed and expressed respecting it. A full and final solution of the grave and difficult questions that it involves would require a much wider field of observation than any one individual is likely to command. Dr. Thomas Davies has published a tabular account of the several cases of operation which he had then superintended. In sixteen cases of empyema, so treated, there were twelve recoveries; that is, the operation was successful in three-fourths of the whole number of cases: a very encouraging result. In three of the less fortunate cases, the lung could not expand after the evacuation of the fluid, in consequence of the thickness of the false membranes covering it.

The value of Dr. Davies' table would have been greater, if it had shown in each case the time, after the commencement of the disease, at which the operation was performed; the symptoms that called for its performance; the nature of the liquid let out; and whether the orifice made by the trocar was closed or not.

The quantity of liquid which the distended pleura is capable of holding is enormous. I have seen upwards of a gallon let out at once. Dr. Townsend mentions the case of a patient of Dr. Croker's, in Dublin, from whose left pleura Mr. Crampton drew off the almost incredible quantity of fourteen imperial pints of pus. Of course this could not have accumulated there without making injurious pressure in all directions: upon the ribs, upon the heart and mediastinum, upon the diaphragm, and the abdominal viscera beneath it. It is interesting to know with what rapidity the capacity of the diseased side of the thorax may, in favourable cases, diminish. The same writer gives the history of a boy, twelve years old, in whom the circumference of the diseased side was sixteen inches and six lines, while that of the sound side was fourteen inches and one line. Nine days after the operation the circumference of the diseased side had decreased nearly three inches: it measured thirteen inches and nine lines; that is, rather less than the circumference of the healthy side. The side had shrunk somewhat within its natural size. This is common in such cases.

There is yet a third question of some importance. Whereabouts should the opening be made?

If any soft inelastic tumour have appeared, marking a tendency in the effused liquid to make its own way outwards, that tumour should be punctured without loss of time; for there will then be *no* chance of the reabsorption of the pus; and if the swelling be left to itself, troublesome, burrowing sinuses will be apt to form in the thoracic and abdominal parietes. As we have no choice in such a case about the place where the aperture is to be made, authors have termed the operation *the operation of necessity*; and they distinguish the case in which the surgeon is at liberty to introduce his trocar wherever he pleases; they say that then the *operation of election* takes place. Now the question is, what spot is the best for this operation of election?

If there be any part of the surface which is resonant on percussion, or which affords any sound of respiration, that part must be avoided. It is probable that the lung, in that place, is fastened by adhesions to the costal pleura. Of course you would not thrust in a trocar where you saw or felt that the heart was beating.

The object to be kept in view is that of making the opening in the situation which will allow the most free and perfect vent for the liquid. The intercostal space between the sixth and seventh true ribs, where the digitations of the serratus major meet those of the obliquus externus muscle, is the place usually recommended. Laennec prefers the space between the fifth and sixth ribs. He observes that, on the right side, an enlarged liver frequently reaches as high as the sixth, or even as the fifth rib. When the diaphragm is pushed as high as this (and I believe that the late Dr. Edwin Harrison, who had paid much attention to this point, could have told you that it is often pushed up even higher) there is an obvious risk of penetrating it with the trocar. In fact, Laennec committed that error himself. After making an incision between the fifth and sixth ribs, he thrust the instrument, as he supposed, into the thorax; and was a good deal surprised to find that no gush of liquid followed its introduction. The patient died; and dissection showed that the trocar had entered the cavity of the abdomen after transfixing the diaphragm, which, having been forced upwards by a large liver, had contracted firm adhesions to the seventh rib. I have myself witnessed a similar mischance, on the other side of the chest. The integuments of the side were œdematous; and it was thought that a little serum issued upon the passage of the grooved needle. The serum must have come from the infiltrated areolar tissue. No liquid was let out by the trocar. The patient died a day or two afterwards of peritonitis. The instrument had perforated the diaphragm, and entered the spleen, which was unusually large.

I am tempted to relate the particulars of one of the prosperous cases that I briefly adverted to before. It occurred in a lad of nineteen; a patient of my colleague's, Dr. Wilson. On his admission into the hospital he bore all the marks of copious effusion into the left pleura; the side enlarged, and motionless, and dull on percussion; the intercostal spaces tense, and level with the ribs; the heart beating to the right of the sternum; respiration puerile on the right side, inaudible on the left; urgent dyspnœa; a tendency to coma, marked by drowsiness and blueness of the cheeks and lips. In short, the boy was on the very verge of suffocation. He had been ill about a month; and had been bled, and cupped, and brought under the specific influence of mercury. Dr. Wilson judiciously directed that the liquid should be let out.

A grooved needle was first passed between the fifth and sixth ribs; and some serum following the puncture, a trocar was then introduced by Mr. Tuson, and nine pints of a clear fluid were drawn off. During the operation the patient became faintish at times, and then the orifice of the canula was stopped for a moment by the finger. The immediate effect of the tapping was most interesting and gratifying. Even while the liquid was flowing, the heart was observed gradually to move over from beneath the right mamma towards its natural situation; and his difficulty of breathing was signally relieved. At the beginning of the operation he respired fifty times in a minute; at its conclusion thirty-eight times only. A good deal of air entered while the liquid was escaping: and for some days after the operation a splashing sound was audible on succussion of the chest; and one part of that side was unnaturally resonant when struck, and another part unnaturally dull; and whatever was the posture of the patient, the hollow sound was uppermost, and the dull sound was undermost; and when he sat up and spoke, or coughed, a brazen resonance was heard by the ear applied to the scapular region. This lad got quite well, without the recurrence of a single bad symptom. He afterwards presented himself at the hospital; and I understand that the left side was found to be in a very slight degree smaller than the right.

The liquid that came out in this case was clear and transparent. It separated, on cooling, into three parts; one of quite watery consistence, one more viscid, and a third which constituted a soft, transparent, jelly-like mass of fibrin.

In this instance no injurious consequences resulted from the free admission of air.

It may sometimes be necessary to puncture the cavity for *mere* pneumothorax: when, for instance, the pulmonary pleura has been pricked by a fractured rib, and air passes from the lung into the pleural sac faster than it can be absorbed; fast enough to compress the lung, and to threaten death by apnœa. The diagnosis of such a state cannot be difficult. The existence of the fracture, the tympanitic sound yielded by the chest on the injured side, the absence of respiratory murmur in the tympanitic part, and the increasing dyspnœa, all point to the same conclusion. Now a trocar of the smallest size—or even an acupuncture needle—may suffice to give vent to the imprisoned *air*, which will escape with an audible hissing noise. In some cases it must have existed in very large quantity, for the stream of issuing air has been strong enough to blow out a candle several times in succession; the flame being each time immediately re-lighted.

The same necessity for puncturing the cavity of the pleura from without may arise in cases of pneumothorax depending on specific disease in the lungs: but we cannot regard the operation as *curative* in such cases. Its value is very different from that which experience has shown to belong to it in empyema from acute or chronic (but simple) pleurisy. Yet if it save life for the time, if it prevent impending suffocation, and relieve existing distress, and postpone the fatal event, it is not *without* its value: and it has many times been done, and been followed by very gratifying results; but it has never, that I know of, been followed by entire recovery. Dr. Davies had superintended the operation in nine instances of pneumothorax with effusion; and *all* the patients died from tubercular complications.

There are, indeed, on record, examples of recovery after the operation, when pneumothorax had existed, and under very unpromising circumstances. I should have stated before, that as the pus, in empyema, sometimes finds its way outwardly, penetrating between the ribs, and forming an external swelling, which, if not opened by the scalpel, will at length burst; so it also, sometimes, escapes by making a road into some part of the air-passages, and being expectorated. Now the operation of paracentesis, in such a case, *there being no tubercular disease*, has been successful. Le Dran relates an instance in which he operated for empyema, where 'the injection of a small quantity of mel rosarum and barley-water through the wound excited coughing, and part of it was coughed up through the mouth, mixed with pus;' thus clearly proving the existence of a fistulous passage through the lung; notwithstanding which the patient recovered completely. The effusion was probably circumscribed. But you will find other cases of a similar kind referred to by Dr. Townsend, in the *Cyclopædia of Practical Medicine*: and I may briefly mention one which has fallen within my own experience.

A gentleman, twenty-two years of age, had the ordinary symptoms of pleuritic inflammation. There had been no previous evidence of pulmonary disease. After a while he began to expectorate pus of a very offensive odour. The physicians in attendance upon him, Dr. Latham and Dr. George Budd, came to the conclusion that a circumscribed collection of pus in the pleural cavity had found its way, by ulceration, into the air-passages. The patient was gradually wasting away. Auscultation long failed to disclose the exact seat of the presumed collection of matter. At length, in a small spot between the scapula and the spine, on the affected side, the mingling of air with a liquid was heard during inspiration. As under the existing circumstances life seemed drawing inevitably towards its close, it was determined, in a consultation at which I was asked to assist, that an attempt should be made to let the matter out by puncturing the chest. The place at which the operation was of necessity performed rendered it a difficult one; but Mr. Stanley succeeded in reaching the abscess, and in drawing off nearly two pints of pus. The wound was kept open. From that time the offensive expectoration by the mouth ceased. Gradually the discharge from the back became less, until there was none. The opening healed in three months after the operation, which was performed in May, 1850, twenty years ago. The patient recovered perfectly, and is now a healthy man.

Since these lectures were last submitted to the press, I have witnessed several other cases in which paracentesis thoracis was deemed advisable. The subject has also been expressly treated of by Dr. Hughes and by Dr. Hamilton Roe. To those gentlemen the profession is much indebted for having shown with what facility, and with how little risk and pain, the operation may be performed. They have not convinced me of its frequent necessity. In so far as simple pleurisy and its consequences are concerned, my own opinion, after careful reconsideration, remains unchanged. The operation seems more extensively applicable, however, than I had formerly supposed, to other cases, where its object and effect are to alleviate present suffering, and to prolong life which it is unequal to save: cases of pneumothorax, and of serous or puriform effusion connected with malignant or tubercular disease of the lungs. But in mere pleuritic effusion I would not puncture the chest, however devoid of hazard that procedure may be, unless I knew that the contained fluid was pus, or unless there was no prospect of recovery without it. I except here the minor diagnostic puncture with the grooved needle. In one very interesting case, related by Dr. Hughes in the eighth volume of the *Guy's Hospital Reports*, the pneumothorax ceased after—it may fairly be said was cured by—the second evacuation of the affected pleura by tapping the chest. The air must have escaped from a small cavity in the lung, which afterwards rose and became adherent to the costal pleura. The patient died from rapid tubercular disease of the other (the left) lung. The right being removed, and submerged in water, not a bubble of air could be made to issue from it by inflation. Air had been absent from the pleural cavity from July 24th to May the 22nd of the following year.

This concludes what I have to say, not only of pleurisy, but also of pneumothorax, and of empyema, which are often treated of as separate and independent disorders. They are more frequently connected with pleurisy than with any other form of disease, and they are almost always *consequences* of disease or of injury. But I believe I have omitted nothing of importance with respect to either of them.

LECTURE XLVIII.

Pulmonary Hæmorrhage: its varieties; its connexion with pulmonary consumption, and with disease of the heart. Pulmonary Apoplexy. Prognosis in Hæmoptysis. Symptoms. Treatment.

Having gone over the *inflammatory* affections of the organ of respiration; having brought before you inflammation of the membrane which *lines the air-passages*, or *bronchitis*; inflammation of the membrane which *invests* the lungs, or *pleurisy*; and inflammation of the *whole substance* of those organs, or *pneumonia*; I proceed next to the subject of *pulmonary hæmorrhage*.

You may remember that, in an early part of the course, I drew your attention to some general facts respecting internal hæmorrhages. I showed you that the blood does sometimes proceed from a ruptured vein or artery, but that it is much oftener poured forth through what seem unbroken surfaces, from a multitude of capillary vessels; and that hæmorrhage of this kind takes place from the *mucous membranes* far more frequently than from any other natural surface of the body. I observed also that such hæmorrhage is almost always preceded by congestion; either by active congestion, which is less common, or by passive and mechanical, which is extremely common: and we speak, accordingly, of active and passive hæmorrhage. Hæmorrhage is also sometimes primary, or idiopathic, and then constitutes the whole disease; while at other times it is merely a symptom, direct or indirect, of some other disorder, in which case we call it secondary.

Now in the lungs we find examples of all these varieties of internal bleeding; but pulmonary hæmorrhage is secondary much more often than it is primary.

In speaking, therefore, of some forms of pulmonary hæmorrhage, I must touch upon certain diseases of which the bleeding is a symptom: but I shall not go further into the consideration of those diseases at present, than may be necessary to elucidate the hæmorrhage. Bleeding from the lungs is a thing of most fearful interest; and it will be useful to take a general view of that phenomenon, whether it be a substantial disease in itself, a cause of consecutive disease, or a sign of other pre-existing diseases.

The blood, then, in pulmonary, as in all other hæmorrhages, may issue through a breach in the walls of some considerable blood-vessel; or it may proceed from innumerable capillaries distributed to the mucous membrane of the bronchial and pulmonary air passages: and the latter mode of hæmorrhage is much the more common of the two, although it is the popular belief that the 'breaking of a blood-vessel in the lungs' is of very frequent occurrence.

The particular vessels injured in the first class of cases, and the nature and origin of the breach made in their sides, are matters of infinite variety. Sometimes the blood is extravasated through apertures, the results of a disorganizing process which has commenced in the coats of the vessels themselves; as when, for example, aneurisms of the thoracic aorta, or of its primary divisions, burst, and pour their contents into the air-tubes. Having pointed out this accidental and well-nigh hopeless form of pulmonary hæmorrhage, I shall postpone any further account of the disease that gives rise to it, to a future lecture.

Sometimes, again, a large blood-vessel is laid open by the encroachment and extension of disease from contiguous structures. Here is represented (Carswell, fasc. vi. plate iii. fig. 5) the perforation of a large branch of the pulmonary artery, and of a neighbouring bronchial tube, by the extension of tubercular ulceration. The blood escaped so abundantly in this case, that the patient was dead in less than a quarter of an hour. And here I show you a preserved specimen of a similar opening made in the pulmonary *vein*.

It will be necessary that I should anticipate somewhat; and in order to include in one view all that relates to pulmonary hæmorrhage, that I should speak cursorily of its connexion with pulmonary phthisis. No one here can be ignorant that in that terrible disease portions of the lungs are liable to be hollowed out by the softening and expulsion of tubercular matter, or by some other form of destruction of their tissue, into what are called *vomicæ*. Now seeing that hæmoptysis occurs very frequently in persons labouring under consumption, and that the expectoration of blood is often copious, and takes place when it is evident that there are tubercular or other excavations in the lung, it would be very natural for you to suppose that the bleeding in such cases proceeded from large vessels which had been laid open during the softening of the tubercles, or by the subsequent extension of the ulcerating cavities. But in point of fact, this is very *rarely* the case. I shall explain to you hereafter how it happens that this hæmorrhage from the larger vessels is *generally* prevented; still it does *sometimes* happen.

But in a far greater number of instances the blood in hæmoptysis is *poured forth* from the mucous membrane that lines the air-passages. When this surface has been examined in the dead body, immediately after the occurrence of pulmonary hæmorrhage, it has often been found to be perfectly entire, from the commencement of the trachea to the remotest divisions of the bronchial tubes; as far, at least, as minute dissection can follow them. The membrane in these cases is usually red, as in simple bronchitis; but it is sometimes pale, or shows scarcely any traces of vascularity. The former of these appearances results from the continued turgescence of the submucous capillary vessels; the latter is the consequence of their having been completely emptied of blood by the last hæmorrhage. We shall meet with analogous conditions when we come to examine the hæmorrhages that proceed from *other* mucous surfaces; and especially from that of the alimentary canal.

When blood is thus discharged from the mucous membrane of the air-passages, the hæmorrhage may be strictly primary or idiopathic; i.e., it may be independent of any discoverable alteration of texture, either in the mucous surface itself, or in any other part which, by reason of some intelligible connection of structure or relation, seems capable of influencing the capillary circulation of the membrane. Active pulmonary hæmorrhage is stated by systematic writers to be the hæmorrhage of adolescence, as epistaxis is that of childhood. There are even stronger *à priori* grounds for expecting idiopathic bleedings from the mucous membrane of the lungs than from that of the nostrils. 'The lung (says Dr. Johnson) is by far the most vascular organ in the body. In addition to its own nutrient bronchial vessels, the whole of the blood from every other organ passes through the pulmonary capillaries. These two systems of vessels in the lung, in consequence of their relation to each other, to the heart, to the movements of the chest, and to the function of respiration, are more liable to sudden strain and pressure than the blood-vessels of any other organ; and it would be indeed marvellous if bleeding did not frequently occur from their accidental rupture, without previous disease.'

Cæteris paribus, the *disposition* to pulmonary hæmorrhage is increased by whatever tends to diminish the capacity of the thorax, and to compress the lungs, or the heart and great blood-vessels. The mechanical congestion thus produced may become a very intelligible cause of the exudation of blood from the mucous membrane. And it is partly on this principle that we may account for the frequency of hæmoptysis in persons with crooked spines; in tailors, who sit continually in a stooping posture; in young women who lace their stays too tightly; and even in those who labour under dropsy, or other cause of distension of the belly. Hæmoptysis accompanying ascites has been known to cease at once upon the performance of the operation of tapping, and to recur upon the re-accumulation of the dropsical fluid; and this not on one occasion only, but so often and regularly as to preclude all notion of accidental coincidence. There can be little doubt, however, that in this class of cases, or at least in a vast majority of them, the hæmoptysis is mainly to be ascribed to organic disease of the heart or of the lungs; and that the pressure which precedes and determines the bleeding is simply a *concurrent* cause.

If we cannot properly rank *that* pulmonary hæmorrhage as idiopathic, which is constitutional and vicarious of some other natural or morbid discharge,—and most frequently of all of the menstrual discharge in females,—it may be considered as forming a link of connection; as lying midway between secondary and primary hæmorrhages. There are a great number of very curious and well-authenticated facts upon record concerning this singular form of hæmorrhage by deviation. I will give you one history of the kind by way of sample; it is related by Pinel, who held that there was no supplemental hæmorrhage more common than the hæmoptysis that is vicarious of menstruation.

A female, 58 years old, born of healthy and robust parents, of strong constitution, of a sanguine and plethoric temperament, and of great sensibility, had lived in the Salpetrière, and had therefore been under constant observation, from the age of 14. She enjoyed excellent health till she was 16 years old. In her 16th year the menstrual discharge commenced without mishap or difficulty; but this, her *first* menstruation, was suddenly suppressed, in consequence of the fright and agitation produced by the sight of an epileptic patient in strong convulsions. From that time the catamenia *never* reappeared, nor did any kind of discharge take place from the genital organs; but at the next period, when regular menstruation ought again to have come on, the girl was attacked with violent hæmoptysis. The hæmorrhage was preceded by vague pains in the uterus and loins, and by other symptoms which frequently announce the catamenia. It lasted two days, during which time she expectorated nearly a quart of blood. With one interval only of exception, this woman continued to menstruate *through her lungs* at each monthly period, from her 16th to her 58th year, *i. e.*, during 42 years of her life. The coming on of the hæmorrhage was sometimes a little accelerated by strong mental excitement; sometimes a little retarded by causes of a contrary nature. It was suspended during one whole year, without any serious impairment of the general health, or the occurrence of any other hæmorrhage: during this interval, however, the patient suffered most severe headaches. Occasionally the hæmoptysis was complicated with hæmatemesis. The symptoms by which the pulmonary hæmorrhage in this instance was generally preceded or accompanied were the following:—a sensation of weight and uneasiness in the loins and in the situation of the uterus, soon followed by chilliness of the surface, general lassitude, and a feeling of oppression and heat in the chest, with some dyspnœa. The face became red, and she had intense headache. Then she began to have a distinct sensation of pricking, and of a sort of bubbling, in the trachea and about the commencement of the bronchi; then followed sharp cough, and the expectoration of blood, often bright coloured and frothy, sometimes of a darker hue. The duration of the hæmoptysis was generally confined to a single day, and it never exceeded three days. It recurred with tolerable exactness at monthly periods; sometimes the interval was longer, and then the *hæmorrhage* continued longer, but was less abundant; and upon the whole, about the same quantity of blood was lost on each occasion. This woman continued plump, and otherwise healthy, though liable to some thickness of the breath upon unusual exertion.

Cases of this kind are not at all uncommon; although the vicarious hæmorrhage seldom persists so long and so steadily. They are not usually attended with much peril. At the 'change of life' when menstruation comes naturally to an end, the hæmorrhage also ceases for ever.

It is, however, a melancholy truth, that capillary hæmorrhage from the mucous membrane of the air-passages, is dependent, in a very large proportion of instances, upon incurable disease. The hæmorrhage is secondary; and the disease of which it is symptomatic is usually a fatal disease. The complaints of which hæmoptysis is by far the *most frequently* symptomatic, are tubercular phthisis, and organic disease of the heart. Trousseau holds that during youth, adolescence, and the first epoch of mature age—from 16 to 40—secondary hæmoptysis is generally dependent upon tubercular disease of the lung; after 40, and still more after 50, upon disease of the heart. When death follows hæmoptysis, and upon dissection we find tubercles still crude and entire, without visible breach in the membrane, or any other diseased condition, we can entertain no doubt about the source and manner of the bleeding; and even when cavities exist, especially if they are found to contain no blood, it is probable that, in most cases, the hæmorrhage has had a similar origin.

When hæmoptysis is thus actually symptomatic of tubercular disease of the lungs, it is liable to considerable variety in regard to the period of its first occurrence, and the symptoms by which it is succeeded. There are many persons in whom the first attack of hæmoptysis precedes, even for years, the primary symptoms of unequivocal phthisis. There are others in whom the first attack of hæmoptysis is *immediately* followed by all the signs which announce the presence of tubercles in the lungs. Many, again, do not spit blood until the lung disease has acquired a considerable degree of development, and the phthisical symptoms have been for some time clearly marked; and now and then, in these cases, the first hæmorrhage proves fatal. Lastly, it is far from being an uncommon thing to see pulmonary consumption run its whole course, and terminate in death, without having been attended with *any* spitting of blood.

Andral gives the following statement as the result of his own observation, in regard to the relative frequency of these several modes of connection between hæmoptysis and consumption.

Of the persons whom he had known to die of that disease, one in six never spat blood at all. Three in six (or one-half of the whole number) did not spit blood until the existence of tubercles in the lungs was already made certain by unequivocal symptoms. In the remaining two-sixths the hæmoptysis preceded the other symptoms of pulmonary consumption, and *seemed* to mark the period of its commencement.

By this comparative statement you will see how very frequently hæmoptysis occurs as one of the symptoms *connected* with phthisis. Under this physician's observation it happened in five cases out of six. In the experience, however, of M. Louis, the proportion, though very large, is not quite so great as Andral found it. Among eighty-seven instances of consumption, there were fifty-seven, or four of every six, in which hæmoptysis had been present.

Next to *tubercular* disorganisation of the *lungs*, the most common source of secondary pulmonary hæmorrhage is to be found in organic disease of the *heart*; and the disease, in almost every instance, is situated on the *left* side of the heart. Most frequently of all it consists in some morbid condition that has maimed the function of the *mitral valve*: some change which directly hinders the passage of the blood into the left ventricle, or which allows a portion of the blood to be forced backwards into the left auricle by every contraction of the corresponding ventricle. In either of these cases, blood accumulates in and behind the pulmonary capillaries, there is increasing mechanical congestion and distension of the pulmonary bloodvessels, and at length the exudation of blood and hæmoptysis.

It has been stated indeed by Chomel, Bouillaud, and others, that disease of the heart, productive of hæmoptysis, is most often seated in its *right* chambers. But that is plainly a mistake. Such a relation between cardiac disease and pulmonary hæmorrhage must, in truth, be very uncommon. You will call to mind the fact that in the lungs there are two systems of bloodvessels: the pulmonary proper, which brings venous blood and carries back arterial, in fulfilment of the special function of the lungs; and the bronchial, which brings arterial and returns venous blood, being the nutrient apparatus of the lungs, and forming a part of the general circulation. Incompetency of the tricuspid valve, permitting a reflux of the blood during every contraction of the right ventricle, would tend, by distending the bronchial veins, which empty themselves into the superior cava, to produce congestion and possible rupture of the bronchial capillaries. Strictly speaking, hæmoptysis so caused should be called *bronchial* hæmorrhage. A case of this kind has been recorded by Dr. Johnson. The only other alteration in the right chambers of the heart which could be supposed likely to cause pulmonary congestion, and thereby hæmoptysis, would be increased strength and thickness of the walls of the right ventricle: hypertrophy: a morbid condition which is comparatively rare on that side of the heart, and which, probably, would not suffice for the production of hæmoptysis, even did it oftener exist.

It is necessary that I should here notice one very remarkable condition of the lungs, which is often directly connected both with pulmonary hæmorrhage and with cardiac disease; though it is not *always*, or *necessarily*, associated with either.

The morbid state to which I allude is far from being infrequent; yet it had been scarcely noticed by or known to pathologists, until Laennec described it under the title of *pulmonary apoplexy*. It appears under two forms. In the one form we find an uncertain number of hard knobs, or compact masses, situated here and there in the substance of the lungs, chiefly in their lower lobes, and towards their posterior surface. Their size varies from that of a marble to that of a hen's egg. When cut through they are seen to be very exactly circumscribed, the cut surface being more or less circular, of a uniform and very dark colour throughout, and exhibiting a strong contrast with the surrounding tissue. Careful examination shows that these masses are composed of blood that has coagulated in the pulmonary vesicles. *Occasionally* the pulmonary substance seems broken down, or torn, by the extravasated blood; and in these cases, perhaps, the resemblance between the injury done to the lung, and that which is inflicted on the substance of the brain in *cerebral* hæmorrhage, is tolerably close. Generally, however, there is no such laceration of the pulmonary tissues; but one, or more, of the *lobules* of the lungs, are gorged and crammed with blood, which has been poured out from the surface of the mucous membrane. These lobules, it is well known, have no direct communication with each other; but are isolated (except where they severally open into the bronchial tubes from which they spring) by a distinct investment of areolar tissue; and it is to this peculiarity in *their* structure and disposition, that the exact circumscription of the dark-red indurated masses is to be attributed.

In the other form of pulmonary apoplexy, there are fewer of these solid spots; perhaps one only, large, diffused, occupying sometimes nearly the whole of one lobe, its limits obscurely defined, and its colour gradually deepening to the centre, which is obviously formed by little else than a black clot of blood.

Now the principal *symptom* attending the formation of these masses is *hæmoptysis*; and the principal though not the only *cause* is disease of the heart. The hæmorrhage is often severe and copious in the first or circumscribed form: sometimes slight and scanty, but commonly slow, oozing, and persistent, in the second or uncircumscribed form. The heart-disease is in its left chambers, most often at their communication through the mitral orifice. No example of pulmonary apoplexy, or of pulmonary hæmorrhage, even *apparently* dependent upon hypertrophy of the *right* side of the heart, has ever fallen under my notice.

In truth, that morbid condition of the lung which I am now speaking of, has been badly named. The application, by Laennec, of the term *apoplexy* to the *lungs* was singularly unfortunate: for it suggests an analogy between two things, which, though resembling each other in the appearances which they leave behind them in the organ affected, are yet, essentially, unlike. I have shown you, in a previous part of the course, that cerebral hæmorrhage depends almost always upon the giving way of a blood-vessel, in consequence of the morbid brittleness of its coats: while what is called pulmonary apoplexy can seldom be so caused. The notions which I have been led to form upon this subject differ materially from those which you will find expressed in the works of almost every writer on pulmonary apoplexy. The opinions I entertain were stated many years ago, in some lectures which I was appointed to deliver before the College of Physicians; and I have constantly been in the habit of mentioning them to the students at the Middlesex Hospital, and to my medical friends. It is a matter of satisfaction to me to find that they were esteemed to be correct by so sound a pathologist as Sir Robert Carswell, who has alluded to them in one of his Fasciculi on the *Elementary Forms of Disease.* Laennec speaks of the pulmonary apoplexy, as if *it* were the *cause* of the hæmoptysis. But this is surely a very incorrect view of the matter. The partial engorgement, and the hæmoptysis, are not mutually connected with each other as cause and effect, but they are *concurrent* effects of the *same* cause; of that cause, whatever it may be, which gives rise to the extravasation of the blood in the first instance. A part of the blood so extravasated passes outwards by the trachea and mouth; while a part is forced in the contrary direction, into the ultimate divisions of the bronchi, so as to fill and block up the whole tissue of a single lobule, or of a bunch of contiguous lobules, and thus arises the *circumscribed* variety. Andral conceives that the sanguine effusion *takes place in* the ultimate air-cells; and this may perhaps be the true pathology of the *uncircumscribed* variety. But it seems to me vastly more probable that in the other form of the complaint the effusion proceeds from one or more of the larger branches of the air-tubes; and that the blood, a part of it at least, is driven backwards into certain of the pulmonary lobules, by the convulsive efforts to respire which the patient makes, when threatened with suffocation by the copious explosion of blood, or by a paroxysm of cough and extreme dyspnœa: especially if the blood be poured forth from the membrane while the chest is in the state of *ex*piration. It is easy to understand how certain portions of the lungs, without undergoing any actual change of texture, may in this manner be so choked up, and crammed with blood, which afterwards coagulates, as to preclude any subsequent admission of *air*.

This view of the formation of circumscribed pulmonary apoplexy, affords an easy explanation of some of the phenomena attending it, which it would be difficult to account for on any other supposition. I mean, first, the occurrence of *several* of the clots, or masses of blood, in different, and sometimes in distant, parts of the lung at the same time; and secondly, the exact manner in which they are commonly bounded and limited to certain lobules. And if (as is sometimes, though seldom, the case) even the texture of the lung be lacerated, it is easier to conceive that this may happen in consequence of the violence of regurgitation during the struggle of impending suffocation, than that the mere impulsion of a thickened muscle at the centre of the circulation should be capable of driving the blood through the walls of an artery with sufficient force, to tear and break down the substance of the lung around it.

In the diffused or uncircumscribed form of pulmonary apoplexy, the congested lung is not relieved by a sudden and copious gush of hæmorrhage, but the impeded and stagnating blood oozes slowly from the vessels containing it into the neighbouring interstitial and vesicular tissues, clogs up a larger and larger space, and is partly expectorated in separate dark-red sputa, combined it may be with a certain quantity of mucus. A similar condition of the lung sometimes occurs in *purpura hæmorrhagica*, without any mechanical impediment to the passage of the blood into and through the heart.

The belief that the dark-coloured, circumscribed spots seen in the lungs, and spoken of as pulmonary apoplexy, are often, if not always, produced in the manner I have just been describing, was suggested to me by the observation of a case, in which these appearances existed, and in which they *certainly were* so occasioned. I told you, when speaking of cynanche tonsillaris, that I had seen one person, and one only, die of that complaint; and that his death was occasioned by the laying open of the lingual branch of the carotid artery in the progress of ulceration. The phenomena attending the patient's dissolution were of deep, though of painful interest. I described them to you before. He had been taken out of bed, and laid upon a table in the ward, in the middle of the night, in order that Mr. Mayo might more conveniently place a ligature upon the carotid. Suddenly the bleeding burst forth afresh; and he expired, before our eyes, in the course of two minutes: not from syncope or exhaustion, but evidently suffocated. The blood entered and choked up the trachea, and he had not strength enough left to expel it by coughing. I felt his heart and the artery at his wrist pulsate firmly for some little time after the last attempt to dilate the chest had been made. This you know is what always happens when death takes place from the sudden denial of air to the lungs. We found the upper surface of the glottis covered by a clot of blood. There was blood also *in* the windpipe; and scattered through the substance of the lungs there were numerous hard, and dark, but not very large masses, precisely resembling those described by Laennec as constituting pulmonary apoplexy. This man had shown no symptoms of any pulmonary complaint; nor was there any morbid appearance in his lungs except those which resulted from the presence of the blood that had been poured into them through the trachea, and rammed home into some of the air-cells, in his convulsive attempts to breathe. All that I have observed since this case happened, has tended to confirm my belief, that what has been erected into a distinct form of disease, under the objectionable name of *pulmonary apoplexy*, is simply an *accident of pulmonary hæmorrhage*. When hæmoptysis has occurred, to any amount, in *consumption*, it is by no means uncommon to find pulmonary apoplexy after death; and Dr. Latham has mentioned to me, in conversation, one remarkable instance of that disease, strongly corroborative of the doctrine I have been endeavouring to support. A young female patient of his, labouring under confirmed phthisis, was attacked, for the first time, with hæmoptysis. The bleeding was so profuse as to cause almost immediate death. Her lungs were found riddled with small tubercular cavities; and each of these little cavities contained a little clot of blood. Surely it is more credible that the blood should have reached each cavity by regurgitation from the larger air-tubes, than that each should have been the seat of an independent hæmorrhage at the same moment. It is by a similar reflux of blood that the appearances are produced which characterise the circumscribed form of pulmonary apoplexy.

It has been made a question, and it is a very important question, whether the spitting of blood which occurs in *connection* with tubercular phthisis, is always indicative, as Laennec supposed it to be, of the preoccupation of the lungs by tubercles; or whether it may not sometimes precede, and give occasion to, their formation in those organs. This question has evidently been suggested by those cases (constituting, according to Andral, one third of all that happen) in which the ordinary signs of phthisis *begin* to manifest themselves immediately upon the occurrence of the first hæmoptysis, or within a short time afterwards. Morton, who has noticed this kind of pulmonary hæmorrhage, includes among his *species* of phthisis, the 'phthisis ab hæmoptöe;' and Cullen held that *spitting of blood* was *often* the *cause* of pulmonary consumption.

Laennec's opinion on this point has, since his time, been generally prevalent among physicians. But Dr. Niemeyer has recently revived the doctrine of Morton, Cullen, and other older writers; and maintains that 'in a very great majority of the instances in which the first attack of hæmoptysis has not been preceded by either cough, dyspnœa, or other signs of pulmonary disorder, the lungs are free, and by no means the seat of tubercular deposit, at the commencement of the bleeding.' He holds that bronchial hæmorrhage occurs oftener than is generally believed in persons who are not consumptive at the time of the bleeding, and who never become so. He asserts, moreover, that 'bronchial bleeding may precede and *cause* the development of consumption, by producing chronic inflammation and destruction of the lung: and that, in the same way, it may accelerate the fatal issue of pre-existing consumptive disease.'

These views have been more or less fully adopted and avowed by able pathologists in this country, by Drs. Johnson, Baümler, and Hermann Weber.

I have myself carefully and anxiously watched very many cases of hæmoptysis. Almost always, after the cessation of the hæmorrhage, I have found crepitation in some part or parts of the lungs; crepitation denoting, as I concluded, the mingling and passage of air in portions of blood that still lingered in the air-passages. In a few days this symptom and sound has commonly disappeared.

Still I cannot doubt that a part of the effused blood is often retained more permanently in the lungs. It must be so in those cases which fall under the head of pulmonary apoplexy. It is Niemeyer's belief that blood so retained may become a source of irritation, inflammation, and other degenerative mischief in that part of the lung which it occupies. If *tubercular* disease is ever a direct consequence of such retention, there must, in my judgment, have been a constitutional predisposition to tubercular deposit. The diffused form of pulmonary apoplexy may be expected to result in further disorganisation of the lung.

Upon the whole, the occurrence of hæmoptysis, considered in reference to the probable duration of life in those who are the subjects of it, is of melancholy omen.

Making due allowance for a limited number of cases in which decided hæmoptysis is neither the effect nor the cause of organic mischief, I deplore my inability to alter the opinion which I have long held and expressed:—that if from any given number of persons who have been known to spit blood, we subtract those in whom that symptom was connected with irregularity in the uterine functions, there will remain but few in whom the hæmoptysis did not depend upon disease, incurable and progressive in its nature, of the lungs, or of the heart; and that if we still further subtract those persons in whom the hæmorrhage was symptomatic of cardiac disease, there will be very few indeed left, in whose lungs the existence of tubercles, or other fatal damage, may not be confidently predicated.

Among these few may be reckoned persons who have suffered hæmoptysis dependent upon the detachment and expulsion of 'bronchial polypi;' and who exhibit no other indication of cardiac or of pulmonary disease.

You will, of course, understand that I do not include in this estimate of hæmoptysis as a prognostic symptom, those cases in which (as in simple bronchitis) the expectoration is merely *streaked* with blood:—nor those in which small quantities of blood are intimately combined and amalgamated with the bronchial mucus, and form the rust-coloured sputa so indicative of the presence of pneumonia:—nor those in which pure blood sometimes wells up into the mouth in the preliminary engorgement of the same disease:—nor those in which the hæmorrhage is a consequence of mechanical injury to the chest.

Of those individuals whom Andral had known to spit blood at some period or other of their lives, there was only one in five whom he did not also *know* to have tubercular phthisis. On the other hand, Louis states that for upwards of fifteen years he asked all the patients who came before him, in the practice of a large hospital, and who were *not* affected with phthisis, whether they had ever spat blood; and the answer was always in the negative, excepting only a few instances in which the patients had received violent blows upon the thorax; and the cases of females in whom the menstrual discharge had been suddenly suppressed.

Niemeyer himself says of the prognosis in pulmonary hæmorrhage that in respect of immediate danger to life, it may, on the whole, be pronounced favourable—in respect of complete recovery, exceedingly bad.

The quantity of blood which is brought up in different cases of pulmonary hæmorrhage is extremely variable. Sometimes it is so copious and overwhelming that either the patient dies suffocated, or he dies of syncope, outright; but this is not very common. Sometimes, on the other hand, a small quantity of blood finds its way into the mouth, the patient scarcely knows how. And between these two extremes there is every gradation in regard to quantity.

'When blood is thrown out by the mouth (says Cullen), it is not always easy to determine from what internal part it proceeds; whether from the internal surface of the mouth itself, from the fauces, or adjoining cavities of the nose, from the stomach, or from the lungs. It is, however, very necessary to distinguish the different cases.'

Now the diagnosis between hæmorrhage from the lungs and hæmorrhage from the stomach, in other words, between *hæmoptysis* and *hæmatemesis*, I shall not enter upon, until I have described the latter disease. And the diagnosis between hæmorrhage from the fauces or cavity of the mouth, and hæmorrhage from the lungs, can never be very difficult, if once the doubt suggest itself, and the necessary examination of the mouth be made. And I would advise you not to omit that inspection. I could tell you of cases in which the neglect of this simple precaution has led to needless activity of treatment, and to the ultimate discredit and disadvantage of the practitioner. Blood may ooze into the mouth from spongy gums, or drip from the posterior nasal orifices, and be at length spat out in considerable quantity. Etymologically speaking, these are cases of *hæmoptysis*; but they do not constitute the particular disease or symptom to which alone nosologists have agreed to restrict that term. The sources of the bleeding are manifest as soon as they are carefully looked for.

Patients who are subject to hæmoptysis generally know by experience when it is about to happen. It is frequently preceded by some uneasy feeling within the thorax—pain, or a sense of weight, or of heat, or of pricking, beneath the sternum, with anxiety; and they tell you that they taste the blood in their mouths before it comes up, *i.e.* they perceive a saltish taste; and just before the blood appears, a tickling sensation is experienced about the top of the larynx. To relieve this sensation, the patient coughs or hawks a little, and a certain quantity of frothy and florid blood is expectorated.

In a person disposed to pulmonary hæmorrhage, the bleeding may be determined by a variety of causes; which ought to be pointed out to him, in order that he may avoid them. Anything which hurries the circulation will, of course, have a tendency to excite the hæmorrhage. Straining of any kind; great efforts of the body; active exercise; much talking; and more especially public speaking, or singing, or playing on wind instruments. A diminution in the superincumbent weight of the atmosphere is supposed to be, in some cases, sufficient to bring on hæmoptysis; and blood is said to have been forced even from sound lungs, in persons who have ascended very high mountains, where the atmosphere is rare, and where the pressure upon the surface of the body is sensibly lessened. Perhaps the labour of the ascent may have shared in the production of the hæmorrhage; for I am not aware that any such effect has ever occurred to persons who have much more rapidly reached a very great altitude in balloons.

Auscultation and percussion do not stand us in much stead in cases of hæmoptysis, so far as that *symptom itself* is concerned. Indeed, if they were capable of affording us information, it would in most cases be superfluous; for we *see* the blood, and we can generally satisfy ourselves that it comes from the *lungs*.

But pulmonary hæmorrhage may occur without hæmoptysis. In pulmonary apoplexy there is extravasation of blood: but it is not always attended with the expulsion of a portion of the extravasated fluid through the mouth. Laennec and others pretend to say, that when there is blood in the bronchi, they can distinguish, by the peculiar character of the crepitation to which it gives rise, that it *is* blood, and not mucus; that the bubbles, passing through a thinner liquid, are larger, and break oftener, than those produced by the passage of air through viscid mucus. This distinction is too subtle for me. If, indeed, there have been hæmoptysis, and especially if the hæmoptysis have been sudden and copious, and if, after it, you hear large crepitation in one or more isolated parts of the lung, it will be reasonable, as I just now observed, to conclude that the air-tubes contain blood in those parts. Those lobules that are plugged up with blood, to the entire exclusion of air, will not, of course, be the seat of any sound during respiration ; but this limited absence of sound will be scarcely appreciable unless the impacted portion lies near the surface of the lung. Around the spot thus rendered solid the sound of crepitation may, however, be audible.

Yet, although the method of auscultation furnishes but little help towards the *detection* of pulmonary hæmorrhage, it will often afford us most precise and valuable information respecting the disease of which the hæmorrhage is a consequence, and an index. Thus, it will frequently teach us, with absolute certainty, that the heart is diseased, or that the lungs are occupied by tubercles, or otherwise disorganised. The precise sounds, or deficiencies of sound, which supply the key to this knowledge, I shall describe when I come to those disorders.

Whatever may be the source and organic cause of the hæmoptysis, the bleeding should be stopped as soon as possible ; not, however, merely by *suppressing* it, but by relieving the necessity on which it depends. The longer it is suffered to continue, the more likely is it to add to the damage, which already, in too many cases, exists in the lungs. If it lead to the formation of blocks of pulmonary apoplexy, the portions of lung so filled up are rendered useless for a long period, and probably for ever ; or they may become the seat of further degeneration. Now as in most cases the hæmorrhage is a capillary hæmorrhage, and depends upon congestion, active or mechanical, we shall stay the hæmorrhage if we remove the congestion.

The congestion may be either mechanical, or active, when it results from the presence of tubercles in the lungs ; it is almost always mechanical when it depends upon disease of the heart. The tubercles may press upon the blood-vessels and so lead to mechanical engorgement ; or they may provoke by their presence an active determination of blood to those organs, just as we know that they may provoke inflammation, which is congestion and something more ; and just as any foreign body lodged in the lung may cause either the one or the other of these conditions.

Sometimes there is a distinct febrile movement accompanying the hæmorrhage: the heart beats with increased force and frequency, the cheeks are flushed, and the skin is hot; the pulse hard, and full, and bounding, and people speak of such a pulse as a *hæmorrhagic* pulse. Now I mentioned in a former part of the course, that hæmorrhage occurring under such circumstances as these often works its own cure; but it is better, when an organ so vital and important as the lung is the seat of the effusion of blood, that *we* should cure the bleeding than that it should cure itself—that we should diminish the congestion with which it is linked, through the safer channels afforded by the capillary blood-vessels of the exterior of the chest. The patient should be surrounded with cool fresh air. His head and shoulders should be elevated. He should be strictly enjoined to refrain from all exertion; should neither move, nor speak, nor cough, more than may be absolutely inevitable. It may be expedient to appease any importunate tendency to coughing by a small opiate. He should be restricted to a meagre diet, and be supplied with morsels of ice to melt in his mouth, or with iced acidulous drinks. His bowels should be freely purged, in the first instance, and then *kept* lax and open, both with the view of deriving (as it is called) from the thorax, and of preventing costiveness and straining. And, in conjunction with these measures, it may be requisite that he should lose blood from the surface of the thorax.

A prejudice has indeed been taken (such, at least, I deem it) against *local blood-letting* in pulmonary hæmorrhage. Inasmuch as leeches applied to the groins in amenorrhœa appear sometimes to restore the catamenia, so they have been thought likely, when applied over the surface of the chest, to attract the blood somehow to that part of the body, and even to *cause* hæmorrhage when none before existed. Now I have so many times taken blood from some part or other of the exterior of the thorax by leeches, or by cupping-glasses, without observing any such effect, of causing hæmoptysis, or of increasing it while already present, that I cannot but regard the objection as a fanciful one. Nay, I am quite sure that very great relief to the congested and oppressed lungs may often be obtained by the rapid removal of blood from the peripheral vessels of the chest:—by cupping the patient freely between the shoulders, or upon the breast. In secondary hæmorrhage from the lungs, in that especially which is connected with tubercular disease, topical blood-letting thus performed is more effectual at the time, and far more safe in the end, than venesection, repeated, as was once the fashion, from day to day, until the hæmoptysis ceases to recur.

When the fever and congestion are abated; or when there has been no constitutional disturbance, and the hæmorrhage has shown a passive character from the beginning, and when the further loss of blood, so far from being curative in its tendency, is likely to be injurious; then we are to employ those remedies which have been found efficacious in restraining and suppressing hæmorrhages.

Now, among the substances which are held to possess more or less of a specific virtue, when taken internally, in arresting the efflux of blood, the sugar of lead, the *plumbi acetas*, enjoys in this country a high reputation. And it certainly is a very serviceable remedy. Dr. Paris speaks of it as one of the most valuable resources of physic: and says that in respect to its power over internal hæmorrhage there is nothing *simile aut secundum.* He states also that its use is equally safe and manageable. There is, in fact, no doubt of its efficacy: but most other writers use very cautious language in recommending its employment. Physicians have been deterred from giving it by the fear of its poisonous qualities; by the dread of producing the disease called *colica pictonum.* Cullen observes, that the preparations of lead are certainly powerful in controlling hæmorrhage, but that they are otherwise of a character so pernicious as to forbid their use except in cases of the utmost danger. Of late years this drug has usually been administered in small doses, and guarded by opium; and it is to this combination that Dr. Paris refers when he declares it to be a safe and manageable remedy. Subsequently Dr. Anthony T. Thomson saw reason to suspect that lead acted as a poison upon the animal body, only in the shape of its *carbonate.* And the results of a series of experiments upon brutes satisfied him of the correctness of this notion. He held, that when the acetate of lead produces the well-known symptoms of the painter's colic, it does so in consequence of its being somehow converted, after its reception into the body, *into* the carbonate: that this conversion may be obviated by a very simple expedient; and that the remedy may then be given with perfect safety in large and efficient doses. The expedient is merely that of administering the lead in draughts containing some dilute acetic acid, which prevents the decomposition of the acetate by any carbonic acid that may happen to be present in the intestinal canal. In this way, as he himself told me, he had given as much as fifteen grains daily for ten days together, without any inconvenience, and with most excellent effect upon the hæmorrhage. I have often prescribed lead in this manner; and I have never known it give rise to any unpleasant consequences.

In slight cases of hæmoptysis, the mineral acids, with or without alum, are often sufficient; or, if there be feverishness, the saline draught with nitre and digitalis. Of the numberless other drugs which have been vaunted as specific in hæmorrhage I have very little personal experience; at least in pulmonary hæmorrhage. You may sometimes be urged to give a celebrated quack medicine — *Ruspini's styptic*—which has obtained a high repute, and sells at a high price. This nostrum seems for a long while to have baffled analysis. The late Dr. Wollaston told Dr. Maton that it contained no metallic substance; Dr. Thomson afterwards announced that it mainly consists of a solution of gallic acid in alcohol, diluted with rose-water. But I believe that all the remedial agents which contain gallic acid are more effectual in another form of internal hæmorrhage, to be considered hereafter. Pure gallic acid itself is now easily procured; and it may be administered in doses of ten grains every six hours. Dr. Hermann Weber has great faith in the efficacy of emetics in obstinately recurring hæmoptysis; both for arresting the bleeding, and for clearing the bronchi of effused blood, and thus preventing inflammatory damage. Trousseau praises the same remedy, and gives large doses of ipecacuan, twelve grains or more at a time; quoting Baglivi's testimony to the good effects of this Brazilian root: 'Radix ipecacuanhæ est specificum et quasi infallibile remedium in fluxibus dysentericis, aliisque hæmorrhagiis.'

One word more of precaution. You may suspect that inflammatory mischief is resulting from retention of extravasated blood in the lungs when you find that shortly after an attack of hæmorrhage the temperature of the patient's body, as ascertained by the self-registering thermometer, is sensibly augmented. So long as this increase of heat continues, whatever improvement be manifest in other respects, keep your patient in a recumbent posture, and absolutely at rest.

LECTURE XLIX.

Pulmonary Emphysema; vesicular and interlobular. Anatomical characters of vesicular emphysema; physical signs; general symptoms; causes; treatment. Interlobular Emphysema; its anatomical characters, symptoms, cause, and cure. Œdema of the lungs. Phthisis Pulmonalis.

I HAVE yet one or two morbid conditions of the lungs to consider and to describe, before I go to that which is the most common and most extensively fatal of all its morbid conditions —pulmonary consumption.

There is a state of the lung, or rather there are two or three different states, to which Laennec has applied the name *emphysema*. A very injudicious name it was for him so to impose. We are infinitely indebted to Laennec for the entirely new light which his able researches have thrown upon the morbid anatomy and the pathology of the lungs: but we have to regret that he should have employed, in several instances, a vicious nomenclature. Emphysema is a term that had long been familiar among medical men in a certain definite sense. It was used to express inflation of the areolar tissue of the body with air: and surgeons still make much of it as an indication, in cases of fractured rib, that the bone has grazed the pleura, and allowed air to pass into the areolar tissue, and to diffuse itself over the chest and neck, and other parts; so that these parts, when pressed, convey a curious sense of crackling to the finger. But emphysema of the lung, as that term is employed by Laennec, includes dilatation of the air-cells of the lungs, and rupture or destruction of the partitions which separate them from each other; and also the infiltration of air into the interlobular areolar tissue, or into the subpleural areolar tissue. In strictness of language these last conditions alone should have been called emphysema of the lung. Laennec has distinguished the two species in this way. To dilatation of the air-cells, with or without a breach of their partitions, he gives the name of *vesicular* emphysema: 'the vesicular (I quote the words of Sir John Forbes' translation), or pulmonary, properly so called.' Now in truth this is emphysema *im*properly so called. To infiltration of the areolar tissue in or around the lung with air, *i.e.*, to emphysema of the lung in the old sense of that word, he applies the title of *interlobular* emphysema. We cannot change these denominations now. They have fastened themselves upon medical language. But it is very fit that you should be aware of their inconsistency with the ancient signification of the same word, and have clear notions of what in Laennec's nomenclature they are intended to express.

The change called vesicular emphysema was not *unknown*, as a mere morbid condition, before the time of Laennec: but it had been noticed by very few writers, and *practically* it was wholly *unattended to*. Yet it is extremely common: much more so than you might suppose: and when rightly studied it is of great interest too, in relation to the general pathology of the chest. Its true nature and modes of origin, however, have been but recently made out; and its very presence may easily escape observation, both in the dead and in the living body, if it be not looked for. For these reasons it will be proper to devote to this every-day disorder a somewhat close attention and survey.

Laennec was undoubtedly the first to put emphysema pulmonum upon the list of definite and cognisable diseases; to point out its frequency; and to collect its symptoms. But when he affirms that, before his time, the pulmonary change which constitutes the disorder was misunderstood by nearly all those persons who had noticed it, he scarcely does them justice. 'All of them (says he) seem to have thought that the derangement in question consisted in the infiltration of the cellular substance of the lungs with air.' He inconsistently adds, 'Ruysch and Valsalva are the only authors, as far as I know, who have observed in individual cases the dilatation of the cells;' and with still greater inconsistency he proceeds to quote, from Morgagni, the following passage, in which this dilatation is very clearly described: 'Sinistri pulmonis lobus superior, quà claviculam spectabat, *vesiculas ex quibus constat mirum in modum auctas habebat*; ut nonnullæ avellanæ magnitudinem æquarent; cæteræ multo minores erant.' You will find the same change noted by Dr. Baillie, in his 'Morbid Anatomy:' and by earlier English writers than he.

Vesicular emphysema, then (to adopt Laennec's phraseology), consists in dilatation of the air-cells. The enlarged cells become misshapen also in many cases. They vary in magnitude from that of a millet-seed to that of a swan-shot; nay, the cavities may even reach the size of a nutmeg or of a hen's egg: but when they are as big as this—and *a fortiori* when they are still bigger—the distension and vacuity are, no doubt, the result of the union of several contiguous air-cells, broken into a single cavity by the stretching or destruction of the partitions that naturally divide and isolate them. You may see the dilated vesicles very plainly through the pleura if you carefully examine the surface of the lung. They appear to the naked eye as the healthy vesicles appear when seen through a magnifying glass. Sometimes all the vesicles belonging to one lobule are enlarged, while those of the adjoining lobules are of the natural size. In that case the emphysematous lobule is conspicuous both by its peculiar colour, and by its protrusion. The surface of the lung is often rendered very irregular and uneven by projections of this kind. Sometimes one large globular prominence is seen, like a bubble on water, or like a little bladder springing from a footstalk: but if you examine it closely you will generally find that the footstalk is merely a constriction at the surface, and that there is as large a cavity beyond it, in the lung, as there is without. These bullæ you cannot slip about, by pressure, from one part of the pleura to another.

The unevenness produced by vesicular emphysema upon the outside of the lung is manifest enough, when looked for; but the same condition of the air-cells exists also within, and *there* it is *not* so readily perceptible. The fluids which the lung contains, obscure all distinction of parts when the organ is cut. The best way of getting a fair view of the dilated cells as they appear in the substance of the lung, is to permanently inflate the emphysematous portion, by blowing air in at the bronchial trunk which belongs to it, and then tying that trunk to prevent the escape of the air. The inflated lung should be hung up in a current of wind, so that it may quickly dry; and during the drying process it should, from time to time, be re-inflated: for else the included air gets out somehow, and the piece of lung shrinks and shrivels up. When it is quite dry, if a section of it be made with a thin sharp knife, the altered state of the air-cells, some of which are *more* and some *less* dilated, will be very conspicuous.

No part of the lung is exempt from liability to these morbid changes: but generally they are limited to certain portions of the organ, and they are much more common and more pronounced at its loose anterior borders, and near its summit in front, than anywhere else. Both lungs appear to be alike obnoxious to the disease; which seldom affects the one without affecting, in a greater or less degree, the other also.

The parts that are emphysematous are usually paler than the rest, and sometimes they are quite white. In extreme cases the surface of the lung presents a sort of piebald appearance; large patches of it looking as if they had been bleached. This pale colour is oftenest seen towards the free edges of the lung. Sometimes those edges are rounded and thick; sometimes thinner, and folded back; while sometimes the margin is blown out, as it were, into an irregular fringe; some of the inflated portions remaining connected with the lung by slender pedicles, and thus forming *appendices* to it of a light yellow colour. I presume that what was thought and called a fringe of fat, garnishing the edges of the lung, in the body of King George IV., was of this kind. At least I have never seen, nor heard of, any other example of fat deposited in those organs. If you hold the emphysematous border between your eye and the light, you perceive that it is translucent: if you prick it with a pin, the puffy part surrounding the puncture sinks down; which shows that the dilated vesicles communicate together.

An emphysematous lung is not only paler, but drier also than ordinary: and for the same reason. It possesses fewer capillary blood-vessels, less blood, and consequently less moisture. It is dry and light, and floats high upon water, like a bladder filled with air.

If you take such a lung out of the body, having its surface embossed with irregular groups of enlarged air-cells—and if you inflate that lung, by blowing into the bronchi—the emphysematous portions will often *seem* to sink in, and flatten, and return to the ordinary level as the whole lung becomes distended. In point of fact, however, these portions remain permanently dilated, and the other parts near the surface, when they are sound and permeable, rise, as the air enters them, until the whole is smooth and even. The emphysematous portions, having lost their proper elasticity, do not subside, as the adjoining portions do, when the pressure of the atmosphere is admitted to the external surface of the lungs. Hence you will see how it is that, when the vesicular emphysema is extensive, so as to occupy nearly the whole of the lung, the lung becomes apparently too big for the case in which it is contained. Not only does it *not sink down* when the sternum is raised, but it sometimes even *protrudes*, the moment that the opening is made. This is a consequence of the circumstance that there is a much larger amount of air enclosed and stagnant in the lung than there would be but for the emphysema. When you handle such a lung, it gives a very different sensation to the fingers from that produced by pressing a healthy lung. It feels like a down pillow. It crepitates less; the air is less easily forced out of it, and escapes slowly, with a slight hissing noise.

The physical characters of the morbid state that I have been describing suggest at once the idea of some physical cause for it. The mode and the mechanical conditions of its production have been much discussed, and various explanations have been offered, most of them containing elements of truth, yet incomplete and insufficient when taken separately.

Acute vesicular emphysema—or mere over-distension of healthy air-cells in lungs otherwise sound—may result from the accumulation of air in the lungs, when it is admitted during inspiration more fully than it is capable of departing during expiration. This is what often happens (as Sir William Jenner has pointed out) in children suffering under diphtheritic laryngitis. The false membrane in the larynx or trachea acts like a valve, allowing air to enter, but little or none to come out. This form of emphysema may be manufactured in lungs removed from the dead body, by forcibly inflating them; or by compressing one part of the lung, and thus driving air enough to other parts to over-distend their air-cells. As no damage is done to the general texture of the lung by emphysema thus produced, the air-cells may recover their natural dimensions when the distending force has ceased to act.

This explanation is sometimes called, erroneously, the theory of emphysema from inspiration. It has more to do with incomplete *ex*piration. I very much doubt the possibility of causing emphysema in a healthy lung by any effort of inspiration, however strong or long continued.

Then there is the theory of expiration, rightly so called—the mechanical effect of forced and long expiration: as in the protracted series of expirations in the paroxysms of hooping-cough—or in the straining expiratory efforts made by persons who blow wind instruments of music—or are engaged in the lifting of heavy weights.

You know that whenever the thorax expands, the lung which it contains must needs follow it, and expand proportionally; for in a material so soft, no vacuum can occur between the two pleural surfaces, which must remain in constant contact with each other. In the ordinary respiration of health, this close adaptation of the lung to its case is maintained without any mutual pressure between them; they simply touch each other everywhere; just as one's hand and fingers sustain no pressure from an accurately fitting glove. But if after a full inspiration strong expiratory efforts are made while the glottis is kept shut, centrifugal pressure of the lung necessarily takes place upon the internal surface of the containing thorax. Now some parts of the lung case are more rigid and incompressible than others; and this being so, the lung will enlarge itself more in the parts less rigidly confined than elsewhere, and the air vesicles in those parts will be more liable than the rest of the air vesicles to be over-distended and stretched; in other words, more liable to be made emphysematous.

Which are these less unyielding portions of the lung-case? A moment's reflection will tell you. Sir William Jenner has clearly specified them in a paper on emphysema in the fortieth volume of the 'Medico-Chirurgical Transactions.'

In the first place, the lung is covered by comparatively soft tissues only, i. e. by yielding tissues, above the first rib. Accordingly, there is often a manifest bulging above the collar-bones, and above the top of the sternum—perceptible by the eye, and certified by percussion to be *pulmonary* bulging—during the forced expirations of a fit of coughing. The air vesicles of these bulging portions of lung are distended to the utmost, and often over-stretched, and we have emphysema of the *apex* of the lung.

Again, there is a similar liability in the lungs to be over-distended during forced expiration, in their *anterior margins*. 'The expiration of coughing (says Sir William Jenner) is performed chiefly by the abdominal muscles forcing up the diaphragm; the ribs compressing (I would rather say *restraining*) the lung laterally, and the sternum and the cartilages of the ribs, anteriorly. But the cartilages are to some degree yielding, and the anterior margin of the (left) lung is to a considerable extent protected from direct pressure (or restraint) by the heart and great vessels. The air is therefore driven during violent expiration from the parts most restrained into the air-cells of the *anterior margin*, as well as into the air-cells of the apex.'

For like reasons, and because the liver on the right side is more unyielding than the stomach, the lower *lobe* of the left lung is more obnoxious to emphysema than the corresponding lobe of the right. Louis accordingly found the former twice as often the seat of emphysema as the latter.

Another way in which vesicular emphysema is very often produced has been indicated and clearly explained by Dr. William Gairdner. You know already what is meant by pulmonary *collapse.* The complete occlusion of a bronchial tube is followed by collapse and closure of the pulmonary air-vessels beyond and belonging to that tube: and this collapse of certain portions of the lung leads to emphysema of certain other portions. Collapse of any part of a lung implies a proportional diminution in the *bulk* of that lung, and since there can be no vacuum, it follows that, as the thorax expands, more than the standard quantity of air must enter those tubes and vesicles which are not involved in the collapse: and if the quantity exceed a certain amount, dilatation of the vesicles must, of necessity, ensue. There may indeed be atrophy and reduced bulk under other conditions of condensation; and if so, there may arise a correspondent emphysema. The shrinking up of one part causes undue inflation of other parts which may also thus be rendered permanently unsound. It is when a portion of the lungs has lost its expansile power, and those organs rise in pursuit of the widening parietes of the chest in inspiration, that some of the permeable portions are apt to be stretched beyond their healthy dimensions by the force of the entering air.

Remember then that vesicular emphysema is often a mechanical and complementary (though by no means a compensating) effect of the condensation and diminished bulk of other portions of the lungs, and not a substantial affection in itself. Disease of one kind in one part, brings about disease of an opposite kind in another part of the same organ.

I have shown you how collapse of one portion of a lung—collapse consequent upon the sealing up of a bronchial tube—may lead to emphysema of another portion. When there is simply some amount of impediment to the free entrance of air through such a tube, short of its complete plugging, the portion of lung beyond the obstacle does not collapse; on the contrary, air may collect beyond the obstacle, and not be able to get back again, and some of the air-cells may thus become dilated. This is just an instance, on a diminished scale, of what sometimes happens when the larynx or the trachea is partially obstructed.

Whether the obstruction be complete or not, the resulting emphysema falls under what is called the expiratory theory; it is during expiration, if at all, that the mischief is done.

But there is still another and a very different way in which a kind of emphysema of the lung may arise—or increase. The membrane that constitutes the wall of the air-vesicles may be wasted, and destroyed, by interstitial dilapidation. The change is one effect, among many others, of senile decay. Mr. Rainey has described, in the thirty-first volume of the 'Medico-Chirurgical Transactions,' the rise and progress of this change, as seen through the microscope. The membranous partitions between the vesicles become riddled with perforations, which are well defined, and more or less numerous according to the stage of the disease; at length the partitions break down, and disappear. In this *atrophic* emphysema, there is no necessary enlargement or want of symmetry, either of the lung or of the thorax.

Yet a similar withered condition of the pulmonary tissue may coexist with thoracic enlargement and loss of symmetry—for it may grow out of those forms of vesicular emphysema which I first described. As the enlarged air-cells continue to dilate, the minute blood-vessels distributed over their parietes are gradually compressed and emptied; and many of them are at length completely obliterated. Hence not only an exsanguine condition of the pulmonary tissue, but atrophy also of the inter-vesicular partitions, which become first thin, then tattered and imperfect. In dried specimens of emphysematous lungs you see very plainly the remains of the former walls of separation between the vesicles.

The state of the emphysematous lung, as discovered after death, being such as I have attempted to describe, you will naturally be inquisitive to know by what signs the existence of conditions so remarkable is revealed during life. First, then, when the emphysematous distension is considerable in amount, and extensive, it is associated, as I have already hinted, with notable alterations in the shape and movements of the chest. The lung having lost much of its elasticity, and often holding also air imprisoned by obstruction in some of the bronchial tubes, cannot subside as a healthy lung subsides. The act of expiration is arrested and incomplete. The movements of the ribs are very limited. The thorax remains nearly in that position which it has assumed after inspiring. It is prominent and rounder on the diseased side: or on both sides, if both lungs be affected; but it is apt to be irregularly prominent, and unsymmetrical; to bulge here and there in correspondence with the bulging of the lung within. The dimensions of the two sides may differ. The ribs lie less obliquely than they should lie, and the chest is, therefore, more cylindrical, or barrelshaped. The clavicles are ill-defined in such persons. They are so, as you know, in *fat* persons; wherefore this aid to the diagnosis is of most value in those who are *spare*. In them it is a valuable sign, for it is simple and obvious. The distended lung presses upwards, as well as in other directions, and tends to efface the depressions which naturally exist both above and below the collar-bone. This symptom is the more to be depended on, if it present itself on one side only. It is readily distinguishable, by means to be mentioned immediately, from a similar fulness of the subclavian region produced by a large collection of liquid within the pleura. The manner of breathing is instructive also. The ribs, never receding within their proper limits after *ex*piration, can move but little during *in*spiration: and the breathing is in a great measure diaphragmatic and abdominal. The diminished elasticity of the pulmonary tissue, and the partial obstruction sometimes existing in the bronchial tubes, cause the duration of the expiratory movement to exceed that of the inspiratory.

Now all these signs are physical signs. But what, in the second place, are the *auscultatory* physical signs? Why, in the emphysematous regions, which commonly are also the most bulging, percussion yields an unnaturally clear and resonant sound; while auscultation discovers a very indistinct vesicular murmur. The two modes check and explain each the information afforded by the other. Percussion ascertains that there is *air* beneath the part struck: auscultation ascertains that there is little or no *air in motion* beneath that part. It follows, therefore, that there is stagnant air: air shut up in the enlarged air-cells and gaps of the pulmonary tissue; or air interposed between the ear and the lung in the cavity of the pleura: air at rest, in fact. I say the respiratory murmur is very feeble. This partly depends upon the limited play of the ribs, partly and chiefly upon the circumstance that the air is imprisoned in the dilated cells. When none enters or leaves them during respiration, no vesicular breathing can be heard: and then we must call in the aid of other considerations to determine whether the air be contained in the cavity of the pleura, or in the emphysematous lung.

Pure pneumothorax is extremely rare. Pneumothorax with liquid effusion is easily recognised by its proper signs. If these be absent, we conclude that the stagnant air occupies the lung, and not the pleura: and this conclusion is strengthened if the unduly resonant part be prominent also. Almost all writers on this subject follow Laennec in stating that *dry* crepitation may be heard in the emphysematous parts. It may be so; but if so, I cannot distinguish it. I mean that I know no crepitation but that which is moist, and which proceeds from the formation and rupture of bubbles, as air passes through liquids in the bronchial tubes. But some people believe that they can hear a dry crackling, such as may be produced by inflating a portion of dry areolar tissue out of the body, or by crumpling a dry bladder. You will try this by your own experience. I do not deny that such a sound occurs: I only say that if it do occur, I cannot tell when I hear that dry sound, and when I hear large moist crepitation. But I more than suspect that no such sound is given out by an emphysematous lung; and that the sound heard is really large crepitation in the neighbourhood of the dilated cells: for emphysema is very often accompanied by catarrh: and the sounds in question, authors agree, are not permanently audible.

So much for the physical signs of emphysema pulmonum. The *general signs*, when the change is extensive, are habitual shortness of breath, with occasional paroxysms of more urgent dyspnœa; cough, which, however, is far from being a constant symptom; palpitation in most instances as the disease advances; pallor or blueness of the lips and skin; and more or less œdema of the ankles. Usually the appetite remains unimpaired, and the patient does not lose flesh. The disorder is unattended with fever; and is essentially chronic.

The paroxysms of more urgent dyspnœa are frequently concurrent with, and apparently excited by, attacks of smart bronchial catarrh; but they sometimes arise without any obvious cause. They are apt to come on suddenly, in the night, and the patient is obliged immediately to sit up, and even to open the doors and windows of his bed-room, that he may breathe more freely. In one word, he undergoes a paroxysm of *asthma*. These attacks become more frequent and more severe as the patient and the disorder grow older. They are attended with much wheezing; and in the lower posterior part of the lungs crepitation is generally audible. At first the palpitation of the heart, and œdema of the feet, abate and cease as the violent dyspnœa goes off: but at length these symptoms become permanent.

We see a reason in the physical condition of the thorax, why the breathing should be more oppressed, and why the paroxysms of orthopnœa should occur more frequently, *in the night*. Whenever the respiration is principally abdominal, it is apt to be embarrassed by the recumbent posture, which throws a part of the weight of the viscera of the belly upon the diaphragm. The horizontal position is always ill borne by these patients; and, for similar reasons, flatulence or fulness of the stomach, however caused, distresses them.

Cough, as I observed before, is an occasional, but by no means an essential accompaniment of vesicular emphysema: the expectoration, when there is any, is thinnish, like gum-water, and full of foam: or it may consist of those semi-transparent pearl-like bits of mucus which Laennec describes as characteristic of dry catarrh.

The disease of broken-winded horses is pulmonary emphysema; and Sir John Floyer, in his *Treatise of the Asthma*, published in 1698, sets forth, in the quaint language of that olden time, both the alteration which Laennec thought he had been the first to describe, and the manner in which he supposed it to arise. His observations respecting the lungs of horses are equally applicable—and he no doubt intended to apply them—to the human lungs. After speaking of 'the broken wind, from the rupture or dilatation of the bladders of the lungs, by which the air is too much retained in the bladders, or their interstices, and thereby produces a permanent flatulent tumour'—and stating that 'these horses wheeze much after filling their stomachs by water or food, because that keeps up the diaphragm'—he goes on thus:—'As it happens in external flatulent tumours, they at first go off and return, but at last fix in permanent flatulent tumours; so it is in the flatulent asthma, the frequent nervous inflations induce at last a constant windy tumour or inflation; and it ought to be considered how far holding the breath in hysteric fits, or the violent coughing in long catarrhs, or the great distension of the lungs by an inflammation in the peripneumonia, may strain the bladders and their muscular fibres, and thereby produce the same rupture or dilatation or hernia as happens in the broken-winded. This must be observed by the help of the microscope; and if the air blown into any lobe will not be expelled thence by the natural tone or muscle of the bladders, that the lobe may again subside of itself, 'tis certain some injury is done to the ventiducts; the bladders are either broken, and admit the air into the membranous interstices, or else they are over-distended like a hernia in the peritoneum; and this will produce an inflation of the whole substance of the lungs, and that a continual compression of the air and blood-vessels, which will produce a constant asthma.' This is really a capital piece of pathology for the seventeenth century.

Emphysema is always (in my opinion) a consequence of preexisting disease or disorder of the chest. It has, indeed, been thought by some to be occasionally a congenital, idiopathic, and inherited affection. That it should ever be congenital seems impossible. It is difficult to conceive how it could ever be idiopathic. But you may often trace back the habitual shortness of breath to the period of infancy. Patients will tell you that from their earliest recollection they have been easily put out of breath: that they were never able to engage heartily, and to the same extent with their companions, in the active sports of childhood. It is unquestionable, too, that the complaint is traceable, from one generation to another, in certain families. Being thus often present at an early age in children born of emphysematous parents, the emphysema, in such cases, has been set down as a vice of the original bodily formation. I am not convinced by this mode of reasoning. The facts upon which it rests show simply that the disorder may run in families, that a proclivity to it is often inherited, that in fact it is hereditary in the same sense in which many other disorders are hereditary. The same constitutional peculiarities of fabric or shape which favoured the production of emphysema in the parent, are repeated in the offspring, and yield the same fruit. Young children are very liable to those disorders which are fertile of emphysema, to obstructive disease in the larger air-passages, and to lobular collapse and its consequences. We know that lobular collapse is a frequent legacy of hooping-cough in the early periods of life. After the cough has ceased, however, the shortness of breath which it leaves behind is easily overlooked, until, with the increase of the emphysema, it forces itself into notice. For when once the morbid process has begun, it tends, slowly often, but surely, in the way already described, to its own augmentation.

It is the bad influence of vesicular emphysema upon the nutrition of the lung that makes it always a *progressive* disease. We see why it is that, speaking generally, the extent of the morbid change is proportioned to the age of the patient: why paroxysms of severe dyspnœa at length supervene; and become more and more frequent and trying. The function of the lung becomes year by year more limited; until it can no longer bear, without a struggle of distress, that further encroachment upon its office and capability which a slight catarrh, rapid movements of the body, a distended abdomen, or even the recumbent posture, may be sufficient to produce.

Vesicular emphysema may arise then, and receive increase, from various disordered conditions that precede or accompany it, and of which it is the effect: from any condition which renders a portion of the lung impermeable by air, and at the same time diminishes its bulk. On the other hand, it may be, and it often is, itself the cause of subsequent disease, not merely in the lung, but in other parts: and above all, of disease in the right chambers of the heart. The smaller blood-vessels, as I have shown you, are gradually effaced as the dilatation of the air-cells proceeds; the emphysematous lung is evidently in a state of comparative anæmia, and incapable of admitting all the blood which is due to it from the pulmonary artery. In other words, the right side of the heart does not empty itself with its accustomed ease. Hence increased muscular contractions of the right ventricle: and a yielding of its walls to the augmenting pressure of the contained blood; with hypertrophy also of its walls. And this embarrassment of the circulation in the right side of the heart is aggravated at those periods when the paroxysms of urgent dyspnœa occur. Now nothing is a more sure cause of anasarca than a permanent dilatation of the right cavities of the heart: and this influence of the unsound lung upon that organ is clearly seen in the palpitations to which such patients are liable, in their dusky complexion, and in the œdema of the feet and ankles which often becomes manifest at the same time.

But emphysema of the lungs is often associated with valvular disease on the *left* side of the heart, and then the relation of cause and effect would seem to be reversed. It is supposed that disease of the mitral or of the aortic valves may so impede the circulation of the blood as to interfere with and impair the nutrition of the lung tissue, and to diminish its natural elasticity.

Impermeability of part of the lung, with reduction of its bulk forming one element in the genesis of vesicular emphysema—we see how that change *may* be produced by *pressure*, when the pressure implies condensation and atrophy of the pulmonary substance: by tumours, therefore, within the thorax, by an enlarged heart, by aneurism of the thoracic aorta, by deformity of the chest from a crooked spine, and even by tight lacing.

The relations between pulmonary emphysema and tubercles of the lungs require also a short notice, for they have been misunderstood. Louis, who has analysed with his accustomed care and exactness a considerable number of cases of emphysema, states that the disease is seldom complicated with tubercles. It has even been affirmed that the two disorders are antagonistic of each other, and incompatible: and this doctrine has led to the absurd proposal of attempting to prevent phthisis by creating emphysema. The latter exploit would probably be even more difficult to achieve than the former.

The simple truth appears to be that vesicular emphysema bears precisely the same relation to tubercle as to other structural diseases of the lung. The mere deposit of crude tubercular matter solidifies a portion of lung, and impairs its permeability, without reducing its bulk. The mere deposit of crude tubercular matter has, therefore, no tendency to generate emphysema: and when there is no other pulmonary defect, these two morbid conditions seldom meet in the same lung.

But tubercular phthisis is very often, most commonly indeed, attended in its progress with *cavities* in the lung; and the tissue surrounding these cavities is often in a state of atrophy. I am touching by anticipation upon the terrible disease which will presently engage our attention. Such cavities may be increasing in size, and ill-defined, with weak and yielding walls; and then they, rather than the sound air-cells, will be likely to enlarge under the expanding force of inspiration, so as to balance the amount of atrophy. Large or numerous cavities, therefore, with flaccid walls, are unfavourable to the development of vesicular emphysema, and may be said to guard the lung against its occurrence. On the other hand, when the cavities are tending to heal and contract, and are surrounded (as under such circumstances they usually are) by dense fibrous tissue, and when they are small and few in number, and much of the lung is sound, the expansion during a full inspiration may then tell upon the air-cells, and favour the production of emphysema.

Once more—I shall have to show you that tubercular deposits may issue in a sort of cure, and leave in the lung depressed cicatrices, involving often cretaceous relics of the tubercles. Here we have manifest loss of bulk, while a large portion of the pulmonary tissue may be sound. Here we have the conditions of the generation of vesicular emphysema, and accordingly it is acknowledged by the best observers that vesicular emphysema is almost invariably present in lungs that bear those marks which are considered to be evidences of recovery from phthisis.

Such are the views put forth by Dr. William Gairdner of the relations between tubercle and emphysema of the lung. They are novel, they are interesting, and to the best of my judgment they are true.

I must beg you to bear in mind that emphysema of the lung is one cause, as it is also sometimes a consequence, of *spasmodic asthma*. The asthma so arising is less perilous than that which proceeds from certain other organic changes, to be described hereafter. Vesicular emphysema, in its simple uncomplicated form and early stage, is seldom attended with any present danger. Its progress is indeed constant, but usually slow. When it proves at length fatal, it is so in consequence of the superaddition of some other disease. Laennec states very truly, that, of all the varieties of asthma, this is the one which affords the patient the best hope of a long life.

The condition that I have been describing, when once it has fairly been established, can scarcely admit of cure. We shall do our patients good, not by any treatment addressed to the existing emphysema itself, but by guarding them against those circumstances which are likely to aggravate it; and by mitigating or removing those other disorders with which the emphysema is apt to be combined. Whatever is calculated to put the patient out of breath is bad for him. It is observed that they who, having emphysema, are obnoxious also to catarrhs, during which the dyspnœa is singularly increased, are much more free from such attacks in the warm weather of summer than in the winter. This explains the beneficial influence of a judicious change of climate upon such persons, and it points to the necessity of warm clothing in the colder seasons for those who are obliged to remain in this country. The feet especially should be kept dry and warm. No care to prevent bronchial irritation can be too great. In an advanced state of the emphysematous disease, with dilatation of the right side of the heart, livid lips, and a feeble systemic circulation, extreme danger to life may arise, as Dr. Gairdner cautions us, from almost insignificant attacks of catarrh.

The great assuager of those fits of extreme dyspnœa which resemble asthma has been opium; and especially opium combined with æther. Half a drachm of Hoffman's anodyne, with a third of a grain of the acetate or muriate of morphia, in camphor julep, will operate like a charm often, in quieting the whole system, and removing the difficulty of breathing. This circumstance alone would lead us to suppose that the access depended, in part at least, upon a spasmodic state of some of the muscles concerned in respiration. To this question I shall revert when I speak of asthma as a separate disease. At any rate you will find that some such formula as I have just mentioned will stand you in good stead when you have to deal with asthma engrafted on emphysema: but I must add that I have trustworthy authority for saying that chloral administered in a full dose under the same circumstances, is not less efficacious, perhaps as a rule even more efficacious, than opium.

Dr. Johnson has lately drawn attention to a class of cases in which, after the middle of life, emphysema is associated with an excessive accumulation of fat in the subcutaneous tissue, in the abdomen, and about the heart, especially on its right side. This 'fatty growth' weakens the muscular walls of the right cavities of the heart. The result, as in the case of mitral disease, is that the circulation through both the pulmonary and the bronchial vessels is impeded. 'The impeded circulation leads to impaired nutrition and lessened elasticity of the pulmonary tissue, and thus the lung becomes emphysematous.' Timely warning, he says, against this danger may be usefully addressed to persons in whom the fat-making disposition is manifested. They should take active exercise in the open air, and regulate their diet somewhat after the fashion of what is popularly known as *Bantingism*. 'As a general rule, they should be directed to take the fibre of fish, flesh, and fowl, liberally; to eat sparingly of fat, butter, sugar, and farinaceous substances, including potatoes, bread, and pastry; to abstain entirely from malt liquors; and to take no more wine or spirit than is absolutely needful for health or comfort.'

The *interlobular and sub-pleural* emphysema of the lungs is a species of *true* emphysema, the air being contained in the meshes of the common areolar tissue. When it appears on the surface of the lung, it may be distinguished from the bladder-like prominences which sometimes form there by dilatation of the air-cells, in this way: the bullæ which are situated in the areolar tissue connecting the pleura with the lung, may be made to move hither and thither under pressure; whereas those which result from the protrusion of an enlarged cell or cells cannot be made thus to change their place. This sub-pleural effusion of air is sometimes enormous. I have seen a bladder as large as a hen's egg. Bouillaud mentions a case in which the bladder or pouch was equal to the size of a stomach of ordinary dimensions. It proceeds, I presume, from the rupture of a superficial air-vesicle. Sometimes, as I mentioned before, the *pleura also* gives way, and air is poured into the cavity of the thorax. More commonly the membrane remains entire, and then these large bubbles of air may be seen upon the surface of the lung.

Of interlobular emphysema I can give you but little account except from the observations of others. I have seen but few well-marked examples of it. The lobules of the lungs cohere together by means of areolar tissue, which is dense and close in the natural state, but which admits of considerable expansion when it is inflated with air. If the emphysema be slight in degree, the lozenge-shaped spaces visible on the surface of the lung are defined by little bubbles of air, that look like beads strung upon a thread. But in extreme cases the lobules are fairly blown asunder by the air; the partitions between them increase in width, and are said to be sometimes as much as an inch broad. They are broadest towards the surface of the lung, and narrower towards its roots; and exhibit somewhat of the arrangement seen in the section of an orange, the septa radiating and diverging from a centre. If the areolar tissue could be taken out, there would be left cracks and clefts in the lung. When the interlobular emphysema penetrates to the roots of the lung, the air readily passes into the areolar tissue of the mediastinum, and thence to the subcutaneous tissue of the neck and chest—and then we have the genuine emphysema of authors who wrote prior to Laennec.

There is this material difference between vesicular and interlobular emphysema; that the one is commonly slow and gradual in its formation, the other sudden. The permanent dilatation of the air-vesicles is the work of time. They yield, and lose their elasticity, and break into one another, only by degrees. The interlobular effusion of air may be effected in a few minutes or seconds. It is caused by *violent* straining efforts; such as those made by a woman in childbirth, or by any one who exerts himself to lift a weight which is too much for him. A deep inspiration is taken; then the glottis is voluntarily closed, and a strong expiratory effort is made. Some rupture must take place and form a communication between the air-vesicles and the areolar tissue: but such rupture has never been *traced*, nor is it likely that it should be.

They say that this form of emphysema also is revealed by large dry crepitation; why it should, is not evident. I can only say of that sound, as I said before: it may exist, and it may be distinguishable from large *moist* crepitation, but my ear is not delicate enough to distinguish it: and to speak the truth, I doubt exceedingly whether any such sound really occurs at any time. But do not let my doubts infect you. Try for yourselves; and till you have had opportunities of investigating this point, consider it as *adhuc sub judice.*

Again, they say that the noise of friction denotes the existence of interlobular and subpleural emphysema. On this point I can give you no information of my own knowledge. That you may sometimes hear the costal pleura rub against the pulmonary during inspiration and expiration I know. I have often heard that sound (as I mentioned to you before) when the membrane has been roughened by pleurisy. But that a soft, smooth, moist lung, though embossed by emphysema, will give rise to a rubbing sound, I do *not* know. It may be so, but it has never occurred to me to hear it.

We may be more certain that interlobular emphysema has arisen when, immediately after some violent straining effort, considerable dyspnœa and oppression ensue, and presently the subcutaneous areolar tissue becomes emphysematous. You will understand how rapidly the inflation of the areolar tissue may take place if you ever saw a butcher blow up that of a calf which he is in the act of skinning.

As interlobular emphysema differs from vesicular emphysema in its seat, in the suddenness of its formation, and in its cause, so does it differ in its curability. Under favourable circumstances it will soon cure itself—the air will be reabsorbed, and the dyspnœa cease. I do not know that we can do much by art to accelerate that process. If the dyspnœa be extreme, and if the air make its appearance externally, and can be felt, crackling, beneath the skin, you may let it out by making a few punctures with a lancet, and the deeper-seated emphysema will be lessened as the air escapes. I believe that this interlobular emphysema is more common in infancy than in any other part of life; on account, I suppose, of the greater delicacy and tenderness of *all* the tissues at that age.

The interstitial areolar tissue of the lungs, as well as the air-cells, is liable to be filled not only with air, but with serous fluid: and this constitutes *œdema of the lungs*; a condition which is by no means uncommon, and one of which you ought therefore to be aware ; but it need not long occupy our attention at present. When a lung, or a portion of a lung, is anasarcous (and you will often find that the œdema is limited to the most depending part of those organs), it is generally of a pale gray or yellowish tint ; is heavier than healthy lung, and less crepitant ; and pits more on pressure—is *doughy.* And if the œdema be extensive, the lung does not subside perfectly when the chest is laid open. If incisions are made into such a lung, a thin watery fluid flows out, more or less spumous ; and if the lung be well squeezed, the whole of the liquid may be expressed ; and then it will be obvious that the texture of the organ is sound, but that it had previously contained less air than usual, in consequence of the presence of the watery fluid.

This condition of the lung very commonly occurs as a part of general anasarca : and we may discover its existence, first by noticing that there is dropsy of the areolar tissue in other parts ; secondly, that the patient has dyspnœa ; and thirdly, by hearing crepitation, produced by large bubbles, at the lowermost portions only of the lungs. Into those portions the liquid gravitates ; just as it sinks into the ankles when the patient sits up or walks about. There still is air in the œdematous portion ; so that percussion still gives a hollow sound ; as hollow at least on the one side as on the other. With the air there is liquid also, which transudes, I suppose, from the areolar tissue, or is exhaled from the surface of the membrane : and the liquid is from time to time coughed up and expectorated. Sometimes, however, there is but little expectoration. What does come up is chiefly aqueous, with occasionally a piece or two of mucus floating upon it ; and it is also somewhat foamy.

Œdema of the lung is symptomatic of other disease ; generally of disease of the heart or great blood-vessels : it is always to be found after death by slow apnœa, whether the cause of the apnœa was in the larynx, in the larger air-passages, or in the substance of one or both lungs. In any case, it is capable of no other rational treatment than such as is suited to the original disorder ; and therefore I have nothing further to say about it now.

I proceed, in the next place, to that most prevalent and lamentable disease of the lungs, which is well known to everybody, under the name of *phthisis pulmonalis*, or *pulmonary consumption.* And without pausing to make any general reflections, touching facts which must be familiar to you all—the fatal tendency and the too often hopeless character of the disease, and the havoc it produces among the young, the most gifted, and the most beautiful, of the human race—I shall commence by inquiring into the *morbid anatomy* of tubercular phthisis; which will naturally introduce us to the consideration of its causes, symptoms, general history, and treatment.

Phthisis, you know, means a wasting away, or a consuming; but of late years the term has been mainly restricted to that *species* of wasting disease, which consists in the occupation of the lungs by tubercular matter, and the changes which that matter *suffers* and *works*. But it would be an error to suppose that the disease is limited to the *lungs* in these cases. The lung disease would be sufficient at length to destroy life; but its mortal tendency is aided and accelerated, in most instances, by disease of a like kind, situated in other organs. 'The *pulmonary* consumption (as Dr. Latham justly observes) is no more than a *fragment* of a great constitutional malady.' But that malady plays its part most conspicuously in the lungs. I shall notice its complications as I go on; but I am desirous of cautioning you in the outset against supposing that tubercular phthisis is *exclusively* a lung disease.

Before I proceed to a more particular description of the changes wrought in the lungs in the progress of pulmonary consumption, I must revert to certain statements, made by me in a former lecture, of the doctrine that has been held respecting the probable *genesis of tubercle*. I do so for the purpose not simply of refreshing your memory, but of reviewing, and it may be of rectifying, that doctrine.

You may remember that, without vouching for their absolute correctness, I adopted the opinions put forth by Sir Robert Carswell as to the source and distribution of tubercular matter in the lungs. But even in the short space of time that has since elapsed, new views have been opened to me which I am bound to lay before you; for they contravene, and if established must completely revolutionise men's former beliefs concerning the etiology of tubercular disease. It is my misfortune, and my shame, that I am unable to read the German language. What I know of the researches of the able and patient workers in medical science among the Germans, I get from translations, or from English notices, of their writings: and I have but just made acquaintance with some Clinical Lectures on Phthisis by Professor Niemeyer of Tübingen, which have been deemed of sufficient importance and value to merit translation and republication in this country, by the New Sydenham Society. To the doctrines enunciated in these lectures I am now about to direct your attention—reminding you, as I go along, of that part of the older doctrine which they claim to refute and supplant.

It is not difficult, in looking back, to trace the birth and growth of the newer theory. Ever since the phenomena of *pyœmia* and *septicœmia* received clear explanation under the light of modern scientific enquiry, pathologists have been aware that the introduction of pus, or of other morbid animal matters into the veins—nay, of inorganic substances, capable of minute subdivision, such as quicksilver—has led to the dissemination of pus corpuscles, of septic particles, or of inorganic particles, through the medium of the blood, in various parts of the body, but principally in the lungs, which are the first to receive and to entangle as it were whatever materials foreign to its nature, and too large to pass through the pulmonary capillaries, the blood might carry. The similarity, as to place and arrangement, between the distribution of these materials in the lungs, and the distribution of tubercles, must have struck most observers, as it had long struck myself. Both, and all, it was natural to conclude, were deposited from the blood. How the former kinds of deposit found their way to their respective places, we knew: how the tubercular matter got into the blood was not so clear. It had been a reasonable conjecture that it took its rise in the blood itself; was the result of some constitutional vice, or defect, or impurity of that fluid—impairing thus the healthy and perfect maintenance of the pulmonary (and other) tissues. This was Laennec's theory; and with various modifications, such was substantially the theory, till within the last five or six years, of most English pathologists.

The key-note of the latest hypothesis on this subject was struck, I apprehend, when Villemin first announced, in 1865, that tubercles may be introduced into the lungs and other organs of the body by *inoculation.* It seemed probable that the tubercles found in human lungs—the tubercles with which we as physicians have to deal—might, or must, have a similar origin. But where, or whence, was the contaminating matter?

Now it is asserted by Buhl, and especially insisted upon by Niemeyer, who is the chief expounder of the newer doctrine, that, speaking generally, this problem has been solved; that the source of the tubercles can be assigned.

Niemeyer, and the rest, affirm that the gray granulations which all agree to call tubercles, are derived, in the greater number of cases, if not in all, from pre-existing inflammation—a proposition which, you see, is exactly the reverse of what Laennec's theory asserts. Not that the tubercles are the *direct* product of inflammation. To simplify the explanation I will confine myself for a moment to what takes place (they say) in the *lungs*. In pulmonary inflammation of whatever kind, there will always be inflammatory *products*. Some of these are from first to last fluid, and are soon and easily expelled through the mouth. Some are more solid, consisting of coagulable and coagulated lymph, or of an unnaturally profuse and crowded cell-growth, or of both of these together. These more solid products often undergo gradually a fatty or other degenerative change, liquefy, and so become re-absorbable, and are actually re-absorbed and gotten rid of; and then the recovery from the inflammation is complete. But in other cases there is no such liquefaction. The mass of cell-forms grows denser and closer, till the compressed shrivelling cells lose something or all of their vitality and suffer a change which, from the colour and consistence of the resulting material, has received the disagreeable name of *cheesy*.

Now the new theory says that particles of this cheesy or caseous matter, whatever may have been its origin, are liable to enter the blood by a sort of internal inoculation, and so to give rise to a crop of miliary granulations, which are truly nascent tubercles. And this may happen in lungs which previously to the inflammation were quite healthy—or in lungs which were previously phthisical and tubercular.

More than this. Products of inflammation that have not been re-absorbed, but remain pent up *in other parts than the lungs* (remnants of pleuritic, pericardial, or peritonitic effusions, of chronic inflammations in joints, in bones, in lymphatic glands, and so forth) may, and often do, undergo the caseous transformation, and so at length give rise to the dissemination of tubercles in the lungs, and in other parts of the body. In this way is explained the occasional existence of crude tubercles in lungs which are free from any other semblance of disease.

Blood detained in the lungs after pulmonary hæmorrhage may (it is held) have similar changes and consequences.

I must tell you that, according to Niemeyer, the various forms of pulmonary inflammation have not, indifferently, the same tendency to the caseous degeneration, and the consequent formation of tubercles. The specific change rarely follows ordinary pneumonia; in what he calls acute catarrhal pneumonia it is somewhat more frequent; while in the chronic catarrhal form it is almost the rule.

'I regard (he says) the name chronic catarrhal pneumonia as the only title appropriate to the form of disease usually called infiltrated tuberculosis, and gelatinous and tubercular infiltration; and which latterly, with equal impropriety, has sometimes received the name of tuberculous, or of cheesy pneumonia.'

What he means by *chronic catarrhal pneumonia* seems to be chronic bronchitis limited to certain lobules, or to groups of lobules, and affecting especially the finer terminal divisions of the bronchial tubes, and the corresponding air cells.

He holds that the granulations, thus arising, are really *nascent* tubercles; that they are capable of advancing, and do very frequently advance, into the opaque yellow form—which again, especially when they are collected in groups and masses, may pass into the caseous degeneration, and so become the seed bed of further plantations of nascent tubercles.

You will observe that Niemeyer's theory accounts equally well with Laennec's, for the gradual—sometimes perhaps the very slow—increase and multiplication of tubercles in the lungs: since the internal inoculation of retained cheesy matter may for a long time be occasionally or fitfully repeated; and the admitted degeneration of tubercles themselves into like cheesy material may also give rise, at short or at long intervals, to successive crops, small or large, of younger tubercles.

It is expressly maintained by Niemeyer that 'the formation of tubercle never takes place unless preceded by pneumonia terminating in caseous infiltration of the pulmonary tissue.'

He elsewhere virtually excepts from this broad statement those rare cases in which (according to his theory) tubercles in the lungs are derived from decayed inflammatory products pent up in other parts of the body.

Laennec acknowledged that the semi-transparent miliary tubercles were often of secondary formation; and that this secondary eruption took place at the time when the tubercular matter already existing in the lung began to soften. But he held that this tubercular matter had been deposited directly from the blood: and is never a consequence of pulmonary inflammation. What Niemeyer regards as a caseous degeneration of inflammatory products, Laennec believed to be an infiltration of the pulmonary tissue with tubercular matter.

Both authors agree that the tubercles, whatever their primary origin, may multiply in number, and form groups which, lying contiguous to each other, at length coalesce into lumps or masses crowded with tubercles: so that in the same lung there will commonly be found masses and tubercles, of various sizes, from that of a pin's head to that of a pigeon's egg, or even bigger. They agree also that these tubercles and aggregations of tubercles—as well as the degenerate inflammatory products of the one author, and the tubercular infiltrations of the other—do, in a vast majority of cases, sooner or later soften and liquefy into a condition that admits of their expulsion or escape through bronchial tubes which communicate with the trachea; there being left in the lung in each case an excavation, a cavity, cavern, or vomica. All these names have been given to the void space which the tubercular or the cheesy matter previously occupied.

Niemeyer recognises the fallacious appearances, insisted upon by Carswell, that may present themselves, in tuberculous lungs, upon their being sliced in this or in that direction.

In connexion with these theories the careful experiments made in this country by Mr. Simon, Dr. Sanderson, and Dr. Wilson Fox, are of extraordinary interest. Dr. Sanderson has arrived at the conclusion that in artificial tuberculisation (and by inference in natural disease) the tissues *primarily* affected with tubercle are those which belong, not to the system of blood-vessels but, to the *lymphatic* system. He describes the caseous degeneration as being a kind of slow necrosis, or death (not by mortification or suppuration) of cells crowded together, and suffering anæmia, and consequent diminution and loss of vitality, from mutual compression of their blood-vessels. 'The fate of the cells is to die; of the fibres (which surround and sustain them) to become opaque and granular.'

Dr. Wilson Fox states that the appearances presented in guinea-pigs, under his experiments, consisted of 'growths which in their naked-eye appearances and histological characters correspond with all the essential features of tubercle in man; which occur not only in the organs which are the chosen seats of tubercle in man, but also in the same parts of those organs; which have the same vital characters, and the same early degenerative cheesy changes, not suppuration nor acute softening, and with no marked characters sufficient to distinguish them from tubercle.'

Whichever of the two theories respecting *tubercular* phthisis may ultimately be verified, it must, in my opinion, submit itself to the limitations of the *strumous diathesis* upon which I dwelt in a former lecture. Niemeyer admits the frequent occurrence of an *inherited disposition to pulmonary phthisis*, an inherited weakness and *vulnerability of constitution*; phrases which seem to express what we mean by the strumous disposition. Indeed his description of the effects of chronic catarrhal pneumonia, which with him are the chief originators of tubercle, appears to my understanding very like a description of what I should call the effects of *scrofulous inflammation in the lungs*. And Dr. Wilson Fox expressly admits that in cases of artificial tuberculisation in guinea-pigs, a constitutional predisposition to its acceptance appears to be a necessary element.

The important questions which I have now summarily stated await further inquiry and experience. I refrain from dogmatically forecasting the solution of a problem not yet fully worked out.

With respect to the *human* lungs, it seems certain that the small, hard, translucent granules, generally called *miliary* granulations, but sometimes *the granulations of Bayle*, who first described them, stand in some intimate connexion with the true cheesy tubercle. Both occur in the same persons, in the same lungs, in the same parts of the lung. One very seldom occurs without the other. They both belong essentially to the disease we are considering—pulmonary phthisis. Louis, a minute and faithful observer, states that the granules develope themselves, and show, at a certain period of their development, a central opacity. Upon the whole, we must believe, with Laennec, with Niemeyer, with (I think) Doctors Sanderson and Fox, that the yellow opaque tubercle is often merely an advanced stage of the gray semi-transparent granule; but we must also believe that it may occur independently of the granule, and even become its parent.

It is a remarkable and very important fact, that tubercles, when they affect the lungs, are not deposited at random, or indifferently in all parts of those organs. It is in the upper lobes, and in the upper and back parts of those lobes, that, in nineteen cases out of twenty, and in more than that proportion, we meet with tubercles when they are few. It is in the same part that they are largest, and most numerous, when they are scattered throughout the whole lung. It is here, also, that they first ripen, and grow soft, and become ready for expulsion through the bronchi and trachea: consequently, it is here that we have the most frequent, the most numerous, and the largest *vomicæ.* And the number and magnitude of the tubercles and of the vomicæ gradually diminish from the summit of the lungs downwards.

Now these are not merely *curious* facts: they have a most important bearing upon the diagnosis, in cases that might otherwise be doubtful. It is a rule which has but few exceptions—just enough to establish its claim to be a rule—that the favourite habitat of pulmonary tubercles is the upper part of the superior lobes of the lungs; and I may remind you that the converse of this is true (though with more numerous exceptions) of common inflammation of the lungs. Pneumonia affects by preference the lower lobes; it does sometimes commence in the upper, but that is comparatively rare. When it occupies the superior lobes, it generally has arrived there by travelling upwards from the inferior. You will at once perceive the practical advantage of knowing these points of contrast.

Some modern French writers, relying upon tabulated results of their own personal experience, have supposed that the left lung is much more obnoxious to tubercular disease than the right. Thus Louis, whose volume is the result of immense labour in observing, and is full of the most instructive matter, had met with *seven* cases in which tubercles were confined to a single lung: in *two* of the seven cases it was the right lung that was thus exclusively affected, in *five* it was the left. Of 38 instances in which the upper lobe was totally disorganised by the disease on one side, 28 were of the left, and only 10 of the right. Eight times he had known the pleura perforated by the extension of tubercular disease; and seven times out of the eight the perforation happened on the left side of the chest. So also Reynaud met with 27 cases of pneumothorax on the left side, for 13 on the right. Had the inference drawn from these comparative numbers been correct, it would have disclosed a fact curious enough in itself, but not of such practical value as the last. But it was not correct; as we learn from a more comprehensive statistical calculation. Dr. T. K. Chambers tells us, in his 'Decennium Pathologicum,' that 'among 517 consumptive persons examined in the dead-house of St. George's Hospital during the ten years ending December 31, 1850, there were 171 in whom tubercle was found in its crude unsoftened state. Of these 32 had tubercle in the right lung and not in the left; 21 in the left and not in the right; and 117 had tubercle in both lungs.' Again, 'of 343 persons in whom the tubercular matter had softened and formed vomicæ, 56 had vomicæ in the right and not in the left lung; 59 in the left and not in the right; and in 224, vomicæ existed on both sides. This discrepancy may serve to illustrate and enforce the cautions which I gave you in a recent lecture, as to statistical reasoning; namely, that conclusions drawn from numerical statements can be *legitimate* only when they relate to things identically the same in kind; and *trustworthy* only when those things are sufficiently numerous. If we could have accurate statistical information about every case of tubercular phthisis that has ever occurred, we should, in all probability, find that the two lungs are subject alike and indifferently to tubercular disease. I mentioned, in a former lecture, Andral's conclusion, derived from the observation and collation of 210 examples, that *pneumonia* is more than twice as common on the right side as on the left. This computation challenges more lawfully our confidence, inasmuch as it is based on a large number of instances. M. Lombard, of Geneva, found the ratio somewhat less than Andral's, but still great. Of 868 instances of pneumonia, 413 occurred on the right side alone, 260 on the left alone, and 195 on both sides at once. That is, there were more than three on the right side alone, for every two on the left alone: and that statement still holds true of the whole number of cases, Andral's 210 and Lombard's 868 taken together; that is, of 1,078 cases of the disease.

LECTURE L.

Phthisis, continued. Vomicæ; adhesions of the pleuræ; ulceration of the larynx and trachea—of the intestines; fatty liver; waxy liver; auscultatory signs of a vomica; gurgling, cavernous respiration, pectoriloquy, cracked-pot sound; general symptoms of phthisis; cough, expectoration, hæmoptysis, dyspnœa, pain, hectic fever, frequency of pulse, diarrhœa, wasting, œdema, aphthæ.

We were engaged, when we separated yesterday, in investigating the *morbid anatomy* of tubercular consumption. Bear in mind how and where the tubercular matter, which is the essence of that disease, is deposited in the lungs; that it occupies by preference their upper lobes, and the upper and back part of those lobes; invading gradually the lower lobes, from above downwards, as the disease advances. Both lungs are, commonly, affected at the same time, though in unequal degrees. Among one hundred and twenty-three instances of phthisis, Louis found that the tubercles were limited five times to the left lung, and twice to the right. The tubercular matter, once deposited, may remain quiet and unchanged for some time, but in general it increases in quantity, until at length, according to Niemeyer, it liquefies by a sort of fatty degeneration; or according to Cardwell, inflammation of a low and scrofulous character arises in the pulmonary substance in immediate contact with the tubercles—or in the areolar tissue involved in the larger agglomerated masses—in consequence of which inflammation, a sort of suppuration takes place. Anyhow, the tubercular matter becomes soft, and breaks down, and is ultimately expelled through the bronchi, trachea, and mouth. The vacuities left in the lung after this process of expulsion are called cavities, excavations, caverns, vomicæ. And I go on to consider certain points of practical interest connected with these vomicæ.

In the first place, as you may see by the specimens upon the table, they vary greatly in size; as they also vary much in number. They may be no bigger than, or not so big as a pea; they may be large enough to contain a pint or more of fluid. Sometimes the whole of the upper lobe is converted into a bag of this kind. These large cavities are not met with in the lower lobes. They are formed by the union of several that were smaller; so that they are often of very irregular shape, and divided, as it were, into chambers, by imperfect partitions, or by bands which cross them in various directions. Opening into the cavity, there is always one, and there are generally several, pervious *bronchial tubes*, which seem as if they had been cut off just where they enter the cavity. But you never, or very seldom indeed, find a *blood-vessel* thus opening into the cavity. And the reason of these differences is plain enough. It is not, as some modern authors have fancied, that the arterial and venous tissues possess a degree of vitality which resists or avoids the destruction in which the surrounding tissues are involved. That principle may be applicable to other cases, but it is not applicable to this. The opinion I am now referring to proceeds on the supposition that the bands which sometimes cross the cavities are really blood-vessels that have escaped the disorganising process. Such seems to have been the notion entertained by Bayle; and it was more expressly advanced by Cruveilhier. But the truth is, that these bands rarely *contain* blood-vessels: and when they do contain them, the blood-vessels are mostly *impervious*. In one hundred and twenty-three cases, Louis found pervious blood-vessels in the bands no oftener than five times.

The true reason why bronchial tubes open into these cavities, and blood-vessels do not, is to be found in the natural differences between the two sets of vessels, in respect of their structure, and in respect of the fluids that pass through them. The blood-tubes yield readily to external pressure. Many of them are pushed aside and flattened by the progressive increase of the tubercular matter around them; some possibly are obstructed by its gradual accumulation within. In either case the stagnant blood coagulates, and the vessel is obliterated to some distance from the place of the original obstacle; just as you know a clot forms, and seals up an artery which has been tied during life, for some little way anterior to the ligature. But the bronchial tubes are neither so easily compressed, nor do they carry any coagulable fluid. They are filled up by the tubercular or by the cheesy matter in which they lie, and when that matter breaks down, they are expectorated with the rest of the detritus. Meanwhile their open mouths, on the hither side of the point where the tubercular matter stopped, remain, and afford a channel through which the same matter, after it has become soft, finds its way towards the trachea. Occasionally, indeed—but that, I repeat, is of rare occurrence—a considerable blood-vessel *does* get laid open during the formation of a vomica, and then copious and fatal hæmorrhage may ensue. Small sac-like aneurisms sometimes form in branches of the pulmonary artery running in the condensed walls of the cavity, and these little aneurisms are liable to rupture. In six out of eight cases of fatal hæmoptysis carefully examined after death at the Brompton Hospital within nine months during the year 1868, an aneurism of a pulmonary blood-vessel was the source of the bleeding. Occasionally also an oozing of small quantities of blood takes place from the inner surface of the cavity, tinging the matter expectorated. Hæmoptysis is certainly more frequent before the softening and expulsion of tubercular matter than afterwards. Among eighty-seven cases of phthisis, Louis saw hæmoptysis four times only in the last days before death; and twice only was the bleeding considerable in amount. There was no fatal case.

When the vomica is first formed, its inner surface is soft and ragged; and if other softening masses are in the immediate neighbourhood, the cavity goes on enlarging; that is, two or more vomicæ coalesce. If, however, there happen to be no more tubercles or cheesy deposits thereabouts, the cavity may remain stationary. Its inner surface then becomes smoother; and something like a membrane forms upon it; and sometimes a puriform fluid is poured out by this surface, and sometimes not. Generally the pulmonary tissue around such a cavity is condensed and solidified; partly perhaps by crude tubercular matter which it contains, partly in consequence of the inflammatory process of which it has been the seat during the softening of the tubercles. It is important to bear in mind this fact of the condensed solid state of the lung immediately surrounding a vomica; for it explains certain peculiarities met with in the symptoms.

There is another point of much interest connected with these vomicæ. When they occur singly, without other tubercles or cavities (which, though a rare thing, does sometimes happen); and when occurring thus singly they have been completely emptied; they may gradually contract, and ultimately become obliterated. This approach of their sides leads to a puckering of the pleura on the surface of the lung: and, on the other hand, a puckering of the surface indicates that beneath it there is probably a shrunken, or an *obsolete* vomica. The central part of the diseased spot is converted into a substance resembling cartilage; and the appearance it presents is called a cicatrix; and really it deserves that name. The process which has gone on is a process of natural recovery: and the recovery would be complete and permanent, if no fresh deposit of tubercular matter took place. Too often, however, the tubercles multiply, until at length their number, or size, or effects, became incompatible with the further continuance of life.

This, then, is one way in which tubercular disease, *limited* to *one small* portion of the lung, *may* be eliminated, and the part which it occupied undergo a kind of repair. But the disease, *when so limited*, *may* cease in another way. The moister parts of the morbid secretion may be absorbed; and the earthy salts it contains may concrete; and the whole be converted into a shrivelled, hard, chalky or mortar-like mass, which sometimes is coughed up, sometimes, in favourable cases, remains for years in the lung, an inert and almost harmless body.

Let me state, while I think of it, that the expectoration of these chalk-like concretions, denoting, as it usually does, the existence of pulmonary consumption, marks at the same time the chronic character of the case. I am acquainted with a gentleman who, though delicate, enjoys a very fair share of health, and who has for years been coughing up, at intervals, little branching fragments, like bits of white coral, consisting principally of carbonate and phosphate of lime, and evidently moulded in the smaller bronchial tubes.

Take notice that depressions and puckerings on the surface of a lung do not always indicate the presence of a cicatrix. or of an obsolete tubercle, beneath them. It is obvious that they may, and in point of fact they often do, result from mere lobular collapse, or from circumscribed atrophy of a portion of the lung from any cause; the neighbouring tissue being sound, or perhaps emphysematous.

You already perceive that tubercular phthisis is not necessarily a mortal disease.

When the tubercles are numerous—or rather when they lie near to the surface of the lung, as, of course, they are likely to do when they *are* numerous—they very generally give rise to dry or adhesive pleurisy. So that, in a person dead of consumption, it is a very rare thing to find the lungs free from adhesions to the ribs. I mentioned before that this attachment of the lung to the walls of the chest affords a protection against a much more formidable condition; namely, perforation of the pulmonary pleura, and the escape of tubercular matter and air into the serous cavity; producing that worst kind of pleurisy which implies pneumothorax. That the pleurisy and adhesion are consequences of the presence and irritation of the tubercles, appears from this:—that, for the most part, the extent and the situation of the adhesions correspond with the extent and situation of the tubercular deposit. Thus, a single spot of adhesion has been seen to unite the costal and pulmonary pleuræ exactly opposite a solitary mass of tubercle which lay near the surface of the lung. As the summit and back part of the upper lobes are most thickly set with tubercles, so here also is the adhesion the most constant, and the most firm. You will often find the upper part of the lung invested with a thick cap of false membrane: and the connexion between the pleuræ so tough, that the lung breaks down in the attempt to separate them.

To show you that these statements—which have long been familiar to those much conversant with disease and with morbid anatomy—to convince you that they are strictly borne out by numerical or tabular facts, I may again have recourse to Louis. He tells us that in 112 cases which he himself examined of persons dead of consumption, and having tubercles in their lungs, there was but one in which both lungs were free from adhesions. In eight cases the right lung was exempt from them; and in seven cases the left. Again, in 25 other instances, there were either no cavities, or very little ones; and the adhesions were accordingly of small extent, and easily broken down. In the remainder there were large vomicæ, and the adhesions were extensive, dense, and firm.

Such is a sketch of the changes which take place in the *lungs*, in consequence of the deposition of tubercular matter in them, and of the changes which that matter undergoes. But the air-passages that lead to the lungs are very liable to become implicated as the disease proceeds. The mucous membrane of the larynx and trachea ulcerate: and when the morbid condition of the larynx gives rise to prominent symptoms, and especially (as it is apt to do) to hoarseness and loss of voice, the disease is sometimes called *laryngeal phthisis*. But there is no such disease, that I know of, existing by itself. I mean, that *scrofulous* ulceration of the larynx and trachea occurs only when the lungs are affected with tubercles. It is, however, sometimes a very early symptom. It is said that when ulcers are met with in the trachea, they are often found on one side of it only; on the side, viz., which corresponds with the diseased lung, or with that lung which is most diseased. And again, that when some of the bronchi are found red internally, and even ulcerated, these appearances are confined to those bronchi which communicate with cavities; and do not occur in the bronchial canals leading to crude tubercles. It is towards the back part also of the trachea that the ulcers, especially when large, are most commonly observed; the floor of that channel as the patient lies supine. And when the epiglottis is involved in the mischief, the ulcers are situated, almost always, on its laryngeal surface alone. Can their formation be influenced by the contact of the matter which is expectorated, in its frequent passage over the mucous membrane? Probably the fact that the little mucous glands wherewith the membrane is provided are most numerous at the posterior part of the trachea and bronchi, and that these glands are especially prone to ulcerate, furnishes the truer explanation.

In respect of these points also Louis has made comparative observations. Among 180 persons who died of some chronic disorder, *not* phthisical, he once only met with ulceration of the larynx; whereas of those who perished of consumption, about *one in every five* had ulceration of the epiglottis or larynx, and nearly *one in three* had ulceration of the windpipe. Hence it would appear that, if we except the effects of the syphilitic poison upon the larynx, ulceration of that part is almost peculiar to phthisis pulmonalis.

I have told you that tubercular consumption is not merely a lung disease. Its local ravages are most obvious indeed in the thorax; but it leaves in the abdomen also traces of its destructive agency not less definite and scarcely less constant. You know that the surface of the intestinal canal is strewed, throughout, with separate mucous follicles; and that the lower portion of the ileum is furnished with other follicles, collected together in oval or circular groups. When I come to speak of specific fevers I shall have much to say about the changes which these little glandular bodies undergo, in one at least of those diseases. These same glands are the frequent seat of tubercular deposit in phthisis. Here and there you may see a solitary yellow tumour, not larger than a hemp-seed, projecting from the inner surface of the bowel. In other places the ripened little tumour has burst, the tubercular matter is gone, and a ragged roundish ulcer remains. More frequently the aggregated glands are affected; and the ulceration, in them, varies much in form and extent. It often involves the whole patch, and then the shape of the ulcer is more or less elliptical. Louis met with ulceration of these glandulæ agminatæ in five-sixths of all the fatal cases of phthisis that he examined. Ulcers of greater magnitude were very nearly as common in the large intestines. And it is worthy of notice that, the disorganising process being in these cases slow, nature has time (if I may use such metaphorical language) to provide against the threatened perforation of the gut. The tissue that forms the base of the ulcer, whether it be the muscular or the peritoneal coat, is thickened and vamped; or the bowel becomes adherent to some contiguous surface. Once only in my life have I known scrofulous ulceration, in phthisis, penetrate the serous tunic, and allow the contents of the intestine to escape into the sac of the peritoneum.

You ought to be aware—although the facts possess, as yet, no great practical value—that the stomach is often much enlarged and thinned in those who die of consumption; and that the liver is very apt to undergo remarkable changes, almost yet not altogether peculiar, I believe, to that disease. It, too, enlarges, and there are two varieties of its enlargement. In the first, the liver becomes full of adipous matter, greasing the hands and scalpel of the anatomist, and yielding when heated an oily substance, which makes a grease-spot on paper placed in contact with it. The whole gland partakes in the alteration, is of soft consistence, loses its natural red tint, and assumes a pale fawn colour. In three years Louis met with this fatty liver, as it is called, 49 times; and 47 of the patients died phthisical. It occurred in one-third of the whole number of the victims to consumption; whereas, among 223 cases, not phthisical, there were two examples only of this hepatic change. It is more common in women than in men.

The second kind of enlargement is produced by the infiltration of the hepatic tissue with an albuminous material. The cut surface of a liver thus altered has a whitish and glistening appearance; hence it is called the *waxy* liver; and by the Germans, from its resemblance to bacon, the *lardaceous* liver. The waxy liver so far resembles the fatty, that it is frequently observed in connexion with scrofulous disease; and that it is large, sometimes exceedingly large, with a smooth surface, and a blunt lower edge. Both these forms of hepatic enlargement proceed insidiously, with little or no pain or tenderness. Their presence is revealed during life by no distinctive symptoms, except that the enlargement belonging to them may at length be ascertained by percussion and pressure with the fingers; and that the waxy liver sometimes causes ascites.

Let us now inquire what modifications of the healthy sounds of the chest arise from the altered conditions of the lungs in phthisis. Most of them are such as you would naturally expect. Whether a portion of lung be rendered solid by common inflammation, or by the presence of tubercles in it, the result, so far as the auscultatory signs are concerned, will be the same. In such a piece of lung, supposing the solidification complete, no vesicular breathing can be heard; but bronchial breathing and bronchophony will be audible, in each case, if the solidified portion enclose a considerable bronchus, and come near the surface of the chest. And percussion will give a dull sound, whether the lung lying beneath the part struck be hepatised, or blocked up by tubercular matter. On these points, therefore, after what was said in a preceding lecture, I need not dwell. But the excavations—the empty or half-empty vomicæ—these are something new. We have hitherto met with no condition exactly similar to that of a *large* cavity. And accordingly I have to make you acquainted with two or three new sounds: or sounds which are modifications of those formerly described, and in most instances sufficiently distinct from them to have acquired peculiar names. You will remember that what we have called large crepitation depends upon the passage of air through liquids; the liquids being contained in tubes; those tubes the bronchi and their ramifications. But when pus or vitiated mucus, or liquid matter of any kind, is collected in a *vomica*, which communicates freely with the trachea through pervious bronchi, the bubbles produced by the entrance and exit of air will be still more numerous and large; and a sound is then produced which the word *gurgling* expresses well. Laennec calls it *gargouillement*. This sound is heard, too, in a circumscribed space; and not diffused, as large crepitation usually is. Wherever, therefore, we hear gurgling during respiration, or during the act of coughing, there, we conclude, exists a *cavity*. But the cavity is not necessarily a *vomica*. In ninety-nine cases out of a hundred it will be so; but in the hundredth case perhaps it will not. Bear in mind what was formerly stated of dilatation of the bronchi: how sometimes they terminate in a considerable globular expansion; sometimes belly out and contract again several times alternately: and you will see that cavities containing liquid, or liable to contain liquid, belonging equally to the one condition and to the other, and the sound in question depending solely on the intermixture and agitation of air with liquid in a cavity, we cannot be sure from *mere* gurgling respiration, or gurgling cough, that we have a *tubercular* excavation beneath our ear; or even that the case is one of consumption. Gurgling may also proceed from that very rare morbid condition, abscess, the result of common inflammation of the lung. These constitute the only sources of fallacy in the matter. The fallacy seldom interposes; but it does sometimes interpose; and therefore it must *qualify* our conclusion from this symptom of gurgling, with respect to cases otherwise doubtful.

Again, the vomica may be empty of liquid; and then we hear, as the patient respires, not vesicular breathing of course, nor yet exactly bronchial breathing; it is something more than that when the cavity is large, something different in character from it when the cavity is small: but whatever the character of the sound, as we believe it to take place in a vomica or cavern, we call it *cavernous respiration*. It is a hollow sound, especially when the cavity is of considerable size; an exaggeration of mere bronchial respiration. But the cavity may be small. The moment a portion of softened matter is separated and discharged through a neighbouring bronchial tube, the cavity has commenced; and the sound produced in these little cavities during the breathing may be of various kinds. It may be, as it often is, a click, like the opening and shutting of a valve; or a chirp; or a creaking; or like many other well-known sounds: but, as all these sounds, under certain circumstances, denote the formation of a vomica, it is best, for simplicity's sake, to call them all by the same name—cavernous respiration.

Dr. Latham explains in a few words the causes of these differences. 'The varieties of cavernous breathing are doubtless owing to different sizes, and forms, and situations of cavities, and to different conditions of the surrounding lung. A cavity may be very large or very small. Several bronchi may open into it, or only one. It may be a simple cavity, or it may have many chambers. Its sides may be condensed and equal, or rough and ragged. The lung around it may be solid and indurated, or pervious and vesicular. It may be near the ribs, or far from them: adherent to, or separate from the pleura. It is quite obvious that these different circumstances are calculated to modify the sound, which will, nevertheless, be always such as indicates a cavity.'

The peculiar sound just now mentioned—the click or sucking sound—occurring towards the end of a long-drawn inspiration, is supposed to depend upon the sudden opening of a bronchial tube, of which the sides had previously been held in contact by pressure, or by sticky mucus, and which communicates with a softening mass. When it is heard persistently in a portion of lung otherwise suspected, and belonging to an upper lobe, it announces, almost with certainty, that a cavity has formed or is forming; and while thus significant in itself, it certifies also, whenever it has been noticed before the occurrence of *gurgling*, that the gurgling of which it was the herald proceeds from a vomica. But in many cases the condition necessary for the production of the click is never present: and since, when present, it is always a transitory condition, the actual click may easily escape detection.

A cavity may be *so* large, and of such a kind, as to yield the metallic sounds which are apt to be heard in pneumo-thorax. I show you a cavity in which those sounds were actually heard, most distinctly, by many persons, during the patient's life, while he was under my care in the Middlesex Hospital. I was certain beforehand that these sounds proceeded, not from the cavity of the pleura, but from a tubercular excavation. Once subsequently, in a patient who was dying of phthisis and diabetes, I have heard the same sounds: also, I am sure, in a vomica. That patient insisted on leaving the hospital, and I lost sight of him before he died.

I promised, when speaking of these metallic sounds as arising (as they much more commonly do) from pneumothorax—a collection of air, or of air and a liquid together, in the sac of the pleura—I promised to point out the circumstances whereby you may tell which of the two conditions in question the sounds denote. Both of the conditions imply, in general, the existence of tubercular phthisis; and therefore the observation of the ordinary symptoms of phthisis will not help us much.

Now, in the cases seen by me, there were two circumstances that stamped them as being cases in which the sound proceeded from a cavity *in* the lung, and not *exterior* to it. One was the *situation* in which the sound was *invariably* heard. The other was the absence of excessive resonance when that part was percussed. You know that when air is contained in the pleura itself, the sound yielded on percussing the chest in the corresponding spot is quite tympanitic, like that of a beaten drum. But it is a well-ascertained fact, (though contrary perhaps to what you would suppose,) that in nine cases out of ten, the sound is duller over tubercular cavities than over sound lung. The explanation of this fact is simple enough. It is that the layer of lung which still remains in such cases, thick or thin, is dense and solid, and damps the sound which the vomica might otherwise make resonant. But then again the situation of the metallic sounds was a guide. They occupied the upper part of the chest: the very part where vomicæ are wont to be the most common, and the largest: and moreover a part where pneumothorax seldom or never exists. In phthisis the summit of the lung is often covered with a cap of false membrane, which binds it to the ribs: and this, as I observed before, is the main reason why perforation of the pleura pulmonalis is so rare in that disease; and it is also the reason why, when it does take place, it seldom takes place at or near the apex of the lung. In truth it is found by experience that (though the rupture of the pleura *may* happen in any part) the place where it usually occurs is in the lower and back part of the upper lobe of the lung, opposite the angle of the third or fourth rib; that is, just beneath the edge of the false membrane by which the summit of the lung is generally adherent. But the sound, in the cases I refer to, was *invariably* heard at the very top of the chest. It did not shift, as that of pneumothorax may often be made to shift, when the patient changed his posture. Attention to these points will always lead you to a secure diagnosis. You may say, perhaps, 'The complaint being in each case a mortal one, what is the use of so much refinement?' Why, there is this utility in it. We may sometimes, as I stated before, give great relief to the patient, and save his life for a time, by tapping the chest in pneumothorax. Air may get in with each inspiration, and threaten immediate suffocation; and the thorax being punctured, it will issue in a blast. But no one would think of tapping a tubercular cavity.*

To give out the amphoric resonance and the metallic sounds, the vomica must, I presume, be a *large* one. That which is before you, the only one I ever *saw* in which those sounds had been heard, is very large. Its inner surface is smooth: it adheres to the ribs externally by at least two-thirds of its circumference: and the medium of adhesion is very thin. Quite low down, a single bronchial tube, of about the third division, may be seen to enter it.

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* In this the author finds that he was mistaken. The operation has since been proposed and practised. That it can meet with much success, or favour, he does not yet believe.

So much, then, for the modification, by a tubercular cavity, of the sounds heard during *respiration*. But the *voice* will also be modified, if the cavity be of considerable size, and near the surface, and have dense walls, and be empty. Then we hear, in that part, when the patient speaks, the sound which is called *pectoriloquy*: as if the voice proceeded from the chest. The words are distinctly articulated into the ear of the listener. But I need not trouble myself or you by attempting to *describe* pectoriloquy. You may any day hear the exact sound that word is intended to denote, by placing a stethoscope over the trachea of one of your friends, applying your ear to the other end of it, and getting him to speak: just as you may obtain an exact notion of bronchial respiration by listening then to his breathing.

For some time after the first appearance of Laennec's great and original work on the diseases of the chest, pectoriloquy was deemed to be the pathognomonic and infallible sign of a vomica. 'Oh,' the young auscultator would say, 'I detect pectoriloquy beneath the clavicle. There can be no further doubt about the nature of the disease. My patient has not only tubercles, but a cavity in his lung.' So I long thought; and so some, I fancy, think still. Yet the evidence afforded by mere pectoriloquy of the presence of a vomica, or even of the presence of tubercles, is far from being certain or trustworthy. Experience had taught me this before I knew that many others, studying under the same schoolmaster, had learned the same truth. Among my hospital patients many years ago was a man who laboured under phthisis. Percussion gave a dull sound under his right collar-bone, and in the same spot loud and distinct pectoriloquy was audible. I well recollect inviting the particular attention of the pupils to this case, as affording an exquisite specimen of pectoriloquy; and I predicted very confidently that after the patient's death, which was obviously at hand, a large excavation would be found in the summit of his right lung. My prediction did me no credit. The *left* lung indeed was hollowed by cavities, but there was nothing like a cavity in the right. The upper part of the lung was thoroughly and uniformly solid; filled with hard, gray, tubercular matter. The large bronchial tubes were pervious, and the voice descending into them had been conducted by the solid lung with perfect and almost painful distinctness to the listener's ear. This was a useful lesson to me: and I mention it that it may be a lesson to you. Remember that solidification of the summit of the lung will modify the sound of the patient's voice, very much in the same manner as might a large vomica there situated. It is stated, indeed, and perhaps truly, that a practised ear can discriminate between the loud, diffused, though articulate, resonance of the voice produced by solid lung, and the circumscribed, whiffling pectoriloquy of a cavity. But the distinction is too nice for the average of ears. Now since the pulmonary tissue may be rendered dense and solid by other causes than tubercles, pectoriloquy does not always indicate the existence of consumption. The fallacious condition does not often occur; for common inflammation is seldom limited to the upper part of the lung; and the whole of that part is seldom completely hardened by crude tubercles, or by cancer. But whenever it does occur, it is apt to mislead or puzzle. I was consulted last year about a well-known member of our profession in whom this phenomenon of pectoriloquy was strongly marked. Two excellent auscultators had been led, by this symptom, to the belief that a cavity existed in the lung. Remembering the case I have just mentioned, and learning that the patient had been ill for a few days only, and had not previously suffered cough, nor any apparent pectoral complaint, I was of opinion that the summit of his right lung had become hepatised by acute pneumonia. And it was so. The patient died; and the diagnosis I had formed was verified upon inspection of the lung. Dr. Latham relates one or two examples to the same purpose. Dr. Stokes goes so far as to consider pectoriloquy the least important and most fallacious of all the physical signs of phthisis. Taken alone (he says) it is absolutely without value. Sir John Forbes had come to similar conclusions. Certainly cavernous respiration is a much more alarming sound.

Wherever actual pectoriloquy, from a cavity, is heard, there also will be heard cavernous respiration. But the converse of this is not necessarily true. There may be, and there often is, cavernous respiration and a cavity, yet no pectoriloquy. The cavity is not large enough, or not near enough to the surface of the chest, or not of such a kind as to reverberate the voice.

Often when pectoriloquy is absent, and cavernous respiration is doubtful, and even gurgling cannot be heard (because the communication with the bronchi is not free), a slight splashing sound will occur when the patient coughs: nay, you may sometimes hear it, if he hold his breath, with every beat of his heart, which causes a little succussion in the cavity: but its contents must then be thin.

When the sounds that I have been engaged in describing are well marked, they denote the existence of a vomica. The only source of fallacy is that which I formerly mentioned; the same sounds may arise from a cavity in the lung, whatever be its nature; and therefore they may arise when the bronchi are expanded into cavities. But I repeat that this is a deceptive condition, which you cannot calculate upon meeting with often.

There is yet another sound, sometimes and sometimes only to be evoked by *percussion*, which also is tolerably conclusive of the presence of a vomica. It is called, in translation from the French, by the queer name of the *cracked-pot sound—bruit de pot fêlé*: a dullish, rather metallic percussion sound, accompanied by a slight chinking or hissing noise. You are no doubt familiar with the schoolboy exploit of forming a hollow space between the palms of the two hands, and striking the back of one of them against the knee. When a portion of the included air is thus forcibly driven between the fingers by the blow, a sound is produced which resembles somewhat the chinking of money. This cracked-pot sound, resulting sometimes from the percussion of a subclavian space, testifies to the existence of an empty or nearly empty cavity, having yielding walls, and communicating freely with the bronchi. The blow causes the sound by forcing air from the cavity and against the surface of the air-passages. Hence, as Dr. Walshe has pointed out, if the mouth and nose of the patient be tightly closed, the outward rush of air is prevented, and the sound is no longer heard. The same writer states that a sound of very similar quality is sometimes producible in other morbid conditions of the lung; and that if the chest of a crying infant be percussed, after expiration, the resonance will have the cracked-metal note though the lungs are perfectly sound. You must not therefore put absolute trust in the significance of this curious stroke sound.

I dwell on these signs of a cavity because they disclose a condition of unspeakable moment to the patient, whom it stamps as being the subject, and therefore probably the doomed victim, soon or later, of pulmonary consumption.

Other physical signs there are still, varying in value and in meaning in particular cases of real or suspected phthisis—such as flattening of the walls of the chest below the clavicle, diminished movement of that part in respiration, increase of vocal vibration in the same part, extension of the space over which the sounds of the heart are audible, murmurs in the subclavian artery—of which sounds and signs it would be tedious and practically uninstructive to give here a lengthened description. You must study them, and learn from them, in the living patient.

When the sounds are not well marked, take time before you pronounce a decided opinion respecting them. Strong bronchophony comes very near to weak pectoriloquy: bronchial respiration may closely resemble some varieties of cavernous breathing: large crepitation, confined to a small spot, may simulate gurgling. It is better, when the sounds are thus equivocal, and when they may denote conditions so very different in their nature and tendency, to suspend one's judgment, and to give a guarded opinion. A little time in such cases will clear away the doubt.

It will be well also, when you are about to auscult unsound or suspected lungs, that you should bear in mind a few general facts which have been ascertained respecting the signs proper to the upper lobes of lungs that are *healthy*.

The sound of a whisper is perhaps always, and of the voice very often, louder just below the right than below the left collar bone.

Frequently vocal vibration is more strongly felt, and the sounds of the heart are more loudly heard, in the right than in the left of those spaces.

The same may be said of a forced *ex*piration in the right as compared with the left supra-spinal fossa.

On the other hand, the breath sounds, both in ordinary and in forced *in*spiration, are often louder in the left than in the right subclavian space.

These facts, and others more minute with which I will not now perplex you, were ascertained on the careful examination, by four skilled auscultators, of the chests of fifty healthy men, between 18 and 30 years old, selected from among the inmates of the Millbank Penitentiary by the late Dr. Baly.

I have now to consider the *general* symptoms of this most afflicting disease: and while doing so, I shall point out how the physical signs confirm or confute their language, in cases which might otherwise be doubtful.

The general symptoms of phthisis are cough, expectoration, hæmoptysis, dyspnœa, pains in the chest, hectic fever, frequency of pulse, diarrhœa, wasting; and there are some other symptoms which mark often some of its stages, and to which I shall incidentally advert. I shall speak of them all as briefly as is consistent with clearness.

Cough is one of the earliest symptoms of consumption; and it is that which commonly first attracts the attention, and awakens the fears, of the patient or the patient's friends. Generally at first it is slight, occasional, and dry: it occurs upon the patient's getting out of bed in the morning; or if he make any unusual exertion in the course of the day. It feels to him as if it were caused by irritation about his throat. Sometimes it will cease for a while, as in the warm weather of summer, and recur in winter when the external temperature is lower. By degrees it begins to be troublesome in the night; and to be attended with more or less expectoration of mucus.

Now when such a cough steals upon a person gradually, and when no reason can be assigned for its occurring, that circumstance alone is enough to excite suspicion as to its true nature and cause. But chronic cough may exist without any tubercular disease of the lungs: as you well know. It may depend upon a disordered state of the *stomach*; the *pneumogastric* nerve may be irritated *there*. It may be the cough of mere *chronic catarrh*; it may result from disease of the *heart*; it may be the nervous, barking, importunate cough which I formerly mentioned as of frequent occurrence in *hysterical* girls. And bearing these circumstances in your mind, you will enquire, and you will generally make out without much difficulty, whether there be any unnatural or deranged state of the digestive organs; or chronic catarrh; or cardiac disease; or hysteria. If the cough be referable to none of these sources, and, above all, if there be any obvious or suspected strumous taint in the patient's constitution—such chronic cough cannot but inspire prospective anxiety.

I may observe here, that as chronic cough may exist when there is no consumption; so consumption may sometimes exist, and even prove fatal, and large portions of the lungs may be disorganised, without there having been any cough; or at least without the occurrence of enough cough to draw the notice of the patient or of his friends to it. This is not common, however: cough is usually present, more or less, during all the stages of phthisis, and it is often that symptom which most distresses and harasses both the patient and his family.

Great attention used to be paid to the *expectoration* in cases of suspected phthisis. It was thought that if a patient spat pus, he was in a state of confirmed consumption: and whole volumes have been written, and prizes awarded to their authors, respecting the means of distinguishing pus from mucus. But we now know that, so far as the diagnosis of phthisis is concerned, this is a very idle inquiry. The presence or absence of pus in the sputa affords no test at all of the presence or absence of consumption, nor are there any trustworthy means, even when the microscope is appealed to, of distinguishing between pus and morbid mucus.

A portion of the matter expectorated comes from the surface of the bronchi, and consists of altered mucus; and therefore the sputa brought up in phthisis, and the sputa brought up in bronchitis, are, in a certain degree, the same. These are partly composed of a ropy transparent fluid, in which opaque masses of a yellow or greenish colour are seen to float; and intermixed also with which there may be a good deal of froth. The froth is a measure of the difficulty with which the mucus is brought up: and it is usually less abundant and conspicuous in phthisis than in bronchitis. The heavy sage-leaf sputa that we sometimes see, belong to both diseases. Niemeyer affirms that sputa marked with fine distinct stripes of a deep yellow colour, bespeak the presence of catarrh of the smallest bronchi, and the consequent risk of its extension to the air-cells, and causing deposits which may end in cheesy metamorphosis and the generation of tubercles.

Do not be too hasty in concluding that little curd-like fragments involved in the expectorated mucus, are bits of tubercular matter, and therefore decisive of the nature of the case. Small opaque specks of that character often come from the follicles of the tonsils. This makes the signification of these fragments equivocal. The sputa *most* characteristic of vomicæ consist of globular, gray, flocculent masses, which look like little portions of wool more than anything else. *Nummular* sputa the French call these, because when spat into a vessel not containing water, they assume a flat circular form, like a piece of money, and remain separate and distinct from each other. When they are spat into a glass of water, you perceive that some of them subside to the bottom—some float on the top, suspended, apparently, by healthier mucus in which they are entangled, or by bubbles of air—and some remain stationary at different depths. When stirred and agitated in the water, they render it slightly milky. This kind of expectoration commonly marks a confirmed and advanced state of the disease; but it will continue for weeks sometimes. It is not *perfectly* pathognomonic, but *nearly* so. On one occasion I found expectoration of this nature from a man whom I did not very diligently examine by my ear; and I set the case down as one of phthisis chiefly on the observation of that symptom. The patient evidently had not long to live. Our resident medical officer at the hospital, Dr. Corfe, had more time to explore the condition of the chest: and he came to the conclusion that the disease was not tubercular phthisis, but extensive chronic bronchitis: and sure enough he was right. When we came to examine the lungs after the patient's death, not a vomica or a tubercle could be found. I am satisfied that there is no kind of expectoration which to the naked eye indicates phthisis with *perfect* certainty; but that which I have just been describing very *seldom* occurs except in phthisis. Louis appears to have noticed these round, separated, woolly masses twice only unconnected with tubercles: and once the same thing occurred to Chomel: so that, when the other symptoms are obscure and doubtful, this will materially augment the gravity of the prognosis. Flies appear to be more attracted by the sputa of phthisis than by any other sputa.

Practised microscopic observers are sometimes able, I believe, to discriminate 'tubercular corpuscles,' and portions of broken-down pulmonary tissue, in the expectoration of phthisical patients. In this method of diagnosis I must confess that I am inexpert.

Hœmoptysis is a kind of expectoration—the expectoration of *blood*. I have already spoken of this symptom as connected with phthisis, and have stated my belief on that subject; viz.: that if a person spit blood who has received no injury of the chest, in whom the uterine functions are healthy and right, and who has no disease of the heart or great bloodvessels, the risk of pulmonary consumption in that person is fearfully great. Excluding cases of amenorrhœa, and of mechanical injury to the thorax, Louis did not meet with a single example of hæmoptysis among twelve hundred patients, except in such as were phthisical.

The gravity of this symptom is not much relieved by the adoption of Niemeyer's views—to which I briefly alluded—respecting the way in which hæmoptysis may sometimes give rise to phthisis. He maintains that most cases of rapidly-fatal consumption occurring in persons previously healthy, immediately after an attack of hæmoptysis, 'cannot be otherwise interpreted than by assuming that the blood which remained behind in the bronchi and alveoli has led to a pneumonia undergoing cheesy transformation, the retained blood and the products of inflammation afterwards breaking down.'

Threatenings of mischief of this sort may be discerned, he states, in the springing up of febrile symptoms soon after the hæmoptysis, especially in a marked rise in the temperature of the body; and not seldom in corresponding signs of the local inflammation.

Prior to the age of fifteen, hæmoptysis, even in phthisical children, is extremely uncommon.

Dyspnœa is not a very important symptom in phthisis. It is seldom extreme till towards the termination of the disease, and not always then. Patients who fear, and yet are unwilling to believe, that they are consumptive, will fetch a deep breath, and bid you remark how thoroughly they can distend their lungs; and they expect you to say that there can be no disease in those organs. I have been told that the late Dr. Matthew Baillie died of pulmonary phthisis; and that even he was accustomed to delude himself by this test. However, if phthisical persons do not in general suffer much from dyspnœa, their breathing, although they may not be aware of it, or may not choose to acknowledge it, is generally, in some degree or other, short, or hurried. You may wonder that a disorder in which so large a portion of the breathing apparatus is so often effectually spoiled, should be attended by so little distress in respiration; so little dyspnœa: but your surprise will be diminished if you consider the insufficient manner in which consumptive patients are nourished, in consequence of abdominal disease; and the extent to which their blood is wasted by diarrhœa, and by perspiration. The mass of blood is thus kept down to that measure which, passing through the still pervious portions of the lungs, is capable of being arterialised without any great deviation from the ordinary mode and frequency of breathing. The presence, however, of marked pyrexia in the progress of the disease, is always accompanied by frequency of the respiration.

Neither is *pain* in the chest a very important symptom in consumption. In many cases pains, more or less severe, resembling those of rheumatism, are complained of; in the sides, or beneath the clavicles. In others, no pain at all is experienced. When sharp pain occurs, it may be supposed that the pleura is inflamed and beginning to adhere in the painful part.

There is, however, one contingency of which the two symptoms last mentioned are sometimes very significant. When, during the progress of phthisis, violent pain in the side, and extreme dyspnœa and anxiety, set in *suddenly*, and *together*, they denote, with much certainty, perforation of the pleura, and its serious consequences.

The *hectic fever* which accompanies phthisis is of much greater moment. It often creeps upon the patient insidiously. He feels chilly perhaps, towards evening; and in the night his hands and feet are dry and burning; and towards morning he perspires. The most marked symptoms of the hectic are to be found in the variations of bodily temperature, in the perspiration, and in the state of the pulse. The variations of temperature are great and striking. The heat will rise from about 98°, that is from the degree which is nearly natural, in the morning, to 102° or even higher in the afternoon or evening. The perspiration is usually out of all proportion to the previous chilliness and dry heat. It seems to have a close connection with the *sleep* of the patient: it seldom comes on while he continues to lie awake; but after sleeping he wakes, and finds that he is sweating. The perspiration is generally most copious on the upper part of the body, the chest and head. Sometimes it is moderate; sometimes the patient is drenched and drowned in it. There is a good deal of uncertainty about this symptom, and of obscurity as to its cause. Generally speaking, it belongs to the more advanced stages of phthisis; but occasionally it accompanies its early periods. It will cease without any apparent cause: and recur again with the same capriciousness. A poor friend of mine, who died of phthisis, and was particularly harassed by the nocturnal perspirations, took it into his head that *posture* had something to do with them; and slept for several nights in succession *sitting* in an easy chair: and during those nights he certainly did not sweat, though he had been doing so, profusely, before. Louis found that one patient in ten escaped this symptom.

This is a symptom which is often very distressing to the patient, making him even dread to go to sleep; it tends also to the rapid exhaustion of his strength; and betokens, it is believed, when copious or persistent, a short duration of the disease.

Frequency of *pulse* is a symptom so generally present in tubercular phthisis, that too much importance has been ascribed to it as a diagnostic sign. I mean, it has been too much the opinion that the lungs are safe, when the pulse does not rise above its natural standard. Sometimes it remains steady to that standard nearly up to the period of dissolution. Such cases are, I believe, generally slow in their progress. Very recently I lost a friend whose lungs were full of cavities and crude tubercles. He had been a valetudinarian for years; but the pulmonary disorder had been manifested by decided symptoms during a few months only. At no period did his pulse exceed sixty-eight beats in the minute. Commonly, however, the pulse is continually above ninety; and often it is much higher. When there is nothing to account for this increased frequency of pulse, it is a suspicious symptom.

Diarrhœa is a common and an ugly symptom in phthisis. When it occurs early, as it sometimes does; when a patient having habitually costive bowels, becomes habitually relaxed; and you *suspect* only, from other causes, that he may have incipient phthisis; this change often sets its seal upon the nature of his disorder. Usually, however, diarrhœa does not become urgent until the disease is far advanced, and has already declared itself by other and unequivocal symptoms. When it so occurs, it is apt to harass the patient exceedingly; and rapidly to waste his strength and flesh. He appears to melt away under the influence of the purging; which is therefore said to be *colliquative.* It used to be held that the diarrhœa and the perspiration bore an inverse ratio to each other: that when one of them abated, the other always increased. But the more exact observations of Louis and of others have proved that this is not so: that neither in phthisis, nor in other diseases, have these symptoms any such regular reciprocal relation. One reason, perhaps, for this error, may be found in the circumstance, that acids, which have the effect often of checking the perspiration, tend also, in some persons, to produce diarrhœa. Louis found that this symptom began early in the disease, and continued through its whole course, in one out of every eight patients; and in one case only in every twenty-two was it wholly wanting. It depends most commonly, if not always, upon scrofulous ulceration in the small intestines and in the colon. In Louis' experience, there were, invariably, *large* ulcers, whenever the diarrhœa had been chronic and abiding, and the stools had been numerous. In the small intestines the ulceration evidently commences in the mucous follicles; the glandulæ solitariæ, or the glandulæ agminatæ; and sometimes, though not often, the ulcer perforates the bowel. It is probable that, in the large intestine, ulceration begins in the same way, by the deposit of tubercular matter (which is subsequently removed) in the solitary glands: but when once begun, the ulcerating process extends itself indefinitely to the surrounding mucous membrane.

I should have stated before that, with this disease of the intestinal canal, there is often found enlargement of the corresponding glands of the mesentery, which are frequently filled also with tubercular matter.

Several of the symptoms that I have been mentioning—the state of the digestive organs, which interferes with the due assimilation of the food; the hectic fever; the drain implied in the profuse sweats, and in the habitual diarrhœa;—conduce to cause another constant accompaniment of progressive phthisis: and that is *emaciation*. You know that the wasting in this complaint, when it is not cut short by some accidental complication before it has reached what may be called its natural termination, is extreme. It often is one of the earliest, as it is one of the most alarming, of the symptoms which the patient presents: and it frequently becomes excessive before any perspiration or purging have occurred to account for it. If, without any apparent cause, a person grow thin and weak, and his pulse be quick, and his breath at all short—these are intimations which seldom prove unfaithful, that tubercular disease is at work in the lungs, and in the abdomen; and is *consuming* his life.

Œdema of the ankles, and even some puffiness of the hands and face, are circumstances which seldom fail to appear in pulmonary consumption: but they are among the latest of the symptoms. Œdema does not tell us what the disease is, in such cases. We have been satisfied as to that, some time before. But (unless there is some marked disease of the heart or of the kidneys) it tells us that life is about to terminate. It is worth attention as a prognostic symptom merely.

And the same may be said of aphthæ. This is one of the last of the symptoms: but in some cases it does not happen at all. I have lately described this morbid condition of the mucous membrane of the mouth and tongue, and have nothing more to say of it at present. It bears the same relation to phthisis as to other chronic disorders: and marks, for the most part, the approach of their fatal ending.

It is always interesting to couple changes of structure with their appropriate signs. I will therefore take this opportunity of telling you what Louis has observed of this relation, with respect to the larynx and windpipe.

Ulceration of the epiglottis is often latent—gives no appreciable signal of its existence. The symptoms that belong to it are, a raw, or pricking, or burning sensation, at the upper part of the thyreöid cartilage, with occasional dysphagia, and the rejection of liquids through the nose, while the tonsils and pharynx present no visible alteration.

Ulceration of the interior of the larynx is marked, when slight, by trivial pain in that part, and some variation from the natural voice; when deep, by severer pain, and abiding aphonia.

When these observations were made, the laryngoscope, which suffices now for the recognition of such ulceration, was not in use.

Ulceration of the trachea is seldom revealed by any symptom. And this is worth remembering; for patients are continually persuaded by medical men who know no better, that their symptoms are all *tracheal*.

There are still a few other circumstances which, when they occur, accumulate conviction as to the nature of the disorder. *Fistula in ano* is a corroborating complication. The hair falls off. In women the catamenia are suspended. There are certain physical peculiarities too, which are strongly indicative of a tendency to consumption; or perhaps I should say of the scrofulous diathesis. Largeness of the pupil, with a sluggish iris—in other words, a not very sensitive retina—constitutes one of these. A clubbed state of the ends of the fingers, with convex and adunque nails, forms another. Yet this last is not peculiar to tubercular consumption. I have heard of one case in which it was strongly marked: the patient died after a long illness—chronic puriform discharge from the pleura after paracentesis thoracis: but there were no tubercles. And I have recently (1857) been consulted by a gentleman whose right chest I found flattened and much contracted, and marked at its lower part in front by the scar of a puncture through which matter was discharged four or five years ago. He was still coughing up thick and puriform mucus. The ends of his fingers were remarkably bulbous, and their nails very convex. They had naturally (he assured me) been delicate and taper: and he had watched the change in their shape, which commenced with the commencement of his chest symptoms.

In the year 1863 I saw, in consultation with Mr. Gregory

Forbes, a boy whose case we thought to be one of pleuritic abscess on the right side, the left lung being sound. He died in 1867. The right lung was firmly adherent to the walls of the chest and to the diaphragm, and it was converted into a mass which looked exactly like coarse sponge, of a dark gray colour. The vacuities of this sponge were of various sizes, and purulent matter was infiltrated into these and not collected into any separate pleuritic cavity. The other lung was sensibly enlarged, but there was not a trace of tubercle in either lung. The nails both of the fingers and the toes of this boy were clubbed and convex to a great degree. This peculiarity seems to be a sign rather of present disease than of a diathesis.

LECTURE LI.

Phthisis, continued. Diagnosis. Forms and varieties of Phthisis. Ordinary duration. Age at which it is most frequently fatal. Influence of sex; and of occupation. Question of Contagion. Treatment.

In a former lecture, the twelfth of this course, I entered somewhat fully into the *pathology* of scrofulous and tubercular diseases in general. I pointed out the fact, that though such diseases affect vast numbers of persons, and are most extensively fatal, yet that they affect almost exclusively certain *classes* of persons. That while some are so prone to tubercular disease as to fall into it upon the operation of the slightest external causes, or even spontaneously—nay, in spite of every care to the contrary—others again, who are constantly exposed to influences likely to call scrofulous disease into action, either do not suffer therefrom, or, if they do becomes crofulous, it is only when the external circumstances most favourable to the production of such disease have been intense in degree, and protracted in their application.

At the same time I showed you how commonly the *disposition* to scrofula descends in families: and I told you what observation has collected with respect to the *causes* which may *excite* scrofulous disease in persons *hereditarily disposed* to it. I shall not, therefore, go over that ground again. What I then said of scrofulous disease in general is true of tubercular consumption in particular. I will merely remind you that these exciting causes are essentially causes of *debility*. Whatever tends to depress the vital powers, and permanently to weaken the body, tends also, in a predisposed frame, to engender or to call forth this fearful and most destructive malady.

With respect to the detection of tubercular disease in the lungs, it is sometimes very easy, sometimes extremely difficult. It is easy when the tubercles are numerous, large, or far advanced: difficult, sometimes, when they are crude, scanty in number, and thinly scattered, and individually small. In the latter case they may not cause any appreciable deviation from the natural resonance of the chest on percussion, or from the natural smooth, equable rustle of the breathing. It would be tedious to travel again over all the auscultatory and other symptoms, with the view of pointing out their bearing upon the diagnosis. I touched upon that point incidentally, when discussing the individual symptoms, in the last lecture. Many of the symptoms tell their story so plainly that any attempt to expound or interpret them would be quite superfluous. One or two cardinal points, however, which have rather been hinted at before than expressed, I may just advert to.

The fact that tubercles occupy the *upper part* of the lung by preference, is of great moment in relation to the diagnosis. When the symptoms are equivocal; when, so far as *they* are concerned, the case may either be one of mere chronic bronchitis or of tubercular consumption; a careful examination of the superior regions of the chest will often decide the anxious question. The sound resulting from the first gentle tap upon or beneath the clavicle, often rings in the physician's ear the knell of his unfortunate patient. Even unusual distinctness of the sound of *ex*piration, if heard at the summit of the lung, and *à fortiori* if at the summit of one lung only, warrants the terrible suspicion that tubercles are breeding in that lung. It may, indeed, be laid down as a rule, which very few exceptions diversify, that if you find dulness on percussion; or indistinct breathing; or coarse inspiration; or loud and prolonged expiration; or tubular breathing; or undue resonance of the voice; or a click or morbid noise of some sort when the patient respires, or speaks, or coughs; if you find this day after day, and always between the clavicle and the mamma in front, or between the clavicle and the upper edge of the scapula, over the top of the shoulder, and nowhere else; and more especially if these deviations from the healthy sounds be limited to one side, or greater on one side than on the other, or different in quality on the two sides; you may set the case down as a case of phthisis. On the other hand, if in the same parts you still distinguish all the natural sounds of respiration, and can still obtain a clear sound on percussion, you are not to condemn the case, nor to despair of recovery, whatever its other circumstances may be. The worst symptom certainly, when auscultatory signs are wanting, is hæmoptysis.

In cases otherwise doubtful, you will find the region that corresponds to the upper and *back* part of the superior lobe of the lung more pregnant with conclusive evidence than any other region of the chest. Make your patient grasp each of his shoulders with the hand of the opposite arm, and then apply your ear to the flattened back, in the acromial region, in the supra-spinal fossa, and in the space between the spine and the scapula, and you will often hear a bronchial blowing or tubular sound which will painfully confirm your awakened suspicions.

Pulmonary consumption is a disease of many forms and aspects. It will be profitable to discriminate some of these, and to survey them separately, however shortly.

First, then, consumption is sometimes an *acute*; sometimes, and more often, a *chronic* disease.

The acute form is of this kind:—The patient, who may or who may not have seemed previously to be in good health, is suddenly attacked perhaps with copious hæmoptysis; or he catches a severe cold; and almost immediately afterwards intense fever is set up, of a hectic character, the physical signs of pulmonary phthisis, especially of cavities, rapidly develope themselves, and death ensues within a few weeks. The case has been one of what is called *galloping* consumption. After death the lungs are found hollowed by numerous vomicæ. Under Laennec's view, tubercular matter has been widely distributed, and has quickly softened. According to Niemeyer's, there has been catarrhal pneumonia in various lobules of the lungs, the pneumonic products have fast degenerated into cheesy matter, which has as rapidly broken down. Any crude tubercles that might also be seen in the lungs he would regard as secondary, but by no means necessary, results of the inflammation and its products.

To my mind, what seems certain in this form is, that *scrofulous* inflammation, scattered broadly through the pulmonary substance, causes its rapid and extensive disorganisation.

Over cases of this acute nature medicine can have very little effective control.

There is another form of acute consumption, or—as it is usually, and I think more fitly, called—*acute tuberculosis*. It is a striking but not very common disorder, and is sure to arrest the attention of the observer when it does occur. I have met with some half-dozen examples of it.

The following are its main features. The patient becomes suddenly very ill, has frequent rigors, difficulty of breathing, cough, a very rapid pulse, night sweats, and high fever. You listen at his chest, but you do not hear the sounds that are proper to phthisis. You do not find dulness confined to the upper lobes, nor pectoriloquy, nor gurgling respiration; but what you do find are rather the sounds which belong to acute capillary bronchitis, small crepitation diffused all over the chest, and succeeded by absence or deficiency of the natural breath sounds everywhere, without any defined consolidation of the lung. Meanwhile there is none of the expectoration which is characteristic of phthisis. In short you would not suppose that the disorder was phthisis at all. It resembles more the onset of one of the specific fevers. The disease runs a short and distressful course, the countenance and lips of the patient become livid, often he cannot lie down, and within a few days, or at latest in a week or two, he is dead; and after death you find his lungs bestrewed from top to bottom with miliary tubercles—the granulations of Bayle—myriads of them, gray, glistening, and minute. The granules are thickly and uniformly spread over the whole of the air passages, or throughout the entire extent of the lungs, and their sudden presence there in such abundance excites inflammation, which masks and conceals the actual mischief; and the true nature of the case is often not suspected until the body is examined after death. Such, and not the reverse sequence, appears to me to be the *ordo rerum.* The tubercles are the cause of the inflammation, and not the inflammation the cause of the tubercles.

It remains to be stated that sometimes this copious eruption, as it were, of granules is confined to the lungs and the pleuræ; but they are apt to occur also in the abdomen alone, in the membranes of the brain alone, or (what is more common) in all the three great cavities of the body at once, or perhaps in two only of them.

I can offer you no counsel as to the treatment of these sad cases. They baffle our art, and they are always, so far as I know, fatal. All that can be attempted is to assuage the severity of the most distressing symptoms.

It has been fairly made a question whether these granulations are really identical in nature and cause with the nascent pulmonary tubercle of ordinary phthisis: in other words, whether or not they can be regarded as constituting the initial stage of tubercular consumption in man, and only fail to advance into the opaque, yellow state because their multitudinous invasion of the lung destroys life too soon to allow of such advance. It is Dr. Andrew Clark's opinion that the tubercles produced in some of the lower animals by their inoculation with various matters, are *not* true tubercles, do never collect into groups, nor produce consolidations, nor break down into a fluid consistence, nor form cavities. In his experience the animals so treated do never die of the artificial infection. If not purposely killed, they live on, apparently unharmed. It is presumable that some at least of the animals thus spared must have had tubercles engrafted upon their lungs; but they all remain quite well, and Dr. Clark believes that the so-called tubercles are absorbed. We must bear in mind, however, that the guinea-pigs, the subjects of these experiments, had undergone one or two inoculations only, whereas in the human body the internal inoculation may be repeated indefinitely.

Whenever you may meet with an instance of this sudden besprinkling of innumerable granules, whether within the cranium, or within the thorax, or within the abdomen—or in all or some of these great cavities—I would recommend you to search very diligently for any possible residue of inflammatory products, lurking, unsuspected, in some part of the body.

Again, the more chronic cases of consumption arrange themselves under several different forms or varieties which well deserve separate consideration.

Heretofore I have been in the habit of bringing before my hearers certain distinctions with respect to the various forms of phthisis, first drawn by my friend Dr. Latham, and published in the little work on Clinical Medicine to which I have often referred. The facts upon which these distinctions were built had long been known; but they had never before, that I am aware of, been made so instructive, by being clearly disposed, and exhibited in their proper bearings.

Whether Dr. Latham's conceptions of the actual changes wrought in the lungs in the several forms of phthisis described by him are pathologically orthodox or not, the pictures which he has drawn are so clinically faithful, and so graphic, that I shall reproduce them without attempting to trim and square his statements to the pattern of the newer theory of the origin of tubercle. Whether the doctrine of Laennec or that of Niemeyer shall prevail, the outward features of these varieties of phthisis will remain the same.

Dr. Latham first divides phthisis generally into two forms, which he calls *mixed* phthisis and *unmixed* phthisis. And he illustrates what he means by those terms very simply and skilfully. He takes the case of an absorbent gland in the neck, affected with scrofulous disease. The changes which are liable to take place in it are wrought before our eyes: we have the privilege of watching them. Now such a gland will sometimes enlarge, in consequence of the deposition of tubercular matter in its substance: it will grow large and hard, without there being any pain, or heat, or redness observable; and it may remain in that state for weeks, or months, or years.

But in the majority of instances the absorbent gland, after remaining for a certain time in this condition, will undergo, and cause, other changes. Pain, heat, and redness will ensue; the hard gland will soften; the integuments will grow thin, and at length give way; the softened tubercular matter, mingled with pus, will escape; and then the pain and heat and redness—the inflammation, in short—will disappear; and the abscess will heal, leaving behind it nothing more than a slight scar. This process may happen to one such gland; or to more than one *simultaneously*; or to several in *succession.*

In this case there has been no more inflammation than was just enough to accomplish its purpose of removing from the body the tubercular matter. The inflammation has not transgressed what Dr. Latham has called its specific limit.

But again, it may go beyond that limit; it may be both more severe and more extensive than is necessary for the removal of the tubercular matter in the diseased gland. It may pervade the whole neck, giving rise to diffused redness, and swelling, and pain: and the whole of the subcutaneous areolar tissue between the angle of the jaw and the clavicle, may be loaded with effused serum and pus.

All this you may see almost any day in the wards or waiting-rooms of a hospital. And Dr. Latham has happily chosen this affection of the cervical glands to elucidate what happens when the tubercular matter is deposited in the *lungs*, where one cannot *see* the changes it is suffering or producing.

Tubercles in the lungs may remain for an indefinite period of time, in their crude state; never softening at all, or softening only at a very late period. Or just so much of inflammation, and no more, may arise, in the pulmonary tissue surrounding them as is sufficient to bring about their softening and subsequent expulsion. Or, lastly, the tubercles may excite much more inflammation of the lung around them than is requisite for their elimination: inflammation of every degree, and of any extent.

Now to the first two conditions, when they occur, he gives the name of unmixed phthisis: the third he calls, on the other hand, mixed phthisis. We learn from auscultation whether the case be one of mixed or unmixed consumption; *i.e.* we hear, in the unmixed form, the sounds, or the modifications of sound, which result from the presence of tubercles or of vomicæ; and we hear these morbid sounds only: in every part of the lung where they are *not* audible, we hear the vesicular murmur of health. But in the mixed form we also hear these sounds. True, and we hear other morbid sounds besides. The tubercular disease is mixed with common inflammation; and we hear the sounds that denote common inflammation of the mucous membrane, or of the substance of the lung—sibilus, or large or small crepitation—we hear these sounds *mixing* themselves with the sounds which belong to the tubercular affection.

This distinction is of considerable importance, for it concerns the *treatment* of the malady. The tubercular disease, when established, is beyond our power. The inflammation which is incidental to it we may hope to alleviate or to remove. It is in the stage of vomicæ that the disease commonly assumes the mixed character; and sometimes the bronchial or vesicular effusion, upon which the added sounds depend, may be got rid of by the seasonable application of a few leeches, or of cupping-glasses, or of a blister, and the disease be brought back again, for a time at least, within its specific limits, and the patient be relieved from much distress and imminent danger. It is upon this principle that Dr. Latham explains the fact that most consumptive patients improve considerably, soon after their admission into the wards of a hospital. The poor are necessarily much exposed to those causes which tend to complicate the tubercular disease. The tubercular disease may as yet be slight and limited; but the superadded mischief, the bronchial and vesicular effusion, may be immense; and this being submitted, often for the first time, to rest and to treatment, upon their admission into a hospital, is for a while removed.

If we had not the advantage of the method of auscultation, we could not ascertain these differences, nor detect them when they existed. You will perceive, I am sure, their practical importance.

Of course the more ready the surrounding lung is to take on inflammation—in other words, the stronger the disposition in the complaint to assume the mixed character—the more rapidly fatal is it likely to be.

But of the unmixed form of phthisis Dr. Latham has pourtrayed two interesting varieties. In one of these the lungs are apparently tenanted by a multitude of tubercles, which remain crude and unaltered for a considerable length of time. In the other, successive *crops* of tubercles appear to form: or at any rate the tubercles ripen and are expelled in successive crops: and there may be long intervals between each crop and the next.

Dr. Latham thus describes the former of these two varieties: —'An individual loses the complexion of health, and becomes thin; he coughs a little; but perhaps he has no notable fever, and no constant acceleration of pulse.' Upon auscultation of his chest it is found that there is dulness beneath one or both clavicles, or about one or both scapulæ, and an indistinct respiratory murmur in those parts; but the vesicular breathing is free and perfect in every other part of the lung. Here we have tubercles, crude, and in the upper lobe alone: and this state of things may endure for years, without variation; the patient remaining always a great valetudinarian. 'To such a patient (says Dr. Latham) it is a continual puzzle why he does not get well. He consults an infinite number of medical men: and it is remarkable that he gets no comfort or satisfaction from those who understand his disease the best, and the greatest comfort and satisfaction from those who understand nothing about it. Those who know what it is, out of kindness do not tell him the truth, and they cannot asseverate a falsehood stoutly enough to carry any weight with it: whereas they who know nothing about it affirm boldly and unhesitatingly that *it is all stomach;* really believing that the whole and sole disorder is in the stomach, and that it is within the reach of an easy cure.'

But at length—perhaps after a very long period—vomicæ *are* formed; and then the patient sinks rapidly, and his lungs after death are found riddled by cavities and stuffed with tubercles; but every part of them not occupied by tubercles or vomicæ is crepitant and healthy. In these cases, disease lingers long in the crude stage; new tubercles are added, probably, year after year; but none of them soften. They do not excite inflammation in the lung around them. You recognise the presence of the tubercular matter by the ear; but there are no vomicæ. At last vomicæ are formed, many at the same time or in rapid succession, and the patient presently succumbs.

The other variety of unmixed phthisis may be just as protracted as this; but its character and progress differ materially. In the former case the patient's condition was one of invariable ill health; in the one I am about to mention he has fits of ill health, and fits of comparative good health. He spits for a time considerable quantities of puriform matter, and then ceases from expectorating altogether. He has hectic fever, and then throws it off, and then suffers it again: wastes, and recovers his flesh, and again loses it. You will find such cases common enough; and in these cases the morbid sounds will be correspondent to the symptoms. During the fits of illness you will hear gurgling respiration or gurgling cough at the apex of one or of both lungs; and during the fits of good health you will hear cavernous respiration or pectoriloquy in the same parts; but everywhere else you will hear a clear sound of vesicular breathing. Here the tubercular matter excites just enough inflammation round it to achieve its own expulsion, and no more. The lung is destroyed bit by bit. Fresh portions of tubercular matter are deposited; these ripen and soften, and are expectorated, and a vomica is the result; and then there is a period of quiet. And there being still a large portion of each lung to breathe with, the patient regains more health and strength in the intervals of his attacks than the former patient possessed habitually.

But in this form of unmixed pulmonary consumption, a period at length arrives when the patient does not revert to the former state of apparent health. The quantity of lung that has now been destroyed forbids it. You hear the sounds proper to tubercular disease over a large space, between the clavicle and the mamma, or anywhere about the scapula, on one or both sides; yet still that part of the lung which is free from tubercles and vomicæ is pervious and healthy: but the hectic continues, the emaciation increases, and the strength declines; and the fatal consummation arrives.

Of these two varieties of genuine and unmixed consumption, the first is the most hopeless. The tubercles are numerous; they probably go on increasing in number though they do not soften; there is not, and there cannot be, any even temporary return to health, either real or apparent.

Whereas where the tubercles come singly, or in successive crops, and rapidly soften, and are expectorated; and where some long time interposes between the crops; the health and strength return, and there is just a chance that no more tubercles may form. It is in this variety of unmixed phthisis that a natural cure, by the contraction and cicatrisation of a vomica, may by possibility take place. We cannot expect, we scarcely dare permit ourselves to encourage hope, that the disease *will* cease in that manner: but if it cease in any form of the malady, it is in this.

This grouping of the different characters under which pulmonary consumption may appear, has been performed by Dr. Latham with perfect fidelity. There is nothing overstrained or fanciful about his sketch; it is after nature,. and it is by the hand of a master. And there is something very refreshing in original views of this kind. Vastly more instructive, too, they are than those presented by a dull compilation. I therefore again recommend you to study his little volume. I am sure that *I* have derived much useful and usable knowledge from it: and so also may you.

I have yet to describe one other pulmonary disease to which we neither need nor can deny the name of phthisis, but which differs essentially from those forms of phthisis that we have hitherto been contemplating. For reasons that will presently appear it has been called *fibroid phthisis.* The physical signs to be collected by percussion and auscultation of the chest, and by its inspection, are in many instances insufficient to discriminate this from other varieties of phthisis; but there are other signs and circumstances which enable us to make the distinction during the lifetime of the patient, and we see specific differences plainly marked in the lungs themselves when we have the opportunity of examining them after his death. You will find a very interesting paper by Dr. Sutton on fibroid degeneration of the lungs in the 48th volume of the 'Medico-Chirurgical Transactions:' and cases of fibroid phthisis, with comments, by Dr. Andrew Clark, in the first volume of the 'Transactions of the Clinical Society.'

The anatomical characters of this form of pulmonary consumption are as follows. The affected lung is shrunk within its natural dimensions, and a great part of it is solid, dry, heavy, and tough, and grates sometimes when cut through with a knife. The cut surface is slate-coloured, or like gray granite, or of an iron-gray tint. Through these solid portions run fibrous septa in various directions. The connecting tissue that bounds and separates the pulmonary lobules is hypertrophied and augmented, and therefore unnaturally visible. Cavities are sometimes found in the apex of the lung, or scattered through the indurated parts. These appear to consist, for the most part, of dilatations of the bronchial tubes. Occasionally, however, the ragged sides of a cavity, and the offensive odour of its scanty contents, suggest the notion of its having originated in circumscribed gangrene. Amid the gray fibroid degeneration may sometimes be seen a few cheesy-looking spots of soft consistence. The pleuræ are often adherent, or, if not adherent, are considerably thickened.

Both lungs are liable to this morbid condition, but it is often limited to one lung only; and so far as observation has yet gone, the left lung is most frequently the one affected; and the disease begins almost always in the upper lobes.

It is plain that the auscultatory sounds in this form of disease must be very much the same in kind with those that occur in cases of ordinary phthisis: contraction, flattening, and diminished mobility of the chest walls; dulness on percussion; tubular breathing; increase of vocal vibration; and even large crepitation and gurgling. We cannot therefore discriminate this form of phthisis by auscultation alone. But if we look to outward tokens and peculiarities, the distinction becomes more manifest and easy. In the first place there is the grand difference that the disease is exempt from bondage to the strumous diathesis. It is met with most commonly in adults at or beyond the middle period of life, of large and vigorous frame, having strong and sound teeth—whereas the teeth of scrofulous persons decay fast and early. It is attended neither with hectic fever, nor continued elevation of the bodily temperature, nor frequency of pulse, nor, till the end approaches, with any marked dyspnœa. The skin is pale, muddy-looking, and dry. Often there is some œdema of the ankles, and the urine is apt to be albuminous. Another marked difference is the slow progress of the disease, which may go on even for many years; and lastly it is unaccompanied by those ulcerations of the mucous membrane in various organs which are so common in ordinary phthisis. The disease is believed to originate in some kind of pulmonary inflammation, and to find most of its victims in habitual spirit-drinkers. It may, however, follow an attack of pleuro-pneumonia in persons of temperate habits. Of this I have in my mind one remarkable instance.

Several pathologists, in Germany and in this country, state that fibroid changes analogous to those found in the lungs, may be traced in many other parts of the body—in the kidneys, liver, spleen, heart, skin, and bronchial glands. But there are reasons, which I shall give you hereafter, for mistrusting the doctrine, derived from these alleged changes in various organs, that the disease hangs upon a constitutional disposition.

I should add that in the lungs of patients dead of this fibroid disease there may sometimes be found a few tubercles, but this is an accidental complication. Often not a single tubercle can be detected. The fibroid change would even seem antagonistic to the formation or advance of tubercles; and Niemeyer's dictum, that the greatest danger for the majority of consumptive persons is that they may *become* tuberculous, is in some degree applicable here.

Upon the whole I think, with Dr. Andrew Clark, that it will be well, for the future, to define phthisis pulmonalis, generically, as 'comprehending all progressive consolidations and circumscribed suppurative degenerations of the lung.'

From what I have been stating you will perceive how difficult it is to say what is the ordinary *duration* of phthisis; concerning which a question was put to me at the close of the last lecture. The disease may be present for some time without declaring itself by any marked or unequivocal symptoms, and therefore without attracting attention. But taking the cases as they occur, and estimating the duration of the malady from the time when it first manifests itself in a decided form, we find there is quite enough of variation to warrant the distinction that has been made between chronic and acute phthisis; or, to use the more popular and more expressive phraseology, between slow and galloping consumption. The following tabular statement of the results observed by Bayle and Louis, will give you a somewhat more precise notion of the general progress and duration of the disease. The whole number of cases noted was 314. Of these, 24 died within three months; 69 between three and six months; 69 also between six and nine months; 32 between nine and twelve months; 43 between twelve and eighteen months; 30 within from eighteen months to two years; 12 between two and three years; 11 between three and four years; 5 between four and five years; 1 between five and six years; 3 between six and seven years; 1 between seven and eight years; 3 between eight and ten years; and 11 between ten and forty years.

You will remark that, so far as this account goes, more than one-half of the whole number died within nine months from the time when the disease first became manifest. This agrees with the experience of the late Dr. Gregory of Edinburgh. He used to state that the ordinary duration of phthisis was about six months; that sometimes it lasted only two or three months; and that he had seen one case which proved fatal on the seventeenth day after the symptoms were first observed. On the other hand, he had known one man who was at least 72 years old when he died, in whom symptoms of phthisis first appeared at the age of 18, and who was never free from them during all the intervening period; being often hectic, and frequently spitting blood. It has been my melancholy task to watch the long decline, and the death at last, of a statesman who served his country well and strenuously, yet of whose years and health a precisely similar description to this would be true. The *average* or *mean* duration of consumption has been computed to be about two years. This is a very different thing, you will please to observe, from its *ordinary* duration.

There are many other points in the statistical history of phthisis well worthy of attention and inquiry; but I have not time to go into them in any other than a summary manner. This part of the subject is very well worked up in Sir James Clark's lucid and sensible book upon Consumption; but you will have perceived, from the references I have so frequently had occasion to make to M. Louis, that *his* work is the great storehouse or treasury of tabular information, with respect to the facts of tubercular phthisis.

It is an interesting question to determine at what period of human life consumption numbers the most victims. There are two short tables—one drawn up by Louis, containing observations relative to 123 cases, and the other by Bayle, respecting 100—which throw some light on the question. The two tables agree, in the main, very closely. Thus, from the age of 15 to that of 20, Louis met with 11 deaths from phthisis, Bayle with 10; from 20 to 30, Louis met with 39, and Bayle 23; from 30 to 40, Louis 33, Bayle 23; from 40 to 50, Louis 23, Bayle 21; from 50 to 60, Louis 12, Bayle 15; from 60 to 70, Louis 5, Bayle 8. You see from this account how erroneous the common notion is, that consumption does not occur at an advanced period of life: that a person who has reached his thirtieth or fortieth year is thenceforth safe from that disease. From these two tables, and others collected by Sir James Clark, it appears that, taking decennial periods, the greatest number of deaths from phthisis happen between the ages of 20 and 30: the next greatest number between 30 and 40: the next between 40 and 50: and that, after these, it is a doubtful matter whether more perish of consumption between 50 and 60, or between 15 and 20, which last is only one-half of a decennial period. These calculations refer, as you will remark, to human life after the age of puberty. Before that age, tubercular disease is fearfully common, especially in infancy and childhood. Among 920 children (532 girls and 388 boys) who died from the age of 2 to that of 15 years, no less than 538 (nearly three-fifths of the whole) were affected, Dr. Papavoine tells us, with tubercles.

In a report made by the physicians to the Hospital for Consumption at Brompton, the decennial periods are calculated from a different point. Their tables, which comprehend the cases of 2,679 males and 1,679 females, lead to the conclusion that, in both sexes, the '*liability to consumption* is greatest' between 25 and 35 years of age.

From the same tables, as well as from those of the Registrar-General, it appears that *in this metropolis* more men than women die of consumption. In the provinces, according to the Registrar-General's returns, there are more deaths from phthisis among women than among men.

Statistical researches are of still greater interest, perhaps, when they elucidate the influence of different trades and occupations in calling phthisis into existence. Sir James Clark has brought together much curious information on this point. There are certain occupations which appear to provoke pulmonary consumption by the direct application of local irritants to the lungs themselves; and there are others which tend indirectly to bring on phthisis, by lowering the tone of the general health—by producing debility and cachexia. But these two causes often go together: and it is difficult to estimate with accuracy their separate effect. The workmen whose employments have a directly irritating operation upon the respiratory organs, are stone-masons, miners, coal-heavers, flax-dressers, brass and steel-polishers, metal-grinders, needle-pointers; and many others who of necessity inhale during their labour an atmosphere loaded with irritating particles of matter. But, then, most of these men work also in towns, and remain for many hours day after day in a constrained position, in crowded or in close apartments. Moreover, some of these occupations, being sedentary, and requiring no great muscular power, are unfortunately selected, for that reason, by persons who are naturally of feeble or delicate constitution. On the other hand, butchers, fishermen, and their families, and farm-servants, are said to be comparatively free from phthisis.

Beddoes ascribed this exemption to the use of animal food by these classes: but much of their better health is due, no doubt, to their habits of active exercise in the open air, and to the circumstance that such employments demand a certain amount of bodily strength and energy, and therefore are not likely to be adopted by weak and scrofulous individuals. It is obvious that the whole inquiry is beset with sources of fallacy. We know, however, on the evidence of undoubted facts, that certain occupations do tend to induce pectoral complaints, and to shorten life. Dr. Knight, of Sheffield, informs us that fork-grinders, who are what are called dry grinders, die there of the *grinder's asthma* or *grinder's rot*, before they are thirty-two years old. Razor-grinders, who grind wet and dry, live a little longer: the moisture diminishes, of course, the number of floating particles of metal. Table-knife grinders work on wet stones, and survive till they are between forty and fifty. I must refer you to Sir James Clark's book for similar facts with respect to the inhalation of silex, of the dust of mines, and so forth. Without pretending to assign to each alleged injurious influence its precise contribution of mischievous effect, we must be content, at present, with the practical inference, that such employments should, if possible, be avoided by all those who show any tendency to scrofulous disease.

Is phthisis *contagious*? No: I verily believe it is not. A diathesis is not communicable from person to person. Neither can the disease be easily (if at all) generated in a sound constitution. Nor is it ever imparted, in my opinion, even by one scrofulous individual to another. Yet in Italy a consumptive patient could not be more dreaded and shunned if he had the plague. And in this country the suspicion will now and then arise that the disease may be infectious. A girl dying of phthisis is nursed by her sister, who afterwards droops and dies of the same complaint. Here the presence of the peculiar diathesis is strongly presumable. But the parties may be different in blood. A wife watches the death-bed of her consumptive husband, and presently sinks herself under consumption: and there may be no traceable or acknowledged example of scrofula in her pedigree. Yet even here the latent diathesis may fairly be presumed to have existed. Very few families are perfectly pure from the strumous intermixture. The predisposition may be slight; it may be dormant for a generation; or, like other inherited peculiarities, it may alight capriciously on some individuals only of the kindred. In both the supposed cases there have been other influences at work, more authentic than the alleged contagious property, in calling forth the fatal malady: watching, the want of rest, confinement in the unwholesome air of a sick chamber, and, above all, protracted mental anxiety, than which no single cause perhaps has more power to foster and forward the inbred tendency to phthisis. The disorder, I am satisfied, does not spread by contagion. Nevertheless, if consulted on the subject, I should, for obvious reasons, dissuade the occupation of the same bed, or even of the same sleeping apartment, by two persons, one of whom was known to labour under pulmonary consumption.

The *treatment* to be adopted, and the plan of regimen to be observed, in respect of pulmonary phthisis, resolve themselves into the methods of *prevention* when the disease is *likely to occur*; of *arresting its progress* when that disease is *incipient or limited in extent*; and of *alleviating* the *most distressing symptoms*, when no hope remains of stopping its course, or of averting its fatal close.

With regard to the *prevention* of the disease, in those who, by inheritance, or by circumstances, are predisposed to it, a great deal might be said; but the subject belongs rather to the head of medical police, or hygiene, than to the practice of physic. We deem that a person *has* that predisposition, which is almost a necessary condition of the development of tubercular disease, when we observe those marks of the scrofulous diathesis which I pointed out in an earlier part of the course: or when we know that the parents possess that peculiarity of constitution: or when brothers or sisters have displayed it. It would be well indeed for society if the multiplication, and diffusion, of the strumous diathesis could be checked, by a prudent avoidance of ill-assorted marriages. But we cannot say—no legislature could say—to a scrofulous man or woman, you shall not marry, and propagate scrofula. It is reasonable, however, to conclude, and the conclusion is amply borne out by the observation of facts, that where both parents are strumous, the child will, in all probability, be doubly so; or that, at any rate, its chance of escaping the scrofulous disposition will be small. It is very desirable, therefore, that correct notions on these subjects should be generally prevalent: and that persons who are conscious that scrofula in any of its shapes exists in their family, and *à fortiori* they who know that it exists in their own corporal frame, should avoid allying themselves with persons who are in the same predicament: and this prudence might be enforced if they could be made to foresee the suffering and misery its neglect is calculated to inflict upon their offspring. Intermarriages of persons of the same family, when that family is subject to tubercular disease, are earnestly to be deprecated. But on these points our advice is seldom asked.*

We *are* liable, however, to be consulted respecting the mode of *warding off* scrofulous disease in those who have derived a hereditary tendency to it from their ancestors. Now the first and most effectual prophylactic in such cases, is a residence in a mild, and dry, and equable climate: and next to that is the avoidance of all causes likely to foster the morbid tendency. I need not repeat what I formerly told you on this subject. Pure air; nourishing, but unstimulating, food; moderate exercise; early hours; cleanliness; warm clothing; and abstinence from excessive study, from severe bodily toil, from occupations in their nature unwholesome, from such callings also and employments as are fertile of care and anxiety, and from vicious and exhausting indulgences of all kinds: these are the topics upon which we must insist, when our advice is sought for respecting the means of preventing consumption in children, or others, who are in danger of contracting it.

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* For these remarks I have been sharply rated by Dr. Rhiven Roberts, in the 'Edinburgh Medical Journal' for 1867.

'Advising the healthy (he says) to marry the healthy, and avoid the diseased, and allowing the latter to intermarry, is the proper way to eradicate the evil from society.' . . . 'In recommending the scrofulous to marry the healthy, we are doing what is at once a gross wrong to the latter (and more valuable (?) of the two lives), as well as what is most prejudicial to the interests of the species; while the benefit to the former is absolutely nothing, and the benefit to the offspring is of a very questionable nature, since it consists in the prolonging of a miserable and painful existence.'

These views, grounded on Mr. Darwin's doctrine of the 'struggle for existence,' may befit a philosopher whose sole end is the worship of humanity; but the duty of a physician is to give his actual patient such advice as may seem best for *him*—just as it is the duty of a barrister to get a criminal acquitted who might with much greater advantage to the community be hanged. Moreover, it has never yet been proved that the highest attainable degree of bodily perfection necessarily implies the greatest intellectual and moral perfection, which after all constitutes the essence of humanity. A strict construction of my words does not advise the scrofulous to choose the healthy, but to shun the unhealthy mate. Yet I believe that such advice—if advice ever had any influence upon the choice—would not be bad advice; for upon the principle of selection, acted upon by cattle-breeders, that course would tend to make the offspring healthier than the unhealthy parent, and repetitions of the same process for a period of time unlimited and illimitable might expunge the morbid tendency from the whole human race. Pushed to their extreme logical conclusions, Dr. Roberts's views would issue in very startling results.

But when the disease *is present*—when tubercles actually exist, and are ascertained to exist in the lungs—may the progress of the disorder be ever suspended by a change of climate? Indeed it may. Our English climate, during the winter and spring seasons, is a dangerous climate for consumptive persons. Catarrhs and bronchial inflammations, and chest affections generally, are notoriously rife amongst us in that portion of the year; and disorders of this kind have a perilous tendency to hasten the progress of existing tubercles, and even, on Niemeyer's theory, to originate pulmonary tubercles. Louis was unquestionably wrong when he affirmed that neither bronchitis, nor pneumonia, nor pleurisy have any effect in exciting tubercular phthisis. This opinion is in direct opposition to the general belief of most men of experience. Many a case of consumption can be traced back to a severe catarrh, and no further. Many that ran a short course were dated, within my own knowledge, from the last visitation of influenza. If M. Louis had meant that thoracic inflammation will not produce tubercles in the lungs of a person who has not the strumous diathesis, I should quite agree with him; but he draws his conclusions from cases of phthisis. I cannot doubt that the dormant predisposition is often awakened into actual disease, and that latent and crude tubercles are often accelerated in their progress, by inflammation of the pulmonary tissues. Whether this happens directly from the local inflammation, or indirectly from its effect in lowering the vital powers, is a question which no one can solve, and of which the solution is not of much consequence. What we are sure of is, that everyone who bears a real or suspected taint of scrofula in his frame, should scrupulously guard against every known and avoidable cause of catarrh, pneumonia, or pleurisy. Migration to an exotic climate is to be recommended, not so much for the sake of any positive virtue or special sanative influence which the climate may be supposed to possess, as for the protection it affords against the dangerous influences of our own winter climate; its chilling dampness, its fogs and gloom, its lack of cheering and salutary sunshine, its inhibition of enough exercise in the open air. In all cases the earlier this escape from danger is made the better; but there comes a time, or a condition, when it is no longer available, and when it ought to be discouraged, or forbidden. The turning point it is sometimes difficult to hit. When phthisis is incipient, whenever it presents itself in either of its slower and unmixed forms, in that form in which tubercles long remain in the crude state, or in that other form wherein a vomica or vomicæ having occurred, the strength is apparently restored, there is no wasting fever, and the remaining portions of lung give out the sounds of health—life may often be preserved, or lengthened, by leaving this country and residing under a less unfavourable sky. But in an advanced stage of the disease, if the lungs are rapidly undergoing disorganization, and there is a corresponding amount of general suffering, no benefit, but on the contrary much inconvenience and useless expense and inevitable disappointment will be incurred by change of place, unless the natural home of the patient be clearly unhealthy. When I am asked about removal, either to another country, or to some distant part of our own, and the state of the patient is such as I have just alluded to, I always advise that he should *not* forego the comforts of his home—and leave his family and friends—to seek advantages which he will not find, among strangers, and amid the discomforts of a lodging perhaps, or an incommodious dwelling. I think it wrong, and cruel, to send people away merely to die: and that many are so sent to this place and to that, in the almost certain prospect of their never returning, no one, I think, can doubt.

It was long thought, and it may still perhaps be the popular belief, that phthisis was more common and more fatal in cold regions than in warm, and that warmth of climate was therefore the main thing to be desired for our consumptive patients. But this was a fatal mistake. Speaking generally, a very warm atmosphere is hurtful to such patients. They melt away under the heat. A conclusive fragment of statistical information on this point has often been quoted from Captain Tulloch's valuable reports upon the health of our troops in the West Indies. 'Out of an aggregate strength of 86,661 soldiers serving in the Windward and Leeward Command, not fewer than 1,023 were attacked by that fatal disease (phthisis), being 12 per thousand, annually; while out of an aggregate strength of 44,611 Dragoon Guards and Dragoons serving in Great Britain, only 286 were attacked, being about 5½ per thousand.' The black troops in the West Indies suffer even more than Europeans from consumption.

In selecting a place of residence for our exiles, the special temperament of the individual patient must be taken into account: for *cæteris paribus* a place that may be very suitable for one consumptive person may be very unsuitable for another.

Ascertain what kind of atmosphere or of weather best agreed with your patient while he was yet in good health. Much the larger portion, I fancy, of our English race feel better, more brisk and vigorous, in dry cold weather than under moist heat; the former they speak of metaphorically as *bracing*, the latter as *relaxing*. A minority feel chilled and pinched by cold, and rejoice in that humid and to them genial and soothing warmth which enervates, weakens, and depresses others, and tends to derange their digestive organs. These peculiarities must not, I say, be overlooked. Generally speaking, the great desideratum is air that is both dry and moderately warm. The sting of our atmosphere lies really in its constant moisture. That is the mischievous factor in the English climate. There is in Great Britain no air, that I am acquainted with, of which the combination of warmth and dryness can be predicated.

If you happen to be consulted about a fit residence, temporary or fixed, in this country, for a phthisical patient, bear in mind the important results of Dr. Buchanan's researches, which were briefly referred to in a former lecture. He found that in the counties of Surrey, Kent, and Sussex (and the same may be presumed of the whole kingdom), there is, broadly speaking, less phthisis among populations living on pervious than among populations living on impervious soils—on high-lying than on low-lying pervious soils—on sloping than on flat impervious soils. So that wetness of soil may be reckoned among the predisposing causes of phthisis to the population living on it. Dr. Bowditch, of Boston, in the United States, had come independently to the same conclusion: and there is both here and abroad a growing opinion that a place is exempt from phthisis in proportion to its elevation above the sea-level; and that alpine regions furnish in some respects the most eligible quarters for our consumptive pilgrims. I have heard, on what seemed good authority, that among the native inhabitants of the high table land of Persia, phthisis is almost unknown.

In all foreign resorts for phthisical invalids, the following ingredients are most especially to be desired: a suitable climate, comfortable house accommodation, proper and sufficient food, good sanitary arrangements, easiness of access and of return, and competent medical advice.

I cannot pretend to go minutely into this subject. For those who need, or benefit by, a stimulating, bracing climate, there are various places of repute in the South of France, and along the northern shores of the Mediterranean. Of these, Mentone presents, I think, the most unexceptional type. You may read in Dr. Henry Bennet's 'Winter in the South of Europe' very interesting particulars about many of these places. For those, on the other hand, who require a warmer, softer, and moister climate, that of Madeira is the most typical example. It has the recommendation, besides, of possessing a remarkably *equable* temperature: whereas in Mentone and in similar spots there are sudden and sharp contrasts between the temperature of the sunshine and of the mountain shadow—of the day and of the night; and against the dangers attending these variations a constant guard requires to be kept. Indeed, in all these places of temporary exile, the invalid needs to be strongly cautioned against the great but common mistake of forgetting that he *is* an invalid. He should resolutely forego all hazardous gaieties, evening parties, archæological excursions, and the like, and devote his whole attention to the object for which alone he is an exile at all, the arrest of his disease, the preservation and prolongation of his life.

Egypt is a country which possesses a dry and invigorating climate, alloyed, however, with the inseparable danger which arises from vicissitudes of day and night temperatures, and from the temptations of sport and of sight-seeing. Much the same may be said of South Africa. Intermediate between such places as Mentone, Nice, or San Remo on the one hand, and Madeira, Teneriffe, or Palermo on the other, may be mentioned Pau, in the Pyrenees.

There are certain persons who love, and find health upon, the sea. For these a well-selected series of voyages may properly be recommended.

In this country Torquay, Penzance, Hastings, Ventnor, Bournemouth—each of these has its peculiar advantages, which must be weighed and estimated according to the different conditions and wants of individual patients. One of the many recommendations of Torquay is that it offers a large choice in respect both of elevation and of aspect.

It is consonant with both reason and experience that the *diet* of persons labouring under this *wasting* disorder should be as sustaining and nutritious as they can take or bear. Many of them have what seems an unfortunate dislike of fats. With very few exceptions, that kind of nourishing food which the patient has found to agree with him best, *is* for him the best. Obviously no list of particular substances proper for each meal can be devised which will meet the requirements of all consumptive persons alike. Comprehending the general principles on which the diet should be regulated, you will seldom find much difficulty in applying them in detail to individual cases. Among food, milk, when easily digested, furnishes most eligible nutriment. A breakfast cup of warm milk, in which has been mixed a teaspoonful of brandy or of old rum, taken in the morning before the patient leaves his bed, is often found to be very comforting and beneficial. There can seldom be any good reason for denying to a phthisical patient a moderate quantity of wine, or (if that agrees better with his taste, or habits, or feelings) of malt liquor, or of alcohol in some one or other of its many shapes; and in cases where the patient is feeble, and chilly, with a tendency to blueness of the surface, some kind and amount of alcoholic stimulant is positively indicated. I do not, however, commend the practice of stuffing a consumptive patient all day long, beyond his appetite and will, with strong meats, and drinks, and drugs accounted tonic. Whatever in the way of diet and of habits tends to conserve or to restore the general strength, is to be enjoined; whatever is likely to impair it, is to be denounced and avoided.

Again as to *exercise.* The patient should be encouraged to take in the open air, and at suitable times, as much bodily exercise as is compatible with the avoidance of great or permanent feelings of fatigue.

Riding on horseback has been strongly counselled in the earlier periods of the disease. Its main advantages seem to depend upon its allowing the enjoyment of fresh air, and of exercise, without putting the patient out of breath; and these advantages are great. Many phthisical patients remain, it is said, free from cough, and those affected with hæmoptysis cease to spit blood, so long as they continue to take exercise on horseback. Gestation in a carriage, or in a boat, has the same good effects, but in a less degree. We are not able, however, to look upon equitation as so certain a cure in consumption as Sydenham did; who says that riding on horseback is as much a specific for phthisis, as the Peruvian bark is for an ague.

Of *drugs*, the oil obtained from the liver of the cod-fish bears away the palm. It is not directed against any particular symptom: but appears, in a much greater degree than any other drug that I know of, to be antagonistic of the consuming power of the disease. Unlike many of the oils, it does not generally purge. One of its most obvious and frequent effects is that of hindering the waste of the fatty tissues of the body; and even of promoting, by the abundant supply of that nutriment, a new deposit of adipous matter. The patient recovers flesh and weight, resumes a healthier aspect, and acknowledges sensations of returning strength and comfort. Meanwhile his cough is mitigated, he expectorates less, his pulse is reduced in frequency, hectic symptoms disappear, and the auscultatory signs declare a corresponding change for the better in the diseased lung. As bearing upon these ascertained facts, I would direct your attention to some interesting observations by Dr. John Hughes Bennett (to whom we are indebted for the introduction of the oil into this country as a remedy for phthisis), upon *the structural relation of oil and albumen in the animal economy.* They are contained in a paper read before the Royal Society of Edinburgh; and published in the 'Monthly Journal of Medical Science' for September, 1847. Dr. Bennett shows that the proper nutrition and healthy organisation of the body depend upon the maintenance of a certain relation between the oily and the albuminous principles which enter into its composition: and that in numerous forms of disease, the excess or defect of one or the other of these principles may be distinctly traced. The whole range of morbid changes denominated tubercular belongs to the class in which there is an excess of the albuminous, and a deficiency of the oleaginous principle. In these speculations do we not obtain some glimpse of the way in which the cod's-liver oil (and other oils, perhaps) may tend to correct, or to keep in check, the strumous disposition? Whatever may be its *modus operandi*, I have often been surprised as well as gratified by the improvement that has followed the use of this remedy, in consumptive patients. The earlier it is resorted to, the better; but in every stage of the disease its healing power has been fully ascertained. In many advanced cases it does no good at all: in some its restorative effect is really wonderful. To diminish the risk of disgusting the stomach, and so indisposing the patient to persist with this remedy, you should prescribe it, at first, in small doses; a teaspoonful, twice or thrice daily: and if no discomfort follows, the quantity may gradually grow into a dessert-spoonful, and finally into a table-spoonful. Beyond that amount, beyond an ounce and half daily, it is scarcely ever useful or advisable to press the oil. Lest it should blunt the natural appetite, I am in the habit of directing that it should be taken at bed-time, and soon after each of the two principal meals of the day; in other words, as long as possible before the meal next ensuing. Of the various kinds of oil in use, that which is the purest, that which is obtained by the simplest methods from the fresh liver of the healthy cod-fish, is not only the least nauseous, but in my opinion the most remediate also. It is not so nauseous, patients tell me, as might be supposed.

Upon children, or others, affected with scrofulous swellings of the cervical glands, the beneficial influence of the cod's-liver oil is perhaps still more certain and conspicuous.

For intercurrent pleurisy, or pneumonia, it may sometimes be necessary to take away small quantities of blood by means of leeches or of the cupping glass: but such necessity is at all times to be deplored.

Counter-irritation is often of essential service for encountering local symptoms: mustard-poultices to the chest when it is painful; or a succession of small blisters, or friction with a liniment containing croton-oil, or best of all, perhaps, painting the part from time to time with the *tinctura iodi*. The effect of counter-irritation upon the progress of the tubercular disorder is apparent sometimes by accident. Dr. Abercrombie has related an example in which cerebral disease operated in this way; the previous symptoms of phthisis disappearing. In some cases mania seems to have a similar consequence, obscuring the manifestation, and probably retarding the course, of consumption. It has been often remarked—you will find this stated by Sir B. Brodie—that after amputation of a scrofulous leg, phthisical symptoms, very little noticed before, have rapidly increased. And there is another fact, in relation to phthisis, analogous to these, which it is fit you should know and attend to, viz. that the progress of consumption is often suspended by pregnancy:—and while a mother is suckling her child, if the suckling be not too long continued, so as to exhaust the mother. I suppose there is no doubt that women disposed to phthisis have been kept alive by successive pregnancies and sucklings. It is a very rare thing for a pregnant woman to die of phthisis. I have known only one instance of it. One of my patients in the hospital, a French woman, died of that disease; and we found suppurating tubercles in her lungs; and a fœtus of about five months in her womb.

The inhalation of steam has often been recommended in phthisis. When the larynx is implicated and irritable it may soothe for a time, but in general the process is more tiresome than useful. Of *medicated* vapours I can say nothing in praise beyond this, that the addition of a teaspoonful of laudanum to the hot water has occasionally been found of singular efficacy in stilling a troublesome cough.

Often—too often—all that we can attempt is to relieve the most urgent or distressing symptoms: and to make easier the patient's decline. One symptom which is both distressing and weakening is the nocturnal perspiration. The common remedy for this is the dilute sulphuric acid; and a very good remedy it is, but it is not equally adapted to all cases. If the bowels are costive—or if the bowels have not, as they often have, a tendency to be relaxed—then the sulphuric acid may be freely given; and it will often have very good results. It may be given three or four times a day, in doses of from twelve to twenty minims. But when this fails, or when the bowels are irritable and will not bear it, we must have recourse to other means. One of these is sponging the surface of the body, at bed-time, or before the patient settles himself for the night, with tepid vinegar and water: using twice as much water as vinegar. And if the bowels are at the same time purged, I find the compound kino powder of the Pharmacopœia an admirable medicine. It certainly has much power over the perspiration; and it has these further advantages, that (containing opium) it tends to control the diarrhœa, and to calm the cough.

Iron is another substance which exercises a marked influence sometimes over the hectic fever. It was its efficacy in this way that gave celebrity to the famous antihectic mixture of Dr. Griffith, the *Mistura Ferri Composita* of the Pharmacopœia. Certain it is, that when iron is borne in the advanced stage of consumption, it often does a world of temporary good:—but in many cases it is *not* borne well. It increases the cough, occasions headache, and heat of skin, and distresses instead of relieving the patient. Nor is it always easy to say beforehand, whether it is *likely* to suit the case or not. I apprehend it will at length be found most applicable to the unmixed forms—the uninflammatory forms, that is—of phthisis. I have frequently, however, succeeded in checking the wasting sweats by the tincture of the perchloride, given in doses of twenty minims thrice a day, after other expedients had failed me. Steel wine, the ammonio-citrate of iron, the syrup of its iodide, are all good and eligible forms.

When the cough is very troublesome, and especially when it breaks the patient's rest at night, we must endeavour to quiet it; and there is no drug, I fear, that we can *depend* upon for that purpose, but opium. Whether chloral may be an adequate substitute is yet uncertain. The old paregoric has been, and is, a favourite form for giving opium to appease cough; and old-fashioned apothecaries will tell you that the alteration which was made by leaving the aniseed out of this compound tincture of camphor in one of the editions of the 'London Pharmacopœia' impaired its efficacy; and the late Dr. Prout was of opinion that *aniseed* has considerable power in allaying the irritation on which the cough depends. He infuses three drachms, or half an ounce, of the bruised seeds in half a pint of distilled water at a temperature not exceeding 120°; and lets it stand till it is cold. On his strong recommendation I have tried this, as a vehicle for paregoric, when the same dose in other vehicles had failed; and I must say, that it has frequently been followed by a marked abatement of the frequency and violence of the cough. The aniseed is retained in the paregoric of the 'British Pharmacopœia.' Hydrocyanic acid has sometimes a very soothing effect upon this harassing symptom. However, at last, opium will be found our sheet-anchor, not merely for the cough, but for the diarrhœa which is so seldom absent in the latter periods of phthisis. The diarrhœa depends, as I have told you, upon an ulcerated state of the bowels. In those cases in which it could scarcely be kept in check at all, I have always found very extensive ulceration in the large intestines: but the diseased condition is often seated higher up, in the ileum or jejunum. The Physicians to the Brompton Hospital commend bismuth as a remedy for this symptom: the tinctures of catechu and of rhatany are also of much service, combined with laudanum and with the officinal chalk mixture:—or a few grains of the *confectio opii* may be given in peppermint water, after every loose evacuation:—or in obstinate cases, a pill composed of a quarter of a grain of the sulphate of copper, and the same quantity of opium, will often answer well, though it sometimes gripes. I mention these several expedients, for you will often require them all. The injection of a small quantity of starch, as much as the rectum will receive and retain, with ten or twenty drops of laudanum, generally affords the patient most sensible comfort; and suspends the further action of the bowels for a considerable time.

These, I think, are the principal means by which we may endeavour to smooth the pillow of the patient dying of consumption. Sometimes very little pain or distress is felt at all, from first to last; the intellect remains free, and the patients are proverbially sanguine about the issue of their disorder. In other cases, do what we will, the patient suffers greatly. One harassing incidental combination of symptoms is nausea and vomiting. I should have stated before, that when, in phthisis, these symptoms last long, and are accompanied by pain and tenderness of the epigastrium, they denote, almost always, a thinned and softened condition of the mucous membrane of the stomach. They may be alleviated by a leech or two—by a blister—by the effervescing draught: or the prussic acid may be used; that is a medicine which certainly tranquillizes an irritable stomach, as it now and then allays an urgent cough. Sometimes, again, the bones of the miserable patient are laid bare, in consequence of pressure upon parts in which the circulation is already very feeble. We cover these with soap-plaster; take off the pressure by arranging cushions; or, what is much the best of all, we put the patient upon the water-bed invented by Dr. Arnott.

LECTURE LII.

Melanosis of the Lung; spurious, and true. Other forms of malignant Pulmonary Disease. Accidental intrusion of solid substances into the air-passages.

I ADVERTED yesterday to certain callings which, for various reasons, are unhealthy; and among the rest for this;—that the work-people engaged in them breathe habitually an atmosphere loaded with particles of matter which clog and irritate the pulmonary tissues, and establish at length a form of disease which might justly be regarded as a variety of phthisis. One morbid state in particular, thus produced, is by no means rare in some parts of this country. It has been sometimes called *spurious melanosis*, sometimes *Collier's phthisis.* The texture of the lungs is spoiled by matters carried in with the air, in the acts of breathing. The lungs are found after death to be throughout of a black colour, more or less uniform. Sometimes the pulmonary substance is dry and friable, as well as black; sometimes moist, œdematous, imbued with an ink-like fluid; not unfrequently broken down into irregular cavities of various sizes; and these cavities are often full of the same black liquor.

Laennec had conjectured that certain kinds of black discoloration of the lungs were of extraneous origin; were owing to the introduction of black matters from without in the process of respiration: and Dr. Pearson, in the early part of this century, had thrown out the same idea: but the soundness of these conjectures was first established by actual proof, about forty years ago, in Edinburgh. Dr. J. C. Gregory had a patient who died in the infirmary of that city, and whose lungs presented the following appearances:—They were both of a uniform black carbonaceous colour, which pervaded every part of their substance. The right lung was broken down, in its upper and middle lobes, into irregular cavities; and the walls of these cavities were black; and they contained a considerable quantity of black liquid like ink. Portions of the pulmonary substance were dense, solid, and friable. The rest of this lung was œdematous; and when the serum which made it thus œdematous was pressed out, it also, the serum I mean, was quite black. The left lung was imbued, in the same manner, with black serum. No tubercles could be detected. The bronchial glands were not enlarged, but they were stained of the same sable hue as the substance of the lungs. No other organ of the body showed any trace of this black colouring.

Analysis of the black matter contained in the serum expressed from these lungs was undertaken by Dr. Christison. I shall not follow out the details of his researches (you may read them at length in the 109th number of the 'Edinburgh Medical and Surgical Journal'), but content myself with stating their result. And I may state it in Dr. Christison's own words. 'In the product of this experiment (he says) it is scarcely possible not to recognise the ordinary products of the distillation of coal. A gas of the same quality was procured, and likewise a naphthous fluid holding in solution a crystalline principle, analogous to, if not identified with, naphthaline.'

Now the man, whose lungs presented the appearances I have described, had, for the last ten or twelve years of his life, been employed in the coal-mines at Dalkeith. He had been exposed therefore to the habitual inhalation of coal-dust into his lungs in breathing: and taking this circumstance in conjunction with the result of the analysis of the black matter contained in the lungs, and nowhere else throughout the body, we cannot doubt that the carbonaceous substance so abundant in these organs was introduced from without. In truth we have now a large amount of evidence in proof that it must have been so. In the twenty-first volume of the 'Medico-Chirurgical Transactions,' Dr. William Thomson has recorded the results of extensive inquiry into the subject. Thus he gives ten examples of black sputa going along with pulmonary symptoms during life, and of black infiltration of the lungs discovered after death; and of the ten persons who were the subjects of these observations, nine had been engaged in working coal-mines, and the tenth was a moulder at the Carron iron-works. He gives also six cases of black infiltration of the lungs, all occurring in persons exposed to the inhalation of carbonaceous matters (one of them was an engineer, and the others were all colliers); but in these six cases there had been no black expectoration noticed during life.

Similar instances of this black ingraining and staining of the lungs have since been published by Dr. Greenhow; and he has referred to two remarkable cases, related by Dr. Zenker, of Erlangen, in which the lungs, instead of being black, were of an intense red colour, like a brick or a tile—and imbued with a bright red fluid. In these cases the red oxide of iron, in very fine powder, had been habitually inhaled during life. The iron dust was used in the preparation of paper books made to contain leaf gold; and during the process of rubbing the dust into the paper, the air of the workroom was visibly reddened by the fine red powder.

That carbonaceous particles, floating in the atmosphere, may be, and must be, and actually are, drawn into the lungs during inspiration, no one who has been long in this smoky town can doubt. Many persons remark that they expectorate during winter, and while in London, a little mass or two of dark gray, dirty mucus, every morning; but when in the country, and in the summer, the mucus so spat up is transparent and clean. So I have noticed, and pointed out to some of you, that the *crachoirs* of our patients in the hospital often bear witness to the visitation, during the previous twelve hours, of one of our dense and dirty fogs, which come with an easterly wind, and bring with them a vast quantity of blacks, and soot, and smoke. Immediately after the prevalence of one of those filthy blankets of vapour, we find the contents of each of the little vessels given to the patients whose expectoration is kept for inspection, to be deeply tinged with black. In the year 1832, I had a patient whose sputa were remarkably loaded with dark matter. He came into the hospital complaining of cough, and of shortness of breath, and of a sensation at the lower part of the right side of the chest, as if it were pierced by needles; and he was spitting a considerable quantity of thick mucus, which was almost black. This colour never entirely left his expectoration while he remained in the hospital; but it greatly diminished, in proportion as his ailments were relieved. Now this man was a stoker at one of the gas-works. And he attributed his illness, which had come on gradually, to the great alternations of heat and cold to which his occupation exposed him; and the blackness of the sputa he ascribed to the continual inspiration of coal-dust. And no doubt he was right.

It may seem strange, if the inhalation of atmospheric air loaded with minute particles of coal or other carbonaceous matter be sufficient to produce this remarkable condition of the lungs, and the characteristic *black spit*, that the change, and its nature and cause, should not have been earlier recognised; especially when we consider the vast number of men who are employed in our mines and collieries. It appears, however, that great repugnance has existed, and probably still exists, among the labourers in the coal-mines, to allowing their dead to be opened and examined. And it appears further that the peculiar state of the lungs which I have been speaking of is produced in a comparatively small number of those who are so employed. The cause why some are thus affected, and some are not, has yet to be discovered. I recommend this as an interesting subject of inquiry to such among you as may have opportunities of prosecuting it. It has been conjectured that the specific change takes place, in a marked degree, only in lungs that were previously unsound. It is a question whether the cavities found in the pulmonary substance, in the fatal cases, were the result of the spurious melanosis; or of the expulsion of tubercular matter which had coexisted with, or preceded, the melanotic state. It is a curious circumstance that the black spit, as it is called in those districts, sometimes does not make its appearance until a considerable period has elapsed after the time when the labour in the coal-mines was given up. Dr. Makellar, who has described the disease as he met with it among the colliers in the Lothians, is of opinion that the blood becomes unnaturally loaded with carbon; and that when once a certain quantity of the black matter from without has been lodged in the pulmonary tissues, a disposing affinity is thereby created for the carbon in the blood, so that more and more of that substance continues to be deposited, even long after the patient has abandoned his occupation, and ceased to inhale the noxious atmosphere. Certainly this is a complaint that offers several interesting points of research, and requires further investigation.

This black staining of the pulmonary tissues from without has been sometimes confounded with an inbred disorder resembling it in its colour only—with what I briefly mentioned in a former lecture, *melanosis*, 'a cancer characterised by the presence of pigment;' and it has for that reason been called *spurious melanosis*.

I may take this opportunity, for I am likely to have no better, of telling you the little that has been ascertained respecting *true* melanosis.

The disease so called was first fully described and named by Laennec in 1806. It consists in a morbid product or deposit, having a black or deep brown colour, moist generally, and differing both in form and in consistence under different circumstances.

The black material is deposited, most frequently of all, in the areolar and in the adipous tissues; and it occurs in greater abundance, and in larger masses, according as these reticular tissues are more plentiful and more lax. It is met with also in the compound organs of the body; especially in the liver. Less frequently in the lungs. Sometimes in the eye. Occasionally in the brain; and even in the heart, and in the bones. The serous membranes are obnoxious to the same kind of disease; the mucous very little so. The black or dark-coloured matter may also exist, in a liquid condition, in the natural cavities of the body. And lastly, the melanotic material is sometimes mixed up with scirrhous and with brain-like malignant tumours.

With respect to the shapes in which it appears—it is sometimes dotted, the surfaces affected by it looking as if they had been thickly sprinkled over with coal-dust or soot. But more commonly melanosis assumes the form of solid nodules or tumours, of variable magnitude. These tumours are largest where reticular tissue is most loose and abundant. They may be no bigger than a pin's head, or they may be as large as a man's head. Masses of this kind have been found in the horse, weighing as much as six-and-thirty pounds. In the human subject they may attain the size of an orange. These large tumours (like large pulmonary tubercles) are usually formed by the union and agglomeration of several smaller ones, and hence they have generally a lobulated surface: while the shape of the separate smaller tumours is mostly spherical. Sometimes the areolar tissue forms around the melanotic masses a kind of cyst: more generally the black matter is in naked contact with the tissue, whatever that may be, in which it is lodged.

From the serous surfaces, especially from the pleura and peritoneum, knobs of a dark colour are seen in some instances to project; in others, round tumours, as big as peas, or cherries, hang from these surfaces by a sort of pedicle. The omentum is a common *habitat* of melanotic tumours.

Occasionally, I say, the black matter is found spread in a continuous layer upon the serous membranes; or is collected in a liquid state in their cavities. But this, compared with the occurrence of solid tumours, is rare.

When this remarkable disease is met with in one tissue or organ of the body, it is met with in others. It is never confined to one part, but pervades several: resembling in this respect both the scrofulous matter which constitutes tubercle, and the matter of cancer. Persons of fair complexion are said to be more liable to it than the swarthy.

Scattered notices of these singular and striking changes in the animal frame occur in the works of Morgagni and of Haller;

but since the period when Laennec first drew the special attention of the profession to the subject, the black matter has been carefully analysed by several expert chemists. Without going into any tiresome detail, which you would scarcely remember, as to its exact composition, it is interesting to know that it is very like that of the blood: and no doubt the material is somehow deposited from the blood. Very little, however, has been ascertained about its primary origin and cause. Some have supposed that the melanotic matter is analogous to the natural pigments which are found in the animal economy; all of which are known to be rich in carbon. It is a curious fact that the disease has been more often observed in white or gray horses than in others. (I should tell you that the complaint is not at all uncommon in various quadrupeds: examples of it have been noted in the horse, ox, dog, cat, rabbit, rat, and mouse.) It has been conjectured that, in white animals, the colouring matter of the surface, and of the hair, has been diverted, by some morbid process, from its proper locality. But the very same disorder occurs also, though not so often, in dark, or bay, horses and cows: and certain pathologists imagine that in these cases there has been an undue accumulation, in the blood, of the carbon which is destined to colour different parts. In the one case, you see, they hold that the pigment is misplaced; in the other that it is excessive. What value these speculations as to the nature and origin of the disease may possess, time alone can determine.

When the tumours are divided, and moist, or when they are rendered moist by admixture with water, they freely impart the colouring matter; staining white paper, and blackening one's fingers, just as Indian ink might do. The disease most frequently happens, when it happens at all, in the decline of life.

You will remark that the spurious melanosis is distinguished from the true, by its occurrence in those persons only who are somehow exposed for a certain length of time to breathe an atmosphere which is largely encumbered with carbonaceous particles; whereas true melanosis may occur in any place. The spurious discoloration never affects any other organs than the lungs and bronchial glands; the true black deposit of melanosis is never confined to a single organ or tissue. Moreover, the one disorder is absolutely beyond remedy; the other, as soon as its presence is rendered probable, by the black expectoration, and the pulmonary distress, may be mitigated, checked, perhaps gradually cured, by removing the patient from the operation of the exciting cause, and pursuing such other measures as the symptoms may seem to require. The distinction is not a matter therefore of mere curiosity: it bears upon the treatment to be followed, which is our proper business. It is connected also with medical police or hygiene, which we should all of us cultivate as extensively as we may; as a science intimately related to our strictly professional pursuits, and to the welfare of the community.

Like every other region of the body, the thorax is liable to the invasion of other forms of disease, unquestionably cancerous: and although these intractable maladies—intractable at least when internal—have less interest for the practical physician than diseases over which medicine has some acknowledged curative power, it is of importance that we should be able to recognise them, and even to know that it would be vain, and perhaps hurtful, to essay their *cure*. Most often it is not an easy thing to distinguish cancer in the thorax from certain other thoracic diseases; sometimes it is not a difficult thing. All that I can attempt in these lectures is to put you in possession of a few facts which have been generally noticed respecting this fearful kind of disease, and which may serve as guiding points in your future experience and practice.

Cancer of the lung, then, is for the most part *soft* cancer; cerebriform, or melanotic, or both. Primary cancer of the lung is very rare; secondary cancer is more common. When it is secondary, it usually takes its origin in primary cancer of the mamma in women; of the testicle in men. A singular case has been reported by Dr. Moxon, in which cancer seems to have been planted in the lung by fragments of epithelial cancer (slips or cuttings, we might metaphorically call them), which had passed from the upper part of the œsophagus into and down the trachea. Primary cancer, rare in itself, rarely affects both lungs. It has much more often been met with in the right than in the left lung. We have in these facts some help towards the diagnosis between primary cancer and tubercle, which is very seldom restricted to one lung. In secondary cancer, as we might expect, both lungs are apt to suffer. Some of the visible and of the audible symptoms in intra-thoracic cancer—retraction and diminished mobility of the chest walls, dulness to percussion—are just the same in kind as tubercular deposit may produce. The dulness, however, is not so often at the apex of the lung as in phthisis. Cavities are scarcely ever a consequence of pulmonary cancer. Other symptoms, again, are simply mechanical effects of intra-thoracic *pressure*, and are therefore often the same as may be caused by aneurismal, or indeed by any other, intra-thoracic tumours. Of these symptoms I postpone a more close consideration. Cancer within the chest frequently involves, or commences in, the mediastinum. From the lung it is apt to extend to the pleura, and to disclose its real nature by thrusting forward, here or there, the walls of the chest, or even penetrating through them.

Of course any suspicion of cancer of the lung would be greatly strengthened by our finding cancer elsewhere in the patient's body, or by tracing it in his history, or in the history of his family.

That same suspicion may sometimes be suggested by the appearance and quality of the matters expectorated. If the sputa are egregiously unlike the sputa that are known to be respectively characteristic of the ordinary diseases of the lung—of bronchitis, of pneumonia, of phthisis, of pulmonary gangrene—they probably belong to some condition that also differs essentially from any or all of these, and therefore, on the principle of exclusion, to some kind of malignant disorder. And this suspicion the microscope may sometimes settle.

In these sad and hopeless forms of disease our office is limited to the alleviation of suffering, and the means for that still blessed object must vary with the varying exigencies of each particular case.

There is yet another affection of the breath-machine, to which I must briefly direct your attention: a casualty that is apt to befall the air-tubes. I said nothing of this, indeed, last year; but having since witnessed an example of the accident to which I allude—the entrance, namely, of some solid substance into the windpipe—I have thus been reminded of my former omission, and taught at the same time how needful it is that every medical man should have well considered such cases. I was taken to Kentish Town, in the autumn (1837), by a professional friend, to see a child, into whose trachea a small nail, what is commonly called a *tack*, was thought to have passed. When I saw the boy, he seemed to have nothing the matter with him: but he had been subject, ever since the accident, to paroxysms of most violent choking cough; alarming his parents and his attendants for his life. There was good reason for concluding that the nail, which was missing, and which he said he had *swallowed*, had really got into the windpipe, and was still there, or in the lungs; and the question was much discussed, what ought to be done in such a case? The result was, that nothing was done: but that, after the lapse of several weeks, the nail was at length coughed up.

Now there are some interesting points arising out of this sort of mischance. Dr. Stokes has devoted a short chapter to the consideration of foreign bodies in the air-passages: and instances of that accident are more common than you might suppose.

It is, at first sight, a surprising circumstance, that a solid body of any considerable magnitude (a molar tooth for example) should be able to pass at all through the narrow chink of the glottis. But, supposing the chink to be plugged by the sudden entrance of a passing substance, just at the commencement of a forcible endeavour to inspire, when, of course, the opening is at the widest, that substance must necessarily sustain, as the chest expands, a strong degree of pressure from the external atmosphere: strong enough, often, to force it through. If you cork a bottle that contains air only, and sink it sufficiently deep in the sea, the pressure of the water will push the cork into the bottle. The condition of the lungs, in the case supposed, and the condition of the bottle, are analogous. A vacuum beyond the plug is attempted by the act of inspiring, and obviated by the displacement of the plug inwards. There are no such powerful forces called into action to drive the intruding substance out again.

The matters which have been actually thus caught in the rima glottidis, and even forced through, are, as you might almost expect, oddly various in kind. Morsels of food: the stones of fruit; of these there are many instances: teeth; three such cases are referred to by Dr. Stokes: portions of bone: pebbles: a piece of money: a nut: a nut-shell: a button: a musket-ball: a large shot: a fragment of nutmeg: iron nails: kidney-beans: ears of grass or corn; of these, four examples at least have been noticed; one is mentioned by Dr. Stokes, two are recorded in the 'Gazette Médicale,' and I show you a monument of the fourth, in this interesting preparation, for the history of which I am indebted to Mr. Mayo. The young son of an English nobleman was riding in a carriage, in or near Paris, and had an ear of rye in his mouth. The carriage made a sudden jolt, and the ear of corn disappeared. Little was thought about this at the time: but soon afterwards symptoms of pulmonary irritation set in, attended with hectic fever, and with the most fœtid expectoration. The boy gradually sank. The ear of rye lay, as you may perceive, in an abscess which was common to the right lung and to the liver, through the diaphragm.

If any of you have tried the boyish trick of slipping beneath your wristband an ear of bearded corn, you will have no difficulty in understanding how and why, with every movement of the parts in contact with it, the ear will travel onwards; and how improbable it is that such a substance should ever be expelled from the lungs by coughing. Yet, in one of the cases recorded in the 'Gazette Médicale,' by a physician whose sister was the subject of the accident, an ear of barley was so ejected, seven years after its entrance. During that long period she had suffered repeated attacks of copious hæmoptysis. Her recovery was perfect.

The very enumeration which I have just made may convince you that the accident is not a very unfrequent one; and it is more than probable that fatal cases happen, the nature of which escapes detection.

The results of the accident are various also.

In the first place, it sometimes causes speedy death by apnœa.

2dly. It may be followed by inflammation of the lung, and perhaps abscess; and *so* destroy life.

3dly. Death may ensue, after symptoms resembling those of chronic phthisis.

4thly. The 'foreign body,' as we oddly enough call it, may be expelled through the glottis, after a variable period of time. Sometimes, yet not always, its expulsion is the condition and the harbinger of the patient's recovery; but he is never safe while it remains.

Death may take place in a few seconds when the substance sticks in the glottis. Death *has* occurred within three days, when the substance had passed the glottis; and in eleven days when it had reached the lung. The intruding piece of matter has escaped, through the natural passages, after remaining imprisoned for seventeen years. In that case, the patient died, hectical and emaciated, a year and half afterwards.

It may be worth our while to consider these particulars somewhat more closely; and to inquire what, in different cases, becomes of the foreign body which thus, to use a common phrase, 'goes the wrong way;' and what the symptoms are to which it may give rise.

First, then, I say, it may get wedged in the slit of the glottis and produce immediate suffocation. I mentioned, before, the frequency of this kind of death by misadventure. If you are summoned to any one whom you find comatose, or apparently just dead, and you learn that he had been suddenly attacked with choking during a meal, lose no time in examining his pharynx and gullet. You may chance to save a life so. The accident often happens to persons who are drunk. No doubt it happens oftener than we are aware of. The attack is very likely to be mistaken for an apoplectic seizure.

In these cases of sudden choking, the morsel of food is not always caught in the rima glottidis. If it be large enough to stick fast in the *pharynx*, it may provoke, through a reflex action, an abiding spasm of the little laryngeal muscles, and so produce death by apnœa. The remedy for such an emergency, as Dr. Marshall Hall truly observes, must be *immediate*: and this is what he tells us should be done.

'Pressure being made on the abdomen, to prevent the descent of the diaphragm, a forcible blow should be made by the flat hand on the thorax. The effect of this is to induce an effort similar to that of expiration; the larynx being closed, œsophageal vomiting takes place, and the morsel is dislodged.

'If this plan fail, not an instant being lost, the pressure should be kept up on the abdomen, the finger should be introduced into the throat, and the same smart and forcible blow made on the thorax as before. By the irritation of the fauces the cardia is opened, and by the blow on the thorax (firm pressure being made on the abdomen) an effort similar to that of expiration, with a closed larynx, is made, and a direct vomiting ensues, and the morsel of food is carried away.'

It may be better, in some cases, to push it onwards.

2dly. The substance, if small, may, after it has passed the chink, remain in the larynx; entangled in its ventricles, or between the chordæ vocales. In that case it usually occasions very severe laryngeal symptoms—spasmodic gasping cough, choking sensations, stridulous respiration, and pain in the larynx—symptoms which harass the patients without intermission, until death ensues, or until the substance is driven upwards into the pharynx, or drops downwards into the windpipe. There is, however, one instance on record, in which a piece of gold was lodged for years in the ventricles of the larynx, without these distressing consequences.

3dly. Having passed the upper part of the larynx, it may stop, and become fixed beneath the cricoid cartilage, or in the trachea. In these situations, unless it quite blocks up the passage, its presence may be productive of but little distress. A wheezing or croupy sound during one or both of the movements of respiration, and some degree of pain and tenderness of the part where the substance was lodged, have constituted all the evidence of its position in the air-passages, in more than one instance. A very singular and whimsical case of this kind, related by Professor Macnamara, is referred to by Dr. Stokes. A boy had made a whistle, by perforating a plumstone, and extracting the kernel. This, during a strong inspiration, passed from between his lips, through the glottis, and became fixed transversely in the larynx. So little inconvenience did it create, that the boy, finding that he still whistled as he breathed, went about for some time, pleased to display this new accomplishment. For three days he continued to occupy himself in his childish amusements, suffering now and then a seizure of suffocating cough. He was then taken to the Meath hospital. He had no pain in deglutition; but he said that when the cough was severe, it caused pain in his throat. He had also uneasiness in the epigastrium, a bloated countenance, and a frequent pulse. The chest sounded well on percussion, and the vesicular murmur was natural. The fits of coughing were followed by white frothy expectoration. Laryngotomy was performed; but during the struggle and the convulsive cough which took place when the opening was made, the stone (so the patient declared) was coughed up, and swallowed. The symptoms were relieved; and the whistling ceased. But it was found that as the wound healed the distress and the whistling sound returned; which showed that the stone lay above the opening; and that the disappearance of the symptoms had been owing, not to its dislodgement, but to the admission of air below the point where it was fixed. Soon after this, however, it changed its place, passed down into the right bronchus, and then up again towards the larynx. By a second operation it was extracted; and the lad recovered without any bad symptoms.

4thly. The substance may get beyond the trachea, into one of the bronchi, and stay there. And it is a very curious fact, and one which has evident importance in respect of diagnosis, that it is almost always the *right* bronchus which the substance enters. Dr. Stokes has explained why it is so. The septum that divides the extremity of the trachea into two branches is not placed in the middle of the channel, but decidedly towards the left; so that any solid body falling down through the windpipe, is naturally directed into the right bronchus. Perhaps this tendency is aided by the more vertical direction, and by the somewhat greater capacity of that tube, compared with its fellow. Now you will readily apprehend what sort of symptoms would be likely to result from the impaction of a solid body in either of the primary bronchi. It would be very apt to excite inflammation of the corresponding lung, which inflammation would reveal itself by its proper signs; but it would produce peculiar auscultatory phenomena, prior to and independent of such inflammation. It would prevent, partially or altogether, the entrance of air into the lung of that side. Hence, when we have other reasons for thinking that a solid body has passed the glottis, if we find the vesicular murmur suspended or enfeebled in one lung, while percussion gives out its usual clear sound, we may conclude that the intruder is lodged in the bronchus belonging to that lung. It seems not improbable that collapse of a portion of the lung may sometimes result from its presence there. This would modify the symptoms; but still the same conclusion would be warrantable from them.

Dr. Stokes believes, and his opinion is fortified by his own experience on the subject, that smooth bodies (beans or shots, for example) are more calculated than such as are rugged and uneven to cause urgent distress when impacted in one of the bronchi; inasmuch as they more completely plug and obstruct the tube, thereby depriving the patient at once of the use of half his lungs. An irregular substance, which can neither seal the passage up, nor be closely grasped at all points by its spasmodic contraction, will probably occasion less dyspnœa, and at the same time will be less likely to be dislodged by the effort of expiration. Under these circumstances we look for more chronic symptoms.

5thly, and lastly. The intruding substance may not be fixed anywhere, but may shift its place from time to time; and this, in fact, is what most frequently happens; and when it does happen, it gives rise to a very striking and distinctive series of symptoms. Paroxysms of suffocating cough and extreme distress, when the substance is driven up into or near the larynx; with intervals of comparative quiet, and sometimes indeed of apparent health, when it subsides into the trachea or bronchi. But, during these intervals, the signs that sometimes mark its situation in those tubes may perhaps be discoverable.

There is, then, a set of general symptoms, which lead us to believe, or to suspect, that some solid body has entered the air-passages: and there are other sets of particular symptoms, which inform us, with more or less certainty, whereabouts it is fixed, or that it is not fixed at all. A person, previously in good health, is seized with violent cough and choking dyspnœa, suddenly, during a meal, or while he had in his mouth some loose substance, which he fancies he has swallowed. This is a sufficient clue to the probable nature of the case: and we next inquire for a sense of soreness in the windpipe, and wheezing respiration; for signs of bronchitis or of pneumonia, especially in the right lung; for signs of obstruction of the bronchus on one side, and especially on the right side; or for alternations of suffocating cough, with intervals of outward calm. In the last case, we may expect to find the bronchus unstopped during the periods of laryngeal irritation; and *vice versâ*.

When we know that a solid body has been entrapped in the air-tubes, our business is plain; there is no room, in my opinion, for hesitation: we must let the substance out through an artificial wicket. There is danger, so long as it remains in these vital passages, of speedy suffocation; of fatal damage to the larynx, or to the lungs; of cerebral mischief during the violent paroxysms of coughing. Convulsions and apoplexy have, under such circumstances, actually occurred. Against these perils there is no security, except in the early performance of tracheotomy. If the included substance be loose and smooth, it will presently be shot forth at the new orifice; if it be fixed, or angular, it may generally be extricated by a skilful and delicate hand.

Even while this sheet has been passing under the press (May, 1843), another instance has occurred of the same accident, and excited a degree of anxious interest in the public mind, scarcely less than is accorded to a royal illness. It befel a gentleman whose name was previously famous. Mr. Brunel, in amusing the children of a friend with some tricks of legerdemain, put a half-sovereign into his mouth; and the coin slipped, as from its size and shape it might easily do, through the chink of the glottis. It seems to have occasioned no very urgent distress. The patient was made aware, by some internal sensation, that it lay towards the right side. After more than three weeks had passed, the trachea was opened: but the piece of money did not come forth. Probably its weight prevented its being driven up and down the windpipe; and when it lay edgeways, its form did not oppose much impediment to the breath. The same weight, however, brought it back to the larynx whenever Mr. Brunel placed himself with his head downwards. In some of these experiments, coming crossways I suppose, it produced most violent cough, and feelings of impending suffocation: but in a final and happier trial, at the end of six weeks, it dropped out again, through the natural passage—just as a coin may sometimes, by good luck, be shaken out of a box through a slit in the lid.

A still more recent example of a similar mixture of bad and good fortune has been recorded by Dr. James Duncan in the 'Northern Journal of Medicine.' A man was amusing himself with tossing up a shilling, and catching it in his mouth. Suddenly it fell into his larynx, and produced violent cough and severe dyspnœa, which gradually subsided. The difficulty of breathing returned in paroxysms, upon his making a deep inspiration, or after certain movements of the body. When the larynx was compressed externally, the man felt that the coin was lying opposite to the cricoid cartilage. He was now held with his head downwards by three strong men, was shaken once or twice in that position, and his larynx was moved from side to side, when lo! the shilling re-entered his mouth, and dropped out upon the floor. During this process he suffered neither cough nor dyspnœa.

An almost exact counterpart of this case has since been communicated to me by Dr. G. B. Halford. In the year 1852, when that gentleman was house-surgeon to the Westminster Hospital, a man was admitted, about noon, who stated that on the previous evening, while entertaining his children by throwing a shilling into the air and attempting to catch it in his mouth, the shilling slipped into his windpipe. He had been to several medical men, and had taken emetic after emetic in vain. The shilling could be distinctly felt opposite the crico-thyreöid membrane. The man's voice was reduced to a whisper. Dr. Halford 'directed the porters of the hospital to turn him upside down in the corner of the surgery, when, after several expiratory efforts, the shilling rolled out of his mouth.'

Notwithstanding the speedy and easy dislodgement of the coins in these instances, it may be prudent and requisite, in less fortunate cases, when the piece of money does not fall, at first, in the right direction, to lessen the risk of suffocating cough, by making an artificial opening in the trachea. If the substance be within reach, it may be then plucked forth with forceps: if not, the safety valve thus established will render the inversion of the patient's body less distressing and less hazardous; and allow it to be repeated until it shall prove successful.

For Mr. Brunel's convenience a sort of platform was constructed, moveable upon a central hinge. To this platform he was bound in the prone position, by a broad strap passing across his shoulders; and then his head was lowered until the platform was brought to an angle of about 80 degrees with the horizon. This did not succeed until after the opening was made in his windpipe.

The invention of the Laryngoscope is calculated, henceforth, to throw light, in a twofold sense, upon the character and requirements of these accidents, whenever the foreign substance happens to be impacted either in the gullet or in the larynx. It may teach both what to do and what not to do. Instances are not wanting of its usefulness in facilitating the removal of serious obstruction from each of the two passages. By help of the mirror, Dr. G. Johnson discovered a copper penny sticking in the upper part of the *œsophagus* of a boy, twenty months old, who had swallowed the coin two days previously. At first the boy seemed on the brink of being choked to death. This distress, however, soon subsided, but left behind it utter inability to swallow solid food, and great difficulty in swallowing at all. Using a pair of long, slender, curved forceps, opening front and back, Dr. Johnson was enabled to seize the visible edge of the penny, and to pull it out. The boy was soon well.

In the forty-eighth volume of the 'Medico-Chirurgical Transactions,' Dr. Sanderson and Mr. Hulke narrate a case in which, by similar help, the *larynx* was set free. A man, twenty-seven years old, had in his mouth a sixpence, which, during a fit of laughter, suddenly disappeared. He was instantly seized with feelings of impending suffocation; but, as in the last case, these soon passed away, and for ten weeks he remained in good general health, not suffering from any shortness of breath, but unable to speak except in a whisper. Then all at once he began to have paroxysms of distressing dyspnœa. The missing sixpence was made visible by the laryngeal mirror, and seen to lie horizontally in the glottis, below the false cords. Between its free edge and the arytenoid cartilages a transverse narrow space was left, through which the man breathed. He was put under the influence of chloroform, and an incision was made, an inch and a half long, in the middle line from the thyreöid cartilage, downwards. Through this opening the coin was felt with the forceps, but the operator failed to seize it; it was felt also, by the forefinger of his other hand, in the fauces; and becoming displaced upwards, so as to be within better reach of the finger, it was drawn over the glottis on to the root of the tongue. Thereupon the patient, who was just beginning to recover his consciousness, gave a gulp, and said that he had swallowed the sixpence. It was voided the next day through the rectum.

These small coins are frequent intruders. A case very like the last has just occurred, of which I will briefly give you the details, for they seem to me very instructive.

A man, forty-one years old, was admitted into King's College Hospital on December 15, 1870, under the care of Mr. Henry Smith. Four days before, while drunk, he had put into his mouth a half-sovereign, which slipped into his larynx. The symptoms were pain in the larynx, a feeble husky voice, stridulous breathing, and cough with copious muco-purulent expectoration. On examination with the laryngoscope, Dr. Johnson immediately saw, and showed, the coin lying flat upon the vocal cords, which were entirely concealed by it; its margin on either side impacted in the sacculus laryngis; its surface partly covered by the swoln mucous membrane. Behind, near the arytenoid cartilages, a portion of the margin was free, leaving a small breathing space between the metal and the membrane. The position of the half-sovereign in this case was therefore exactly the same as that of the sixpence in Mr. Hulke's case. The coin was fixed so tightly that it was thought useless to attempt its dislodgement by turning the patient head downwards. As it lay with its flat surface upwards, Dr. Johnson found it impossible to seize it with a pair of laryngeal forceps; but that position was clearly favourable for an attempt to push it up from below. Mr. Henry Smith therefore divided the crico-thyreöid membrane, and inserting a bent probe, pressed upwards against the under surface of the coin, and pushed it into the patient's mouth; whence, his head being at the same time bent forwards, it fell into a basin before him.

The operation, which occupied a few minutes only, was done without chloroform; the wound in the throat was immediately closed; and the patient rapidly recovered.

LECTURE LIII.

Diseases of the Heart: usually partial. Changes in its Muscular Texture. Mechanism of those Changes. Natural Dimensions of the Heart. Natural Sounds. Modifications of these by Disease. Review of the Physical and General Signs that accompany Cardiac Disease.

You will perhaps accuse me, Gentlemen, of a disposition to magnify the importance of every new class of diseases at which we arrive, in our survey of the morbid conditions of the various parts of the body in succession. There are few complaints, in truth, which are not important; either from the sufferings and discomfort to which they give rise, or from their tendency to abbreviate the span of human existence. Yet of the strictly vital organs the derangements are necessarily the most perilous; and therefore, to us, the most interesting. Two props of the tripod of life we have passed in review, and seen how they may be weakened, and how they may fail altogether. The office of the *heart* is not less essential to life and health, than that of the brain, or of the lungs. The well-being of every portion of the frame depends upon its being duly supplied with healthy arterial blood, and duly relieved of that which has become venous: and this supply and relief require that the central organ of the circulation should be sound in its structure, and perfect in its working. But it is frequently otherwise. I can remember, indeed, the time when disease of the heart was thought to be a very rare thing; but it is now well known to be one of the commonest of disorders, and it connects itself with a variety of other affections, with which it was formerly supposed to have no relation.

Like other organs that are complex of structure and formed of different tissues, the heart is subject to *partial* disease. Its lining membrane alone may, in the first instance, become the seat of inflammation, with its various effects; or its investing membrane only may undergo morbid alterations; or the muscular substance that constitutes the organ itself may be gradually changed in its qualities, in its bulk, or in its proportions.

But the morbid conditions of the investing and lining membranes do not always, or immediately, compromise the life of the patient. They are fatal at length, in ninety-nine cases out of a hundred, through the alterations to which they lead in the muscle wherewith they are connected. It may be practically useful therefore to consider, first, these ultimate morbid states which are incompatible with the continuance of life; and then to trace them back to the next link in the chain of their causes, which will be found, in very many instances, to consist in some antecedent morbid state of the exterior or of the interior membrane.

The heart, you know, is a living forcing pump; a hollow muscular engine, with its chambers and their valved outlets, its contractile walls and their strength and thickness, so admirably adjusted, that the healthy balance of the circulation is continually maintained, under many varying outward influences and inward emotions which tend to disturb it. In treating of diseases of the heart we have to consider, therefore, the modes in which its mechanism may be spoiled or deranged; and the effects of such derangements.

Not only the component tissues, but different portions also of the organ, may be separately diseased. It seldom happens, indeed, that the whole heart is affected; although that is probably the vulgar belief. The left side is much more subject to morbid changes than the right: and when both sides are implicated, the alteration is almost always more decided and conspicuous in the left than in the right chambers.

In the rapid sketch which I attempted of general pathology, in the outset of the course, I pointed out the various kinds of alteration to which the tissues and organs of the body, and therefore the heart among the rest, are subject. One or more of the chambers of the heart, you will remember, may become larger or smaller than is natural; or have their walls increased or diminished in thickness, and consequently in power; or one or more of its outlets and orifices of communication may be widened or contracted: and the purposes and function of the organ will be more or less impaired by such changes.

In order, then, to have a clear conception of cardiac disease, it is necessary to analyse it, and to investigate the derangements of the several parts of the heart. And I begin with *hypertrophy*; augmentation of bulk in its muscular substance. And I must first of all define one or two phrases which are current among pathologists in respect of this condition.

The muscular tissue of one, or more, of the chambers of the heart may become thicker and stronger than natural, while the capacity of that chamber, or of those chambers, remains unaltered. The hypertrophy in that case is said to be *simple.*

But, while the muscular parietes are thickened, the corresponding chamber may become unnaturally large. This constitutes the *active aneurism of the heart* of Corvisart, the *eccentric* hypertrophy of more modern writers.

On the other hand, it has been supposed that the capacity of a cavity of the heart may diminish in size as its walls increase in thickness: that the hypertrophy may take place *at the expense* (as it were) of the chamber. This has been called *concentric* hypertrophy.

Now, of these three reputed forms of hypertrophy, considered in their relation to *disease*, two only, the simple and the eccentric, have any real existence. The third, or concentric form, never occurs, I believe, except as a congenital malformation. And of the two genuine species of hypertrophy, the eccentric—which is plainly a compound affection, consisting of *hypertrophy with dilatation*—is much the most common. The reason of this is to be found in the physical cause of the morbid condition, in most instances. The physical cause, in nineteen cases out of twenty, is some obstacle, mechanical or virtual, to the perfect accomplishment of the function of the chamber; some obstruction opposed to the free and thorough exit of the blood from it; or something which hinders the easy play of the organ. Hence, in the first place, a gradual yielding, or tendency to yield, in the sides of the affected chamber, from the continual and unwonted pressure of the accumulated blood against them: and in the second place, a *striving* action of the muscle to overcome the hindrance, or to counterbalance the obstacle; and consequently, according to the law formerly announced, an augmentation in the bulk of the muscle whereof the function is thus increased. If the hypertrophy, which is the result of a truly conservative process, keep pace exactly with the amount of the obstacle and *exactly balance* it, no dilatation happens, or next to none. But this is comparatively seldom the case. According to the principles of mechanics, a little distension of the spheröidal cavity must require an increase of force to propel from it a given quantity of blood, in the same time, through a given discharging orifice. So that incipient dilatation becomes (in addition to the supposed obstacle) an efficient cause of hypertrophy: and the two, the dilatation and the hypertrophy, commonly make progress together.

Even when there is no apparent mechanical impediment, incipient dilatation (and consequent hypertrophy) may spring from morbid conditions of the blood, whereby is put in action that stop-cock function of the minute arteries which has already been explained to you. In this way, as we shall see hereafter, eccentric hypertrophy does often arise, without any valvular fault or obvious physical obstacle to account for it, from renal disease, whereby the due purification of the blood is prevented.

Cruveilhier appears to have been the first to reject *concentric hypertrophy* from the catalogue of cardiac diseases. The smallness of its cavity, with a proportional increased thickness of its walls, was regarded by him as a transient condition of the ventricle, depending upon the mode of death. He found these phenomena very strongly marked in the hearts of all those whose bodies he had examined after decapitation by the guillotine: 'Les parois ventriculaires se touchaient dans tous leurs points.' It was therefore his opinion that the hearts which had been thought, by others, to present examples of concentric hypertrophy, were in reality 'hearts more or less hypertrophied, which death had surprised in all their energy of contractility.'

This question has since been considered by Dr. Budd, in a communication to the Medical and Chirurgical Society, which you may read in the twenty-first volume of its 'Transactions.' He has since favoured me with a statement of his matured views upon the subject.

The semblance of concentric hypertrophy is most common in the left ventricle; and depends upon the ventricle being nearly empty at the time of death, and upon the corpse being examined while the heart is contracted by the *rigor mortis*. The fallacious appearance is accordingly noticed in cases where, from the manner of dying, the left ventricle, or the entire heart, contains but little blood, and where, from the muscular power not having been previously exhausted, the rigor mortis is of long duration.

'In all these concentrically hypertrophied hearts (writes Dr. Budd) the ventricle may be readily dilated by means of the fingers, and always dilates of itself when the rigor mortis goes off.

'In the published cases of concentric hypertrophy, in which there was no disease of the valves (I have given eight such cases in my paper, and could now add a long list to them), there were no signs, or only very slight signs, of disease of the heart, during the life-time of the patient. This circumstance is sufficient proof that the cavities of the heart in these cases could not have been during life permanently in the contracted state in which they were found after death. A left ventricle that could scarcely contain an almond (a common form of expression in the description of these cases) would surely have caused a great impediment to the circulation.'

Moreover, concentric hypertrophy could answer no mechanical purpose ; nor could its formation be accounted for on mechanical principles. But 'concentric hypertrophy of a ventricle, in a high degree, with obstruction at its discharging orifice, and an extraordinary channel for the passage of the blood, occasionally exists as a congenital malformation; and in most cases, the right is the ventricle so affected.'

To resume. Recollect that there may be two distinct kinds of physical cause of excessive action of the heart, and therefore of hypertrophy. In the one kind, there is some obvious mechanical obstruction to the exit of the blood from one or more of the cavities ; a constricted state of the *orifices*, is the most common condition. In the other kind, without any such obvious mechanical bar or dam to the fluid, there is something unseen which hinders the free and efficient play of the heart. This unseen hindrance lies, in most cases, in the stop-cock spasm of the minute arteries. Even malposition of the heart may have a similar effect. If it be pushed, for instance, out of its proper place and posture by effusion into the pleura, or by distortion of the chest, it will not work with the same ease as when all is perfect and symmetrical; and the unusual labour imposed upon it fully to execute its office, may lead to hypertrophy. The causes of hypertrophy may therefore be situated *within* the heart itself, or *without* and beyond it: but in all those cases in which the effect of the hindrance or obstacle is to *detain* the blood in one or more chambers, the hypertrophy will be likely to be accompanied by *dilatation*: and, generally speaking, the hypertrophy and dilatation result from disease in some part which lies *beyond* the affected chamber, in the order of the circulation. Thus either a narrowing, or a dilatation of the aorta at its commencement, will tend to cause hypertrophy and dilatation of the left ventricle.

That *contraction* of the aorta, or of the aortic orifice, may have this consequence, you will have no difficulty in perceiving. The blood cannot so readily pass through the narrowed channel; hence it will tend to accumulate in undue quantity in the ventricle, and therefore to stretch and dilate it; and the increased muscular efforts necessary to drive the delayed blood onwards tend also to thicken the muscle itself. But it may not be so obvious that *dilatation* of the mouth of the aorta—a wider channel of egress—would also virtually prove an obstacle to the emptying of the ventricle. Yet it certainly would, in two ways. In the first place, dilatation of the entrance of the aorta implies a diminution in the elasticity of that vessel; and the blood after it has left the heart is urged onwards by the healthy elasticity. But, again, dilatation of the mouth of the aorta commonly implies an imperfect closure of that vessel by the sigmoid valves; so that, during the diastole, a part of the blood is apt to regurgitate from the aorta, and to keep the ventricle morbidly full. You see, therefore, that a deviation from the healthy state of the aorta, and of the valvular apparatus which lies at its mouth, may obstruct the course of the blood, and lead to hypertrophy and dilatation, whether the deviation be in the one way or in the other; whether, I mean, the natural size of the vessel be increased or diminished. Again, disease of the mitral valve, obstructing the flow of the blood at *that* point, will lead to an accumulation in the left auricle, in the pulmonary veins, and in the lungs themselves. The auricular action is always less regular and energetic than the ventricular, so that we less frequently meet with *hypertrophy* of the auricles, but very often with dilatation. And if we go to the other side of the heart, we find hypertrophy with dilatation, and more especially dilatation, of the right ventricle, when, from some reason or other, the blood passes with difficulty towards or through the *lungs*: either from disease of the pulmonary artery, or from disease in the substance of the lungs—emphysema, for instance; and if the difficulty be great, the accumulation and distension will affect successively the right auricle and the venæ cavæ; and then we have, in most cases, general dropsy. So that, I repeat, disease in the heart tends to propagate itself in a direction contrary to that of the circulation. Furthermore, if the muscular tissue of the heart be pale, flabby, soft and weak, as it frequently is in feeble, ill-nourished, cachectic persons, or if it have undergone that kind of fatty degeneration which I formerly described, it will the more readily yield to the centrifugal pressure of the blood it embraces. In this way we may have dilatation without any hypertrophy. I am anxious that you should in the outset comprehend the mechanism by which the natural dimensions and relative proportions of different parts of the heart may be altered in disease.

One reason why disease of the heart used formerly to be overlooked, was that these natural dimensions and relative proportions were not ascertained or much attended to. It is not easy to form any very precise estimate of the size of a healthy heart. It is commonly held that if the heart be about the same size with the closed fist of the subject, its general dimensions may be considered to be natural. Bouillaud, who has taken much pains with this matter, weighing and measuring a great number of different hearts, states that the *mean* weight of that organ, with the origin of its large vessels, and empty of blood, in adults from twenty-five to sixty years old, is from eight to nine ounces; that in subjects from sixteen to twenty-five years old it may be one or two ounces less; and that, in very large and robust persons, it may rise to ten or eleven ounces. Also, what we should expect, that the weight is less in women than in men. Age, too, has its influence. *Cœteris paribus*, the weight of the heart after middle life increases with increase of years; in consequence, I imagine, of the resistance offered by increasing rigidity of the aortic walls.

So much for the general bulk of the heart. And we must have some standard whereby to estimate its relative proportions. Every one knows that the walls of the left ventricle are thicker than those of the right. Bouillaud found that the mean thickness of the walls of the left ventricle *at its base* was seven lines, while that of the right ventricle was two-and-a-half lines. And taking the thickness generally, he says, that the thickness of the parietes of the right ventricle has not a greater ratio to that of the parietes of the left, than two to five, or even than one to three.

So again of the auricles: he lays it down that the mean thickness of the walls of the left auricle is to the mean thickness of those of the right as three to two.

He holds also that the mean capacity of the right ventricle exceeds, by a little, that of the left: and that the right auricle is larger than the left. Dr. Kirkes, however, thinks that probably 'the capacity of the two ventricles is exactly the same. It is difficult to determine with certainty how much this may be; but taking the mean of various estimates, it may be inferred that each ventricle is able to contain, on the average, about three ounces of blood, the whole of which is impelled into their respective arteries, at each contraction.' You must always make allowance in actual cases for the possible distension of these cavities with blood, beyond the size to which they would have contracted if they had contained no blood.

I may add, that Bouillaud declares the rule I just now mentioned, which had been propo before his researches were instituted—the rule, viz., which makes the bulk of the healthy heart equal to the fist of the subject—to be tolerably correct. By keeping in mind these general facts, you will be better able to appreciate the appearances presented by the heart when it is taken from the body to be examined: but you will recollect that they relate to *averages* only.

Now having pointed out the modes in which the natural proportions of the heart, and of its several parts, may be morbidly altered; and given you a rough standard which may enable you to estimate these proportions in the state of health, and the deviations from them in the state of disease: I will go on to consider the symptoms, by which the altered conditions are accustomed to declare themselves. And it is with respect to the heart as with respect to the lungs; there are *general* symptoms or signs, and there are *physical* symptoms or signs: and the information derived from these sources respectively is of variable utility. Neither of them can be safely neglected; and it is often found that the indications derived from one of these sets of symptoms are confirmed or corrected by those collected from the other. I believe it will be best to pursue the same course in both cases, and to speak, in the first place, of the signs that are brought within our notice by the sense of hearing.

But, in order that we may comprehend the *morbid sounds* of the heart, we must first make ourselves acquainted with those that belong to its healthy condition.

The heart may be heard by the ear, laid flat against the præcordial region, or through a stethoscope, to beat over a certain space. That space, in ordinary circumstances, corresponds to the inferior half of the sternum, and to the cartilages of the ribs from the fourth to the seventh, on the left side. The apex of the organ may often be *seen* to pulsate between the cartilages of the fifth and sixth left ribs; about two inches below the nipple, and one inch from it towards the sternum.

This is the space over which, in the sound state of the heart and lungs, the pulsations of the former are plainly audible. But there are several diseased conditions, both of the heart itself, and of the parts around it, which interfere with this rule.

In the first place, if the heart be larger than natural, it will be heard to beat over a proportionally large space. In this way it may come to be heard all over the chest in front; and behind on the left side of the spine; and even, in extreme cases, on the right side of the spine.

Again, the extent of space over which the heart may be heard to beat will be increased in proportion to the thinness of its walls; and diminished, *cæteris paribus*, according to the thickness of its walls. So that when the heart has nearly its proper size, if its walls be thin, it will be heard beyond its natural limits; and if its walls be morbidly thick, *i.e.* if it be affected with considerable hypertrophy, it will not be heard beyond, nor even to the extent of, its natural limits. I will endeavour, presently, to explain the reason of these differences.

Again, and this it is of great importance to remember, the heart may be heard far beyond its natural limits, even when it is perfectly healthy, in consequence of the lung between the ear and the heart having become solid, and therefore a better conductor of sound: and the solidification may have resulted from hepatisation, or from the presence of a number of crude tubercles, or from cancerous deposits. The sound of the heart's action will also be conveyed to a distance by the liquid effusion in pleurisy, and by aneurismal tumours. If we are not aware of these circumstances, we are continually liable to fall into mistakes.

The heart is likewise heard more distinctly, and over a space which is comparatively larger, in children than in adult persons; in those who are lean and spare, than in those who are fleshy and fat; and I need scarcely say that it may be heard over a wider extent of the chest whenever its action is augmented by exercise, by emotion of mind, or by febrile excitement.

The *impulse* of the heart is another point which you must attend to. In healthy persons who are thin, you may generally feel the stroke which the heart gives to the ribs, by placing your hand on the præcordial region. In persons who are fat, you often cannot feel the heart at all in this manner. For obvious reasons, it is felt more distinctly, over a larger space, and higher up, while the person is stooping forwards, or makes a forced expiration; less distinctly, over a smaller space, and lower down, when he makes a deep inspiration, or is lying on his back. In proportion as the heart is enlarged by disease, it can be felt more extensively: and when there is hypertrophy, the force with which it strikes the parietes of the chest is sometimes extraordinary, and very instructive. You will see the ear and head of the listener distinctly lifted at each pulsation. Sometimes the whole of the patient's body, nay his very bed, is shaken by the strong shock of the heart during its systole. There is no sign of hypertrophy so sure as that afforded by the heart's impulse. You feel, not a smart, quick, and sudden knock, but a steady, heaving, irrepressible swell, which is perfectly characteristic. You may always infer increased thickness of the walls of the organ, when you meet with this regular heaving motion; and the extent to which the whole heart is enlarged in such cases may be conjectured by the extent of space over which the heaving impulse is perceptible.

The *sounds* which we hear are two. One of them coincides, in point of time, with the impulse: and barely precedes the beat of the radial artery. It happens, therefore, when the ventricles contract; during the systole. It is called, accordingly, the *systolic* sound, or the *first* sound of the heart. The other of the two sounds coincides with the diastole, and is spoken of as the *second* or the *diastolic* sound. It takes place at the instant when the heart reverts to that place and condition in which it had been prior to the systolic movement. These two sounds occur in quick and regular succession, and then follows an interval of silence, after which the two sounds are repeated; and so on.

The two sounds are not, however, exactly alike. They differ somewhat, both in quality and in duration. The first is a dull, prolonged noise; the second a shorter and smarter sound, having more of a clacking or flapping character. Attempts have been made to assign the respective duration of each sound, and of the period of repose. I confess that I have never succeeded in measuring them satisfactorily in my mind. Probably Dr. C. J. B. Williams' estimate is as near the mark as any. He divides the whole period, from the beginning of one pulsation to the beginning of the next, into five equal parts: and allots two of these to the first sound, one to the second, and the remaining two to the interval of silence. This order of succession is called the *rhythm* of the heart, and it may be perverted.

Respecting the physical *causes* of these natural sounds there have been much recent discussion and research. Our time, however, will permit me to do little more than tell you what I believe to be the facts of the matter. And I take, first, the diastolic sound, as being the simpler of the two. It used to be ascribed to the contraction of the auricles: but that was quite a mistake. The contraction of the auricles, such as it is, happens *immediately before* each systole of the ventricles: whereas the sound in question occurs *immediately after* it, and is succeeded by the period of silence. This we know from the visible movements of the organ when exposed in a living animal. In truth, the auricular contractions are very feeble, and during health are not attended with any appreciable noise. I have no doubt that the second sound is produced mainly, if not altogether, by the sudden shutting of the floodgates placed at the mouths of the two great outlets of the heart. The recoiling blood forces back the semilunar valves of the aorta and of the pulmonary artery, as one unfurls an umbrella; and with an audible check as they tighten. There is no other tenable mode of accounting for the sound. Experimenters have contrived, by hooks and wires, to prevent these valves from unfolding; and then the flapping sound has been converted into a hiss. Disease of the same valves demonstrates the same things; as we shall presently see. Nevertheless, it is both possible and probable that the relapse of the whole organ to its former place may contribute an ingredient towards this second sound.

The first, or systolic sound, is more complex. Physiologists are not yet agreed as to its cause. Upon this disputed question I cannot pretend to speak authoritatively. In all probability it is a compound sound: but it must be *chiefly* produced by the collision of the blood with the inner surfaces of the ventricles, and with the surfaces of the tricuspid and mitral valves. In part it has been attributed to the impulse of the heart against the ribs. It has been thought to consist also, in part, of the sound that results from the muscular contraction of the ventricles: that the systolic sound commences with the tightening of the walls of the ventricles, including the valves; and is prolonged by the muscular noise. You are aware, I dare say, that the vigorous contraction of a large muscle is accompanied by audible sound. If, during the stillness of night, when lying in bed, with your cheek and ear upon the pillow, you set your teeth firmly, you will hear a continuous dull rumbling, like the noise of carriage wheels in the street, and evidently caused by the action of the masseter and the temporal muscles. Dr. Williams states that, with the help of a flexible stethoscope, one may hear the voluntary jerking contraction of his own abdominal muscles: the sound being as loud as that of the heart's systole, and very like it in character. But Dr. Halford has satisfied himself, and many others who have witnessed his repeated experiments, that when the entrance of blood into the heart is prevented by forcibly compressing the venæ cavæ and the pulmonary veins, the movements of the heart go on, but the characteristic sound ceases, to recommence at once upon the readmission of the blood. The presence of the blood in the heart during the systole seems therefore essential to the production of the sound. My own opinion is that the impulse against the ribs, and the muscular rumbling, have some share (a small share probably) in causing the sound, but that it is mainly due to the collision between the blood and the walls of the cavity which contains and moves it. Dr. Halford however declares that *all* sound ceases upon the exclusion of the blood; and he contends that both the sounds 'depend upon the same cause, which is simply the backward current of the blood producing forcible closure and *tension*, first of the auriculo-ventricular (first sound), and secondly of the ventriculo-arterial valves (second sound).'

The natural sounds which I have been describing are liable to be changed, or modified, by disease. I just now told you that, *cæteris paribus*, the heart is heard more clearly and extensively when its walls are thin, less widely and loudly when they are thick. Of this Dr. Williams offers the following explanation:—'The transition of a thick muscle from slack to tight can never be so complete and sudden as that of a thin one; where there are many fibres they choke and muffle each other's vibrations; hence the sound is dull and prolonged, rather than loud and clear. If we observe the different sounds produced on tightening thin silk, and thick baize or cloth, we find that the thinness of the silk gives a unity and briefness to the impulse which it receives, and the sound is short and loud; whilst in the baize the impulse is divided and prolonged in the complexity of the fibres, and the sound is dull and less brief: so, under similar circumstances, a thin ventricle will give a louder, sharper sound than a thick one.'

But other modifications of the natural sounds, of a more striking and extraordinary character, are yet to be explained. Either sound, or both, may be accompanied by a noise, which, in its commonest type, very closely resembles that produced by the blowing of a pair of bellows. Four persons out of five, I should think, if they were asked what this sound resembled, when they heard it accompanying each systolic movement of the heart, would say that it was exactly like the repeated blowing of bellows in an adjoining room. It is called, accordingly, by the French, the 'bruit de soufflet;' and in homely English, a *bellows sound*. This is the generic sound. It may be divided into species; but it is scarcely worth while so to divide it. We are only likely to confuse our notions by over-refinement. So I will only add, that, when this bellows sound is very harsh or rough, persons will tell you that it is more like the noise of a rasp, or a file, or a saw: but all the while it is some kind of bellows sound. These sounds are often denominated *murmurs* also.

Now what is the cause of this singular deviation from the natural noises made by the successive contractions and relaxations of this hollow muscle, the heart? The whole matter may, I believe, be briefly thus expressed. The blowing sound may be occasioned by any change which alters the due proportion between the chambers of the heart, and their orifices of communication with each other, and with the blood-vessels that respectively enter or leave them; it may also be occasioned by a preternatural velocity in the passage of the blood through a healthy and well-adjusted heart. Dr. Elliotson, I think it is, who has offered this apposite illustration of the phenomenon. If the arches of a bridge have a certain relation to the quantity of water in the river, and to the force of the current, the water passes through them quietly, and without any noise. Diminish the width of the arches, and the water begins to go through them with an audible rushing or roaring sound. The very same thing will happen if the arches remain unchanged in size, but the quantity of water in the river, and therefore its velocity and force, be augmented by heavy rains. So it is in the heart. If one of its orifices—say the aortic orifice—be narrowed, by disease of the valves, or in any other way, the blood will not, as before, glide through it smoothly and without noise, but will yield that sound which we call a bellows sound. So also, if the orifice retain its natural dimensions, but the capacity of the cavity from which the blood is driven be augmented. Nay, the same blowing sound may be produced though the cavities and orifices are all healthy, and duly proportioned to each other, if the velocity of the circulating blood be increased beyond a certain limit. If you bear this explanation in mind, it will be found applicable, I think, to almost every case in which there is a blowing sound accompanying the *systole* of the organ. If, at the same time, the valves over which the blood must pass be rigid, or rough, or even loose and vibrating, those circumstances may modify the blowing sound, and render it louder, or hoarser, than it would otherwise be, and justify the appellations of *bruit de scie*, and *bruit de râpe*, with which you will find the French books full, and many of our English books also.

But this explanation applies to a *systolic* blowing sound only.

What are we to say when there is a similar sound attending the diastolic movement of the heart? Why a diastolic bellows sound will mostly, if not always, be found to result from and to denote some organic disease affecting the valves of the heart. Thus, if the mitral valve be converted, as it often is, from a pliable folding valve into a bony and rigid unvarying chink, the blood which passes through it from the auricle to the ventricle, during the diastole, may (though it seldom does) cause a rushing or blowing sound. On the other hand, the reflux of blood through the unshut mitral orifice, during the ventricular contraction, may also be attended with an audible noise; and thus we have another and not unfrequent source of a *systolic* murmur. Again, if the aortic valves are imperfect, as they often are, and do not effectually close that vessel, blood will regurgitate through them during the diastole, and produce a bellows sound. That this is the true explanation of the diastolic murmurs, I am convinced, both by the observation of disease, and by the results of experiments upon living animals. In some which were made by Dr. Hope, and which he was good enough to allow me to witness, the short clack of the diastole was at first distinctly audible; then hooks were introduced, so as to prevent the perfect closure of the sigmoid valves during the diastole, and then the short smart clack was converted into a prolonged bellows murmur; and upon letting them go again, the short smart clack recurred. The presence of a diastolic bellows sound has repeatedly enabled me to announce some disease of the sigmoid valves, interfering with their proper function—that of forbidding the re-entry of the blood into the ventricle from the aorta; and what I have thus foretold during life, has been verified by observation after death.

Bellows sounds are occasionally caused by accidental circumstances, extraneous to the heart: by mere posture sometimes, or by distension of the abdomen, either of which may *tilt* the heart, and alter its position with respect to its great vessels. Instances are recorded of bellows sounds removed by tapping the belly in dropsy, and reproduced by the reaccumulation of the dropsical fluid. You may even *make* a temporary bellows sound by forcibly pressing your stethoscope upon the præcordia, especially in children, in whom the ribs are feeble and yielding. And you may fall into errors of diagnosis if you are not aware of this.

Such, then, are the principal sounds, natural and morbid, which are audible by the naked ear applied to the region of the heart, or which may be heard through the stethoscope. But we derive assistance, with respect to cardiac disease, from percussion also. It enables us to measure, in some cases, the bulk of the heart; in others, to ascertain that the pericardium is distended by fluid. In the perfectly healthy state of the viscera of the thorax, the heart is somewhat overlapped by the thin edge of the lungs; and the sound elicited by percussion over a part of the præcordial region is intermediate between the hollow sound rendered by lung, and the flat sound yielded by the solid heart. In the centre of the præcordial region, where the heart is not covered by lung, the sound is decidedly dull. When, however, the heart is enlarged by disease, a larger part of its surface is exposed, and a larger portion of the præcordial region gives a dull sound on percussion. And when the pericardium is full of liquid, which distends and expands it, you will sometimes find that not less than a third part of the anterior and lateral portion of the left side is quite dull: and it is interesting often to measure, by percussion, the diminution or extension of the limits of the dulness, as the amount of effused fluid decreases or augments.

What I stated before, concerning the effect of different positions of the body upon the space over which the healthy beating of the heart may be heard, felt, and sometimes seen, applies, *mutatis mutandis,* to the natural dulness which the heart causes when the præcordial region is percussed. The space comprehended by this dulness is thus defined by Dr. Latham. 'Take the fifth costal cartilage on the left side, and let a point, midway between its junction with the sternum and its junction with the rib, be the centre of a circle two inches in diameter. This circle will as nearly as possible define the space of the præcordial region, which is naturally less resonant to percussion than the rest.' The dulness should diminish or disappear, in the supine position, and when a full breath is drawn; and increase in degree and extent upon a forced expiration, and when the posture is prone.

There is another physical sign which is much dwelt upon by Laennec, and which is sometimes very striking. In certain conditions of disease, the hand placed over the situation of the heart perceives a peculiar thrill or vibration accompanying its movements. The sensation conveyed to the hand is really very much like what Laennec compares it to, viz. that tremor which you feel when coaxing the back of a cat while it is purring with pleasure. Accordingly he calls this sensation 'frémissement cataire,' the purring thrill. You feel this vibration often when there is present also a loud and strong bellows sound; and Dr. Thomas Davies was of opinion, that the bruit de soufflet, and the frémissement cataire, constitute, in fact, but one phenomenon, which is rendered evident to the touch by the vibrations communicated to the hand, and to the hearing by the vibrations communicated to the ear, through the solid walls of the chest. I know, however, that the frémissement cataire does accompany other sounds, as well as the bellows sound: sounds of which I have not yet had any occasion to speak, but which I shall make you acquainted with when we come to the subject of pericarditis. And I pass from this general account of the sounds belonging to the action of the heart, in health and in disease, to consider the other symptoms by which we judge that such disease is present.

Among the general symptoms, then, of cardiac disease, some are direct—as pain; palpitation or excessive action of the heart perceptible by the patient; irregular or intermittent action, which the patient may or may not be conscious of: and some are indirect, declaring themselves through the medium of other parts and organs—such are dyspnœa; cough; dropsical accumulations; hæmorrhages; various affections of the nervous system, especially an increased and morbid sensibility, what is usually called *nervousness*: and some others, which I will cursorily notice as we proceed.

I shall take this opportunity of considering, once for all, some of these symptoms; whether they really proceed from organic disease of the heart or not: for the determination of the question, whether they do or do not indicate such disease, is often of great moment, and is not always easy.

We are not, in general, sensible of the beating of our hearts: but when the pulsations become inordinately forcible, they make themselves felt, and the sensation is, in many cases, a most troublesome and distressing one. Palpitation implies increased force, or increased frequency—or an increase both in force and in frequency—of the contractions of the heart. Every one has experienced palpitation in his own person who has run himself out of breath. The pulsations are sometimes tumultuous also, and irregular, as well as unduly frequent and forcible; but this is by no means always or necessarily the case. There may be great palpitation with perfect regularity of the heart's action. The increased beating not only can be felt internally by the patient, but it may often be heard both by himself and by others. However, we do meet with persons whose hearts throb with excessive violence, without their being at all aware of it. Such cases are always, I believe, cases of disease; whereas the palpitations that annoy and harass the patient are very often connected with functional disorder only.

Irregular action of the heart consists in some derangement or discord of its rhythmical movements, and is discovered by the condition of the arterial pulse—by unnatural fluctuations in the strength, or in the number, of its beatings, or in both. Sometimes a few rapid and feeble pulsations occur at uncertain intervals, and are followed by others that are fuller and slower. Sometimes one or more beats are left out, and the next beat, as if to make up for this pause, is unusually strong. The pulse is then said to *intermit*. The intermissions may be unperceived by the patient himself; but often they are attended with a singularly disagreeable fluttering, or trembling sensation in the breast. The pulse may intermit though the heart does not: the ventricle may now and then contract so faintly as not to propel a wave of blood so far along the artery. Intermission implies irregularity; but the action may be irregular and disorderly without intermitting.

Now, any of these deviations from the natural rhythm and action of the heart alarm people very much, and impress them with a belief that they have some fixed disease of that organ; and you will continually be appealed to for your opinion on this point. I suppose there are few medical students who have not, at some time or another, admitted into their minds the apprehension that they had disease of the heart; an apprehension engendered by its occasional palpitation or irregularity. For though there may be palpitation without irregularity, yet it is practically convenient to consider the two together.

These deviations certainly belong both to organic disease and to mere functional disorder of the heart; but I repeat, that in a great number, nay, in a great majority, of the cases in which they so distress and alarm the patient as to lead him or her to complain of them, they are unconnected with any change of structure; and this it is of much importance that you should be aware of.

Palpitation of the heart, and intermission or irregularity of the pulse, are often dependent upon some disordered condition of the stomach, and will cease at once when that disorder is rectified. It is curious that this may happen although the gastric affection does not manifest itself by any other symptom: and it is curious, too, how slight a cause may suffice to produce the irregular action. A friend of mine, a barrister, used to be very anxious about himself, because a fluttering sensation frequently occurred at his heart; an intermission of one or two beats, and then a violent throb when the organ again resumed its play. This is a sensation very familiar to my own consciousness, and probably most persons have occasionally experienced it. However, it happened so often to the gentleman I speak of, that it made him very unhappy. He persuaded himself that he had disease of the heart, and that he should some day suddenly drop down dead. But there was no other symptom of cardiac disease, direct or indirect, general or physical. He was accordingly told that the intermission depended upon some fault in his digestive organs; and he was advised to leave off different articles of food and drink in succession, in order to discover whether any one particular thing offended the stomach, and gave rise to the symptom. He began by abstaining from tea, of which he had been in the habit of drinking a large quantity; and thereupon the fluttering of the heart ceased. After a while he took to tea again, and then the fluttering returned. He repeated the experiment many times, and always with the same result, till at length his mind was satisfied; and by renouncing tea altogether he got rid of his palpitation and of his apprehensions. I mention this instance, because it came within my own cognizance; but it is only a sample of many such, and *tea* is frequently found to be the disturbing agent.

I must caution you, however, against the mistake which is often made, of inferring that the heart is free from organic change because its irregular movements are accompanied by dyspeptic symptoms. Structural disease of that organ is very apt to derange the digestive functions. You will commonly find that patients who labour under such disease are exceedingly liable to flatulence of the stomach; and free eructation of the gas which plagued them mitigates wonderfully the cardiac distress. It does so, no doubt, by relieving the diaphragm from that upward pressure which had embarrassed the motions of the heart.

We judge that palpitations and irregularities are merely symptomatic consequences of gastric disorder when they occur occasionally only; when the rhythm of the heart is perfect during the intervals; and when we fail to discover any other physical or general signs that its texture has undergone alteration.

Besides these over-strong or irregular movements, which are symptomatic of disorder of the stomach, and are remedied by correcting that disorder, there are palpitations of a purely nervous kind. I mean that they depend upon a peculiar and highly sensitive condition of the nervous system; which condition is itself dependent, in general, upon a particular state of the vascular system. Persons of a 'moveable' constitution, whether male or female, are subject to these palpitations: but especially young women: and, of these, such as are pale, exsanguine, hysterical, in whom the menstrual functions are deficient, or excessive, or somehow unnatural. Anæmia, if not a constant, is certainly a frequent and most remarkable feature of this nervous state. The blood is aqueous; poor in fibrine, and in red particles. The age, and frequently the sex, of the patient form leading points in the diagnosis. Nervous palpitations are apt to come on when the patient is quite at rest: palpitations that result from organic disease are, on the contrary, mitigated, usually, by repose. The occurrence of palpitations *in the night*, however, is but an equivocal circumstance, for nervous persons who dream, awake often with palpitation; and the recumbent posture is apt to excite or to aggravate the palpitations that are organic. Neither, in forming our diagnosis, can we trust implicitly to the presence or absence of physical signs. The heaving impulse of hypertrophy is indeed wanting; but, as I told you formerly, the short abrupt knock of chlorotic palpitation is often attended with a systolic bellows murmur: and this murmur is not confined to the præcordial region, but may be traced distinctly in the subclavian and carotid arteries. These unnatural sounds are common in persons whose blood has been drained of its red particles by frequent hæmorrhages, or by copious or repeated venæsection. We may suppose, in seeking to explain them, that the weak and flabby heart dilates a little, so that the healthy proportion between its ventricles and their outlets is for a time disturbed. But the sounds must in some other way also be dependent upon the thin and impoverished condition of the blood in such patients; and this reminds me of another diagnostic clue with which you should be acquainted. In nervous susceptible persons, especially if they exhibit the pallor of spontaneous anæmia, or are blanched by loss of blood, very curious noises are often audible by the applied ear or through the stethoscope, in the *neck*. Continuous rushing or roaring sounds, very like those which are to be heard in shells, and which poets feign, and the vulgar believe, to be the noise of the distant sea.

> Shake one, and it awakens; then apply
> Its polished lips to your attentive ear,
> And it remembers its august abodes,
> And murmurs as the ocean murmured there.

Sometimes the sound is more like the hum of a gnat, or the sighing of the wind through a crevice. Dr. Hope very truly states that it may be imitated, by a prolonged whispering pronunciation of the syllable *who*. Bouillaud, from its resemblance to the whizzing of a well-known toy, the humming-top, calls it the 'bruit de diable.' He fancied this singular sound to proceed from the arteries of the neck: but it is quite distinct from the true arterial bellows murmur, and it has been clearly shown (first by Dr. Ogier Ward), that it is produced by the descent of the attenuated blood through the great cervical veins. The sound, though continuous, has often a marked and regular increase, or swell, which keeps time with the heart's systole, and is believed to depend upon the pulsating pressure of the contiguous artery. It is best heard on the right side of the neck, just above the clavicle, and just behind the posterior edge of the sterno-mastoid muscle. The sound is loudest when the body is in the upright posture, and it increases whenever the circulation is anyhow quickened. It may be suspended at pleasure, by gentle pressure made above the applied stethoscope, upon the track of the veins, so as to stop the current of blood through them, without arresting the pulsation of the arteries. This proves that the murmurs are *venous*. I have no leisure to go more into particulars concerning these sounds; but when you meet with them, concurring with cardiac palpitations, in a young, nervous, anæmic subject, the palpitations, ninety-nine times in a hundred, will turn out to be simply functional—independent of any organic disease. No doubt there may be co-existing change of structure; but that is a rare exception, and when it does occur other signs proper to structural disease will be present, and will betray it.

Now these palpitations, and these musical or rushing sounds in the jugular veins, are to be cured by remedying the state of the blood. And the remedies are preparations of steel, aloetic purgatives, animal food, the cold shower-bath, and exercise, short of producing great fatigue, in pure air.

I have further to remark, with respect to intermissions of the heart's action, and therefore of the pulse at the wrist, that they are frequently connected, both in health and in disease, with feebleness, and also with unusual slowness, of pulsation. So that a slow pulse which is likewise feeble is often converted into an intermitting pulse by depletion; by blood-letting, for example, or by an active purgative; and the intermittence may be removed again by a stimulant. I mention this now, because there is another and very different state of disease, in which the pulse is apt to intermit. I mean when there is *plethora capitis*, and cerebral mischief is present or impending. But then the pulse will be full and strong, and labouring. In these cases a stimulant treatment would of course be injurious: while blood-letting, which would *cause* the other form of intermission, is the *remedy* of this.

Some assistance in determining between organic disease and mere functional disorder of the heart may perhaps be derived from observing the position of the patient. It is stated that when there is mere nervous palpitation, the patient lies as well, and perhaps better, on the left side than otherwise: whereas, when the heart is actually diseased, the decubitus on the right side is more comfortable than that on the left. If there be any tenderness of the heart, or of its enveloping membrane, the posture on the right side is supposed to be the easiest, because the heart is further removed from the ribs, and impinges upon them during the systole with less force. However, no great stress can be laid upon this symptom.

Of the remaining general symptoms of heart disease there is not much to be said. Hæmoptysis is an equivocal symptom. Dyspnœa and cough are indirect symptoms declared through the lungs, between which and the heart there is a close and obvious reciprocal influence. But dyspnœa and cough are direct symptoms of pulmonary disease; and even of pulmonary disease they scarcely help the precise diagnosis. That disease of the heart may materially alter the quantity of blood that is sent to, or transmitted from the lungs, is too plain to require any formal proof; and where the quantity of blood in the lungs is affected, the quantity of air necessary to ventilate that blood must vary: in other words, dyspnœa must ensue.

The negation of a symptom is not without its value. The *absence* of dyspnœa may guide and decide our diagnosis. If our patient can go quickly upstairs, or up a hill, without feeling more out of breath than other and healthy people of his own age, we may comfort him with the assurance that there is not much amiss with his heart.

One very common effect of cardiac disease is an impeded and sluggish transmission of venous blood from the abdominal viscera. Hence congestions of various parts, and especially of the liver, which enlarges and grows tender; and the biliary secretion and functions are deranged. These symptoms are a fruitful source of mistake, leading the unwary practitioner into the belief that the whole of his patient's malady is hepatic; whom he comforts accordingly with the assurance, that 'it is all liver.' In the same way passive congestion of the kidneys and consequent albuminuria are apt to be produced.

The circulation through the *brain* is also liable to be much disturbed in heart diseases; and to this circumstance we must attribute the headaches and giddiness that often accompany them; the dread and causeless apprehension which such patients frequently exhibit; the cowardice and irritability which disease of the heart engenders in men who previously were intrepid, and of strong and firm nerves; also that propensity to dreaming, and especially to distressful and frightening dreams, so commonly observable in them; and the sudden startings from sleep in agitation and alarm. The relations that subsist between apoplexy and organic disease of the heart were fully discussed in a former lecture.

One of the most common indirect symptoms of disease of the heart is dropsy; yet, sometimes cardiac disease may continue long, and even prove fatal, without giving rise to any dropsy. It will produce that symptom, or not, according as it leads to general venous congestion, or not. Hence dropsy is more particularly connected with dilatation of the right cavities of the heart, and attenuation of their muscular walls. But these are points to which I must revert.

Lastly, the heart may become, under disease, the centre whence fatal mischief is shot, like the missile contents of a bomb-shell, into any or every part of the body. You will call to mind what was said in a former lecture about *embolism*, and its results.

Having thus run over, Gentlemen, the morbid changes to which the heart, as a muscular organ, is liable; the alterations of thickness in its walls, and of capacity in its chambers, and the derangements of the natural relations between the several chambers and their orifices; having considered, also, in a brief and cursory manner, the sounds which the heart gives out in its different movements during health, and the modifications to which these sounds are subject in disease ; and having, moreover, passed in review the general symptoms which frequently display themselves in connexion with cardiac disorder, we shall be the better prepared, I hope, to investigate, when we next meet, some of the specific diseases of that important organ.

LECTURE LIV.

Diseases affecting the muscular texture of the heart; and their treatment. Fatty degeneration. Rupture. Changes to which the valves of the heart are subject. Effects, and diagnosis, of those changes. Angina pectoris.

I KNOW not how I can so well put you in possession of what I know, or think, concerning particular structural diseases of the heart, as by taking them in succession, and offering a sort of running commentary upon them. The *mechanism* of those structural changes, and the altered sounds, and the other physical signs, arising out of them, I endeavoured to explain in the last lecture. Bear in mind that in this place I can do no more than draw broad outlines.

Simple hypertrophy of the left ventricle. This sometimes occurs when we can discover no mechanical obstacle to the passage of the blood out of the ventricle, which might account for it: none, I mean, by the closest scrutiny made even after death. Is it then possible that this change may be brought about by physical causes which are not permanent, and have no place within the body: such as undue action of the organ for a length of time, in consequence of habitual bodily exertion? A runner, for example, we may conceive to keep his heart beating with a degree of force and frequency beyond what is natural, for the greater part of the day; and that for many days, or weeks together. Again, can simple hypertrophy grow out of that excessive action of the heart which may be kept up, day after day, for a long period, by protracted mental emotion? It is difficult to answer these questions. But I presume that causes of this kind—that any cause, in short, which implied long-continued forcible increase in the function of the organ,—might suffice to generate hypertrophy. What is certain, however, is that such causes seldom do act with sufficient intensity and constancy to produce these effects: and simple hypertrophy of the left ventricle, with no physical obstruction to the flow of blood through the heart, either near, or distant in the minute arteries, and no impediment to the free play of the organ, is rare.

We ascertain its existence when it does exist, first, by the account which the patient gives of himself. He has a *sensation* of beating of his heart, which he ought not to have; he feels it and hears it beating as he lies awake in bed; or even at other times when he is at rest. The pulsations are regular. Hypertrophy has no tendency in itself to cause the pulse to intermit or to become irregular. The breath may be short, but there is no marked dyspnœa: the circulation of the blood through the lungs is not much affected by this alteration of the left ventricle; they are in fact protected by the mitral valve: there is seldom any dropsy: but the arterial circulation being forced, there is a tendency to active congestion in the capillary vessels. As there is no mechanical obstacle to bridle the excessive power of the muscle, the pulse is full and strong; the face is florid; the patient is liable to headache, to bleeding from the nose, to active hæmorrhage, and to local inflammation. If you listen to the heart in such a case, you find that the systolic sound is less loud and clear than is natural. It is not heard beyond the præcordial region, nor even perhaps over its whole extent: but there is no bellows sound. And if you place your hand upon the left breast, you feel that steady, swelling, incontrollable impulsion, which I spoke of in the last lecture, as the surest sign that I am acquainted with, of hypertrophy. Sometimes the præcordial region is manifestly bulging and prominent. If I do not mistake, I have occasionally seen that condition in young men who have unwisely given themselves in excess to ‘athletic sports.’

If I were to preach for an hour concerning the treatment of such cases, I could say no more than this: that they require perfect tranquillity of mind and body; undeviating moderation in diet and in bodily exercise, small topical bleedings perhaps, often repeated; with a close attention to the functions of the digestive organs. These are among the cases in which, if in any, we may expect to *cure* hypertrophy.

Hypertrophy provoked and sustained by inflammation of the membrane which lines the ventricle is not mere hypertrophy. That complex change is a most interesting one, and will demand our attention hereafter.

If simple hypertrophy of the left ventricle be rare, hypertrophy of the same chamber from a mechanical obstacle, or from some fixed hindrance to the easy working of the hydraulic machine, is exceedingly common. What difference, then, let us inquire, is made in the symptoms, in the treatment, and in the prospect of recovery, by the presence of a permanent physical impediment, out of which the hypertrophy has grown?

The mechanical impediment will frequently signify its existence, by causing some unnatural sound: a systolic bellows sound most commonly, which is audible over the sternum, along the course of the aorta. And the mechanical impediment will *tend* to cause faltering of the pulse: but often the hypertrophy corrects that tendency. So, on the other hand, the mechanical obstacle corrects the tendency of the hypertrophy to cause active capillary congestion: and when the obstacle is considerable, it will prevent the pulse from being so full and strong as in simple hypertrophy. If to the physical signs of hypertrophy of the left ventricle there be added a systolic bellows sound, heard loudest over the base of the heart, and a disproportionate smallness and feebleness of the pulse at the wrist, we may safely conclude that there is some impediment to the escape of the blood from the left ventricle into the aorta; and that this impediment has given occasion to the hypertrophy.

Now, in this case, the hypertrophy is really an endeavour towards health. The increased power of the ventricle compensates for the bar which is opposed to the current of the blood. The blood would not be able to go on without the hypertrophy. There would ensue a tendency to stagnation in the circulation, a faltering pulse, imperfect arterialization of the blood, blue cheeks and lips, dyspnœa, dropsy; but the augmentation of bulk and force in the impelling muscle obviates this: obviates it at least for a while: puts off the evil day to a distance. Since this is the case, and since we have no means of removing the mechanical impediment, we should be mad to desire the cure of the hypertrophy, which is to a certain degree a remedy for the impediment; nor indeed could we cure it if we would. But we have to endeavour to keep it within due bounds. If the beating be troublesome to the patient, we may alleviate that symptom, and check what there may be of superfluous energy in the contractions of the morbid chamber, by abstracting blood from the præcordia by leeches; and by soliciting the action of the kidneys, by means of cooling diuretics, among which small doses of digitalis may find an appropriate place. The labouring action of the heart is sometimes calmed by the application of a belladonna plaster or liniment. In this variety, also, of the disease, it is of primary importance that no undue efforts of the body be made, and that the patient be protected, as much as possible, against all causes of mental emotion;

that scrupulous temperance be enforced; and that all the functions of the body be carefully watched and regulated.

These are not cases in which we can look for recovery: but they are cases which bad management and imprudent habits may hurry on to a fatal termination; and which judicious treatment and a disciplined course of living may render tolerable, and carry forwards for a considerable period.

Under the same condition of mechanical impediment, we oftener have eccentric hypertrophy of the left ventricle: hypertrophy, *i. e.*, with dilatation. Of course the bulk of the whole heart is augmented by both of those conditions; and sometimes it becomes enormous, as big as that of a bullock. The symptoms will differ somewhat, according as the hypertrophy, or the dilatation preponderates, and therefore it will be as well to state here what are the symptoms of simply dilated ventricles. They are, a diminished *impulse* of the heart's action; and therewith a clearer sound than is natural. The first sound approximates to that of the heart's diastole; to the clacking second sound, and it is heard extensively. There is more or less tendency to fluttering palpitations and irregularities of the pulse, which is usually weak and small; to faintness and debility, and to coldness of the extremities: and when the *right* ventricle is dilated, there are some other symptoms which I shall notice presently.

Now, I say, there will be a mixture or modification of the symptoms, when the left ventricle is both dilated and hypertrophic. The dilatation will aid the mechanical impediment in giving a tendency to irregularity and intermission of the pulse; and the hypertrophy will tend to rectify that disposition. And we must *trim* our management of such cases accordingly. If the pulse flutter, we cautiously administer tonics, or stimulants: if it be steady, and the signs that belong to simple hypertrophy predominate, and are excessive and troublesome, we must starve the patient, take blood from his side, purge him and give him diuretics; but at all times keep him as *tranquil* as we can.

Simple hypertrophy of the right ventricle is not a common disease. When it occurs, it results from some actual or virtual impediment to the passage of the blood from the ventricle into the lungs. The most extreme instance of it that I ever saw, was in the heart of a medical friend's son, who died at the age of seventeen; having been for many years affected with the *morbus cæruleus* as it has been called, *i. e.*, an habitual blue state of the cheeks, lips, and tongue, finger-nails, and the skin generally; attended with shortness of breath, and augmented by every kind of exertion. It is seldom that persons thus affected live so long as this poor boy lived. The heart, as is usual under such circumstances, was malformed. The septum between the ventricles was imperfect at its upper part; and the aorta belonged as much to the one ventricle as to the other. The pulmonary artery would not admit a goose-quill; the walls of the right ventricle were as thick as those of the left.

Authors tell us that hypertrophy of the right ventricle of the heart is a cause of pulmonary apoplexy. I explained to you in a former lecture why I cannot believe in this doctrine. In the first place I say that the increased thickness and strength of the walls of that chamber supply a measure of the *difficulty*, and not of the *freedom* and *force*, with which the blood is conveyed to the lungs. In the second place pulmonary apoplexy does not result from rupture of vessels by the *vis à tergo*: and is quite a different lesion from *cerebral* apoplexy. It is simply an *accident* of pulmonary *hæmorrhage*. And lastly, I never met with pulmonary apoplexy coincident with mere hypertrophy of the right ventricle. The right ventricle lies *on this side* the lungs, in the order of the circulation; and accordingly, following the rule I mentioned in the last lecture, its morbid states are for the most part *effects*, and not *causes*, of *pulmonary* disease.

The commonest affection of the right ventricle is dilatation, with or without some increase of thickness, or even sometimes with attenuation, of its muscular parietes. This also is in general the consequence of *long standing* disease, situated beyond or in front of the right ventricle, and preventing the easy passage of the blood from it towards and into the lungs. The onward course of the blood may be hindered by mitral or even aortic obstruction, causing habitual congestion of the lungs: by pulmonary emphysema, and its attendant obliteration of minute blood-vessels: by whatever, in short, may impede the circulation through the lungs. Any of these conditions will tend to constantly overload, and so to stretch and dilate, the right ventricle: and its resistance to that tendency may at the same time lead to some thickening of its walls.

Dilatation of the right ventricle tends to beget a corresponding dilatation of the right auricle also, and of the jugular veins, which then stand out in relief from the sides of the neck, and exhibit an undulating sort of pulsation, produced by the regurgitation of a part of the blood, whenever the ventricle contracts. I have taken from the neck of a person dead of such disease, veins into which I could slip my forefinger. There is no surer sign of dilatation of the right cavities of the heart, and of a yawning, inadequate tricuspid valve, the result of that dilatation, than abiding regurgitant fulness and pulsation of the jugular veins. With all this there is a fluttering action of the heart, an irregular pulse, great distress and shortness of breathing, a dusky skin, and blueness of the countenance, which is bloated and anxious, and a tendency to delirium and drowsiness; while, sooner or later, the whole areolar tissue of the body becomes charged with accumulated serum. Some degree of this may now and then be noticed towards the fatal close of phthisis. Much oftener it accompanies the latter periods of extensive pulmonary emphysema.

Disease, such as I am now describing, in its advanced stages especially, is difficult to treat. If you stimulate, you run the risk of increasing the patient's distress; if you deplete, you incur the hazard of producing fatal syncope, of bringing the heart to a pause from which it is never able to recover. Here, again, you must try to keep the kidneys active; you must enjoin that, as far as may be possible, all causes of agitation or hurry, everything which has previously been found prejudicial to the patient, may be sedulously warded off. I have seen more benefit in these cases from steel, cautiously employed, than from any other drug. With the steel may be combined, with great advantage, a small quantity of digitalis. Without forcing the heart's action, this combination appears to have the effect of increasing the tone of its muscle; which it thus enables, *for a time*, to compete more successfully with the load it has to carry, and the impediment which it cannot overcome. We can do no more in such cases than palliate.

Hypertrophy—or dilatation—or dilatation with hypertrophy—may affect, in their various degrees and combinations, one chamber of the heart; or several at the same time; or all of them together. It would be vain to attempt to represent, in verbal description, these complicated changes. Enough, I trust, has been said, to enable you to unravel them when they come before you; and to ascertain with sufficient exactness, the general indications which they severally furnish, and the plan of treatment which they require.

You will often find the muscular substance of the heart pale, soft, and flabby; easily broken down, or penetrated, by pressure. This may occur with general debility and looseness of tissues; it sometimes accompanies a plentiful deposit of fat about the organ; and it is supposed to be sometimes also a consequence of inflammation affecting the muscle. Walls thus soft are likely to yield under pressure; but I know of no particular symptom by which we can detect with certainty the soft condition.

One mode in which such softness may originate deserves your especial attention. In an early lecture of this course, when speaking on the subject of general pathology, I described two distinct kinds of *atrophy*; the first consisting in mere diminution of bulk, without change of texture; the second involving alteration of texture, without any necessary reduction of bulk. These distinctions are well exemplified in the heart. There are two forms of cardiac atrophy: one well known, in which the organ simply wastes and dwindles in all its parts and dimensions, during the course of some wasting disorder; the other newer to pathology, as yet but imperfectly understood, in which the texture of the muscle degenerates also, and suffers a sort of conversion into fat. It is not that the heart is encumbered by an excessive accumulation of adipous matter upon its exterior, concealing, dipping down among, displacing, and thinning probably its muscular fibres. That morbid state of the organ is common enough. The change to which I now advert, and which has been described by Rokitanski, and illustrated by Mr. Paget, depends not upon any deposit among the fasciculi, but upon some alteration of their proper tissue. There may be no increase in the quantity of fat natural to the furrows and depressions on the outside of the heart; 'the whole of the organ' (I adopt Mr. Paget's description) 'may preserve its customary size, shape, and general external appearance, but it feels soft, doughy, inelastic, unresisting, and may be moulded and doubled up like a heart beginning to decompose after death. It seems never to have been in the state of *rigor mortis*.' 'In colour it has not on its surface, much less on its section, the full ruddy brown of healthy heart, a colour approaching that of the strong voluntary muscle,—but it is, for the most part, of a duller, dirtier, lighter brown, in some parts gradually blending with irregular marks or blotches of a paler fawn colour.'

Microscopic observation reveals the nature of these changes, and exhibits a number of minute oil-particles, scattered more or less thickly along the course of the muscular fibre. The muscle thus spoiled retains no longer its proper power, and the functions of the organ are defeated.

Of death occurring, apparently, through this fatty degeneration of the heart—a state which might easily escape the notice of any but a careful and practised examiner—Mr. Paget details three very interesting examples. They have recalled to my mind one or two instances that I have witnessed of sudden and fatal failure of the circulation, under very similar circumstances. They were perplexingly obscure at the time—but Mr. Paget's remarks elucidate what I now believe to have been their real pathology.

This insidious disorder has been studied more recently by Dr. Ormerod, the late Mr. Barlow, and others, and especially by Dr. Richard Quain, who has embodied the fruits of his own researches, and the principal facts collected on the subject by previous observers, in the 33rd volume of the 'Medico-Chirurgical Transactions.'

In accordance with the views which I formerly set before you, Dr. Quain distinguishes fatty growths upon the heart and between its fibres, from fatty degeneration of those fibres themselves. These unnatural conditions spring from different causes: the one being the result of an accumulation in the blood of the elements of fat; the other the result of decay and disintegration. The fatty growth may occur alone; the fatty degeneration may occur alone; but they often meet in the same heart.

In examining a heart thus diseased, the eye first notices the fainter tracing, or the utter absence, of those transverse marks which cross the fibres of all the voluntary muscles, and less distinctly those of the involuntary muscle, the heart. In an early stage of the disease, these cross lines are dimly seen, and the fibre is studded, here and there, with small dark points. When the disease is more decidedly expressed, the dots are more numerous, and the striæ disappear. These dots are little globules of oil. Lying within the sheath of the fibre, they make it soft and friable.

The parts of the heart which have undergone this change are altered in colour as well as in consistence. They are pale, like a faded leaf, or of a yellowish brown, or a muddy pink colour, and they commonly have a spotty or mottled appearance. The change of texture varies in degree, and in extent. It may render the muscle merely soft and flabby, or it may reduce it to a state in which it feels like a wet kid glove, and can be torn as readily as wet brown paper. Every chamber of the heart is liable to this kind of disease, but most of all the left ventricle, then the right ventricle, then the right auricle, and least of all the left auricle. Generally it is more evident in the columnæ carneæ, and near the endocardium, than elsewhere.

Fatty degeneration of the heart may proceed from a defect of healthy nutrition throughout the body, in consequence of some general disorder, or of natural decay in the decline of life. In such cases the same morbid change is commonly manifest in other parts also; in the arteries, in the liver, in the kidneys, in the cornea.

But fatty degeneration may be limited to the heart, and even to a small portion of the heart, and then it is owing to some local failure of nutrition; of which perhaps the most common cause is a diseased condition of the coronary arteries. You are probably aware that these two vessels have no large or free communication with each other: and it is a very instructive fact that when one of them alone is diseased, that part only of the heart frequently is found to be affected which receives its supply of blood through the unsound artery. Fatty degeneration of the heart is also met with after bygone inflammation, whether of the muscular tissue itself, or of its lining or its investing membrane. It is no uncommon sequel of hypertrophy. In every instance the change seems ultimately traceable to deficient nutrition. To the same principle may be referred that diminution of its firmness, and deterioration of its texture, which the heart is apt to sustain in the graver cases of typhus fever.

Under this dilapidating process the walls of the heart may become so soft and yielding as to bulge out into a pouch, or even so fragile as to crack; in which latter case the patient almost always dies suddenly, the motion of the organ being stopped and strangled by the effusion of blood into the pericardium. So that to die of a broken heart, is not a mere metaphor. A clergyman from the country, whom I previously knew, called at my house in the autumn, and waited some time in my absence; but went away at last without seeing me: and after consulting Dr. James Johnson, set out for his home, ten miles on the other side of Colchester. He had been unwell for some time; had suffered occasional attacks of dyspnœa; and was unusually nervous and irritable. He must have been conscious of some severe distress, for he was extremely anxious to get home, and bribed the post-boys to drive fast. As soon as he reached his own house, he ate some supper, and went to bed apparently comfortable. Half an hour afterwards one of his servants went to him, and found him asleep. At the expiration of another half-hour, he was again visited, and was then a corpse. Among other changes, the pericardium was full of blood, which had escaped from the heart through a rent in the left ventricle, large enough to admit one's finger. That part of the ventricle which surrounded the laceration, was unnaturally thin, to the extent of a crown-piece. There are several specimens of rupture of the left ventricle in the Museum at St. Bartholomew's Hospital. George II. died of rupture of the heart. It is curious enough that a Duchess of Brunswick, of the same family with George II., died of the same disease. In her case an *ulcer* penetrated the parietes of the right ventricle, which in other respects was healthy. The death of a great and good man of our profession, and of our time, Dr. Abercrombie, of Edinburgh, was caused by rupture of the heart. In most instances, the rupture has taken place in the left ventricle.

Although this fatal accident may occasionally, as in the example just referred to, be produced by the progress of a perforating ulcer, its commonest cause is that degeneration of structure which we are now considering. There is reason, indeed, to believe that it is very rarely owing to any other cause. Among 83 fatal cases of fatty degeneration of the heart, collected by Dr. Quain, there were 28, or about one in every three, in which laceration of its muscular tissue was discovered after death. In eighteen of these, the outer wall of the left ventricle was ruptured; in three, that of the right ventricle; in one, that of the right auricle; in one, the septum between the ventricles. The rent in a few others of these cases had not gone through the muscle.

The left ventricle is also liable, almost exclusively I believe, to those partial distensions of its walls into lateral cells or pouches, which are spoken of as aneurisms of the heart.

Is there any sign, or any group or succession of symptoms, by the notice of which we can *assure* ourselves that a living patient's heart is affected with fatty degeneration? I know of no such signs. When that change depends upon a local cause, and is limited to the heart, it is always associated with other changes, of an earlier date; and its proper symptoms, if it have any, are then mixed up and confounded with those of the earlier structural disease. When it proceeds from some general cause, we may sometimes infer its presence with more or less of probability.

There are no auscultatory signs peculiar to the movements of a fatty heart. The pulse has been observed to be weak, sometimes irregular, sometimes remarkably slow: but these qualities are frequent accompaniments of other morbid conditions. The same is to be said of shortness of breath; of attacks of syncope, which are common; of coma, and of præcordial pain, which have been less often noticed in these cases. In seeking to form a diagnosis, the time of life must be taken into account. The fatty degeneration, when it is the result of general decay, is prone to begin about what has been called the climacteric period—the sixty-third year. In more than one half of Dr. Quain's cases, the age of the patients was above sixty. In twenty-one instances out of sixty-eight, death took place in the way of *syncope*. Of the whole number (eighty-three), sixty-eight died *suddenly*. Death is apt to occur upon some shock, or unusual effort:—a hasty ascent, straining at the water-closet, the act of vomiting. 'The principal character (writes Mr. Paget) which all these cases seem to present is, that they who labour under this disease are fit enough for all the ordinary events of calm and quiet life, but are wholly unable to resist the storm of a sickness, an accident, or an operation.' I believe this to be generally true. Dr. Begbie has, however, recorded two interesting instances of this malady, by which the lives of two eminent men were cut short (Dr. Chalmers and Dr. Abercrombie), both of whom 'were actively engaged up to the hour of death in the labours of two most arduous and onerous professions.'

I say that the evidence of fatty degeneration of the heart can never amount to more than presumptive evidence. But if there be tokens of feebleness of the heart, yet no tokens of any valvular or mechanical flaw—if the patient have attained his 'grand climacteric'—if he have shown of late a tendency to grow somewhat fatter, and somewhat paler also, and sicklier in complexion—and if there be withal a marked arcus senilis—then you may reasonably conjecture that (to use Dr. Begbie's words) 'the great organ of life is yielding, through the progress of time, to those organic changes which mark the decay of its structure, and foretell the not distant cessation of its long-continued functions.'

The last circumstance that I have mentioned, the presence of the arcus senilis, furnishes a strong presumption that the fatty change may be in progress elsewhere also in the body. But you must not give this symptom more weight than it deserves. The cornea is sometimes alone in suffering the change. I am acquainted with a gentleman under forty years of age, who, enjoying excellent health, presents a well-pronounced arcus in both his eyes, especially at the summit and at the base of the circle, and in whom that appearance has remained unaltered, certainly since he was twenty-four years old, and perhaps from an earlier date. Mr. Canton has observed a marked degree of arcus senilis in each of three youths of the same family, aged respectively eleven years, fourteen, and twenty. Both their father, aged fifty-three, and their mother, aged forty-nine, had the arcus.

When rupture ends the scene, usually there is sharp pain at the time of its occurrence, and the final struggle is soon over. It would appear, however, from two cases related by Dr. Latham, that laceration of the *septum*, though marked by severe and abiding pain, is not necessarily fatal within so brief a period. One of these patients was kept alive by stimulants for three days after the presumed instant of the rupture. The other endured eighteen hours of mortal agony; and, judging from the symptoms, it seems not improbable that the rupture took place nearly three days before he died.

For that fatty ruin of which I have been speaking there can be no repair. Yet much may be done, even by drugs, for our patient's comfort; and more, by counsel and warning, for his safety. That portion of the muscle which still preserves its primitive structure and qualities may be sustained in its imperfect functions by medicines calculated to renovate the blood, and so to strengthen the muscular tissues. Great improvement does often become manifest under the cautious employment of preparations of iron. When syncope is threatened, diffusible stimuli may be freely used. Above all, you must inculcate temperate habits, and a life of constant quiet. The patient must never be tempted nor surprised into any act which implies unusual effort. He must, for instance, lose a journey rather than hurry on foot to a railway train for which he is late. He must be content to get wet through, rather than run for shelter in a sudden shower. He must never lift a burden, nor climb stairs hastily, nor strain to relieve costive bowels. Neither will it be safe for him, even on what might seem legitimate occasions, to yield to feelings of anger, or any kind of excitement. These cautions are indeed more or less applicable to all cardiac disorders; but they are especially requisite whenever there is reason to suspect that the texture of the heart is infirm, and incapable of bearing the stretching pressure of a hurried, or of an impeded, stream of blood.

Many of the morbid conditions of the muscular substance of the heart spring from pre-existing morbid conditions of the membrane which lines, or of the membrane which invests, the heart. It is necessary therefore, in the next place, to inquire into the nature and history of *these* morbid changes: and I will first request your attention to the diseases of the lining membrane. The investing membrane is familiar to you as the *pericardium*. Of late years, since the diseased states of the internal membrane have been more studied and understood than they formerly were, *it* has been called the *endocardium*: a convenient enough name, which may occasionally spare us circumlocution. Now this endocardium is liable, among other changes, to inflammation, under which it becomes whitish, opaque, and covered sometimes with a thin layer of coagulable lymph. In a well written and well reasoned essay, which you may see in the 'Lancet' for 1846, Dr. Munk has shown how hypertrophy of a chamber of the heart may be engendered by chronic or subacute inflammation of its lining membrane: and, what is most interesting, how such hypertrophy may be cured, when the disease is detected and treated in time, by means which arrest and remove the endocarditis. General diffused inflammation, or other change, of the membrane is, however, comparatively rare. Certain parts of it are much more obnoxious to disease than others: those parts which enter into the fabric of the valves and orifices of the organ. The membrane is here in close contact with a dense fibrous tissue; and participates in the changes to which that tissue is subject. And it is an important fact, that the membrane, valves, and orifices of the left side of the heart, as well as its muscular substance, are much more frequently affected with disease than those of the right side. I have adverted to this fact before. What is the prevailing cause of it I cannot tell; but it seems to be a portion of a more general fact; namely, that the arteries are more liable to chronic morbid changes than the veins. Some explain the difference by alleging that the left side of the heart has the heavier task to accomplish. But nature seldom executes her purposes so clumsily, as not to adjust the strength of her machinery to the labour it is destined to perform. Others remark that fibrous tissue is more abundant, and therefore the changes proper to that tissue are more numerous and extensive, on the left side. And this may be the true explanation. Others, again, have conjectured that the arterial blood is more irritating than the venous. But there is no evidence of this: and it is better to content ourselves with noticing the fact, without attempting to account for it by mere gratuitous hypothesis.

You are not, however, to suppose that the right heart is exempt from valvular disease. When there is much change in the left, we often find a less degree of the same kind of change in the right. The valves of the pulmonary artery are, perhaps, the least frequently of all the valves found otherwise than healthy.

Many of the alterations that take place in the internal lining of the heart result, apparently, from inflammation, which causes a deposit of lymph upon or beneath the serous membrane. The valves are apt to lose their thinness, their transparency, and their pliancy. They become thick, stiff, puckered, curled up, or glued to each other, or to the opposite walls of the channel. On the other hand, quite independently of inflammation, they may become morbidly thin, riddled with holes, and even rent asunder. What have been badly called vegetations or excrescences may also project from them, very much resembling warts. Or they may be converted wholly or partly into bone.

Alterations of some kind or another are very frequent in the *semilunar valves* of the *aorta*. When they are of such a nature as to diminish the orifice during the systole, they commonly occasion a systolic bellows sound. When the diseased valves offer no obstruction to the exit of blood from the ventricle, but do not close again immediately afterwards, so as effectually to prevent the reflux of that fluid from the aorta, they commonly give rise to a diastolic bellows sound. When both these defects of function occur, there is often a *double* bellows sound; a sawing alternate noise; one murmur during the systole, another, distinguishable in tone and quality, as well as in time, during the diastole. These sounds are conveyed along the tube in which they are formed, and are therefore most audible in the track of the aorta, as it leaves the heart. If the sound be diastolic, it will *take the place* of the smart clack of the second sound of the heart, or perhaps prolong it. Sometimes the new sound is very loud and curious. I had a patient in the hospital last year, in whom this diastolic sound was, in character and intensity, like the cooing of a pigeon. The patient could plainly hear it: nay, it could be heard by a person standing near him, but not touching his body, even with a stethoscope. In that instance we found one of the aortic valves irregularly thickened, with its free edge loose and flapping, and unable to fulfil its function of closing the aperture. During the diastole it was retroverted, and vibrated in the regurgitating stream of blood; and thus, no doubt, the musical note, heard alternately with the first sound, was produced. In March, 1837, I heard in a man (Henry Milton) who was under Dr. Latham's care in St. Bartholomew's Hospital, and who had acute rheumatism, a very shrill diastolic sound, like the repeated whining of an imprisoned puppy-dog wishing to be released. This remarkable sound was audible, by means of the stethoscope, even in the radial artery. The patient died at last in St. George's Hospital, and his case is mentioned in Dr. Hope's book on the Heart. One of the aortic valves was torn downwards to some distance from its edge, and formed a flap which was perforated by a round hole.

I need not again point out to you the manner in which such disease of these semilunar valves tends to produce hypertrophy and dilatation of the left ventricle.

The *mitral* valve is often thickened; and it is particularly subject, more so even than the aortic valves, to ossification. And the effects of the ossification are to prevent its closing the auricular orifice during the systole; and to prevent its lying flat against the walls of the ventricle, and allowing a free passage of the blood out of the left auricle, during the diastole. The orifice is often converted into an unvarying oval slit, having puckered edges, and resembling a button-hole; or the valve projects, like a thimble of bone, into the left ventricle. And it is remarkable how small the chink, which is thus permanent, may be, and yet life go on. The heart having been taken out of the body, and the auricle filled with water, I have seen the water pass into the ventricle, by its gravity, *stillatim*; drop by drop.

Let me just remind you, that the direct and necessary consequence of constriction of the mitral orifice, is an accumulation of blood *behind* it; i.e. in the left auricle, in the pulmonary veins, in the lungs. Hence so much mechanical congestion, that the blood bursts at length through the bronchial membrane; hæmorrhage, slow or copious, ensues from the air-passages; and *pulmonary apoplexy* is formed.

In extreme cases, where the mischief is chiefly confined to the mitral valve, the blood necessarily reaches the ventricle in a penurious manner; that chamber contracts unsteadily and irregularly; and its cavity sometimes diminishes. This I think I have seen. But far more commonly there is disease of the aortic valves also: and the condition of the left ventricle is that of hypertrophy with dilatation.

When there is a permanent chink in place of the limber valve, there may be a double bruit. The first heard during the systole, and produced by the regurgitation of blood from the ventricle into the auricle, through the rigid slit. This is not uncommon. The second accompanying the diastole, and resulting from the mechanical impediment to the free passage of the blood from the auricle into the ventricle. This is less common. The constriction must be considerable for the diastolic murmur to occur at all: and when it does occur, it is faint; from the comparative feebleness of the auricular contractions.

The form and the consistence of the altered valves being the same, no difference whatever in the sounds, or in the general symptoms, will arise from the particular nature of the changes. It will, I mean, make no difference whether the obstacle to the flowing blood, or the imperfect closure of the orifice, depends upon mere thickening of the valve by cartilaginous deposits, or upon ossification, or upon wart-like projections from its surface. These last may be found upon any of the valves, but like other morbid states, they are less frequent on the right than on the left side of the heart; and they are most common of all on the aortic valves. Ossification—the accumulation of the phosphate of lime—is almost confined, I believe, to the left side. I never saw the tricuspid valve, or the semilunar valves of the pulmonary artery, converted into bone.

The warts, or wart-like eminences, which are so often found upon the valves of the heart, are strangely interesting things. Sometimes they are separate, and in rows like beads. Sometimes several appear to spring from a common base, which spreads out so as to present a cauliflower appearance. And occasionally they hang in long strings from the valve into the adjoining chamber of the heart. In a patient of Dr. Hawkins', I saw a cylindrical chain of this kind which measured an inch in length. The valves presented slit-like perforations; and from the edge of one of these slits in the mitral valve, this long fibrinous string dangled into the ventricle. The whole of the valves of the aorta were covered, on their ventricular surface, with similar but shorter prominences.

They vary much, these little substances, in consistence. Sometimes they are soft, easily crushed, and capable of being readily detached from the smooth surface of the valve. Others are more firm, and yet separable from the valve without injury to it. Others, again, are so adherent, so rooted apparently into the valves, that they can be removed only by tearing or cutting them off. They are found sometimes on the free edge of the valves; sometimes on their surface, or even on the inner membrane of one of the chambers, especially of the left auricle.

Much difference of opinion has prevailed respecting the nature and origin of these singular appearances. It was a common notion among the French, at one time, that they were really, what they so much resemble, venereal warts. What seems to be certain is, that they are somehow connected with *inflammation* of the internal lining of the heart; and of that which covers the valves in particular. But, then, are they lymph *poured out from* the inflamed membrane? or are they fibrin deposited from the blood *upon* an inflamed membrane? The last, I do not doubt, is the true explanation of their origin: and if so, the terms vegetations and excrescences, which imply a sort of growth from the subjacent membrane, should be abandoned. You know, that when a vein becomes inflamed, the blood in contact with its lining membrane has a strong tendency to coagulate upon it, and to adhere to it. The little fibrinous projections found on the valves are often attached to the rough edges of slits in the valve, or to the broken end of a tendinous cord. When recently formed, these red granules are very soft and frangible. The most interesting fact that I am acquainted with, in evidence of the mode in which they may arise, is one that accidentally came to light in one of Dr. Hope's experiments upon an ass, at which I was present. The aortic valves had been held back by a wire passed into the vessel, with the view of ascertaining the physical cause of the second sound of the heart. The animal had previously been rendered insensible by a narcotic poison; and the circulation was kept up—languidly however towards the last—by artificial respiration. Upon the final cessation of the heart's motions, the organ was removed from the body and examined: and the valve that had been mechanically irritated by the wire was found studded with these little wart-like appearances, which were so soft as to admit of being readily brushed off from the subjacent membrane. Here the deposit took place after the death of the animal, and while some of the functions of organic life alone were kept up by the artificial breathing.

Mr. Simon was, I believe, the first, many years ago, to call in question the then existing belief that the so-called vegetations were really growths or excrescences from an inflammatory exudation. He thought them to be deposits from blood overcharged with fibrin, 'the valves encrusting themselves with the fibrin just as a stick in certain streams coats itself with a calcareous envelope.' This opinion he supported by experiments. Thus he carried a single thread, by means of a very fine needle, transversely through arteries and through veins of living animals; and after the lapse of from twelve to twenty-four hours he always found that 'the arterial blood readily deposited its fibrin on the thread, the venous with the utmost reluctance.' He likened the process to that of *whipping* blood, to get its fibrin out of it. Admitting the general soundness of Mr. Simon's views, Dr. George Johnson maintains, that there is another factor requisite besides the abundance of fibrin in the blood, viz., the change produced in the affected surface by its inflammation. Any foreign and therefore dead substance placed in the stream of the circulating arterial blood—a needle for example, or a coil of wire—soon becomes incrusted with fibrin, as Mr. Lister also has shown: and the surface of the inflamed valve is approximated to a foreign substance—is roughened, and in some measure devitalized—and so fitted for the precipitation of the fibrin upon it. The mere whipping of the loaded or unhealthy blood is not enough. Were it so, the interstices of the *chordœ tendiniœ* would be plentifully clogged with the fibrin. Remaining uninflamed, they rarely are marked by any deposit.

Some very interesting circumstances still remain to be mentioned, connected with the formation of these wart-like bodies. I shall not, however, enter upon them in the present lecture; but when I speak, at our next meeting, of rheumatic inflammation of the heart and its membranes.

Any or all of the lesions that I have been describing may and must lead, at length, according to their places and magnitude, to some of those changes in the actual and relative dimensions of the heart that were considered in the last lecture. They obstruct the stream of blood when moving in its natural course, and when its passage ought to be free; or they allow of its *refluent* course, when that ought to be effectually opposed: and the necessary results, in either case, are dilatation of one or more of the chambers of the heart, with thickening, or with attenuation, as the case may be, of its walls. I have already spoken of the symptoms, physical and general, to which these secondary changes give rise; and of the treatment which they admit and require.

There being valvular disease, and that valvular disease giving rise to a bellows-sound, can we distinguish the particular valve affected? Generally, we can. Our skill in diagnosis outruns here, as indeed it too often does, our skill to cure. A few simple rules and considerations enable us, in most cases, to satisfy our natural curiosity to penetrate the exact condition even of changes that are incapable of repair.

The cardiac valves lie all together, within a narrow compass, beneath an area on the surface of the chest which the end of an ordinary stethoscope would more than cover. Our judgment respecting the place of origin of a given murmur is guided by the *time* of its occurrence with reference to the movements of the heart, and by *the direction in which it is transmitted*; somewhat also by the qualities of the arterial pulse.

When a bellows-murmur accompanies the heart's systole, it must be caused by a current passing *out* of a ventricle. But serious disease of the valves, sufficient to produce a murmur, on the right side of the heart, is rare. In ninety-nine cases out of a hundred, valvular murmurs belong to the left side; so that practically the question lies, almost always, between two orifices, the aortic and the mitral, the outlet and the inlet of the left ventricle. The natural outlet is obstructed, or the natural inlet has become an outlet also.

Now if a systolic valvular murmur be heard plainest at the base of the heart—in the middle of the sternum say, on a level with the third intercostal space—and along the track of the ascending aorta, towards the right collar-bone, and even in the carotids; and if it be inaudible or less audible at or about the apex, the mischief is seated in the *semilunar valves of the aorta*: some obstacle at the aortic orifice causes a ripple in the onward stream of the blood. This murmur being carried upwards by the blood stream, will generally be audible in the back also, opposite the second, third, or fourth spinal vertebra. The pulse, as a rule, remains steady and regular.

On the other hand, if the pulse be weak and irregular, and if the systolic murmur be best heard down at, or near, the apex of the organ, on its left, it is owing to regurgitation through a diseased *mitral* orifice. This murmur also, being conducted backwards with the refluent blood into the auricle, is more or less distinctly to be heard behind, near the lower angle of the left scapula, and at the same level in the axillary region.

When, what scarcely ever is the case, the murmur does result from injury of the semilunar valves of the pulmonary artery, it is heard plainest at the sternal end of the cartilage of the third left rib, and up towards the left clavicle. So again, a systolic murmur produced by change at the tricuspid orifice would be most perceptible towards the apex, on its right, and near the sternum. For obvious reasons, the arterial pulse is but little influenced by disease affecting the orifices of the right heart.

Again, if the murmur accompanies the heart's diastole, it is caused by the entrance of blood *into* a ventricle: and for reasons similar to those assigned before, the fault lies, most probably, in the *left* ventricle. The murmur may be due to the direct but impeded passage of the blood from the left auricle through a narrowed mitral orifice. This is not very common. Or the diastolic murmur may depend upon regurgitation, the consequence of disease at the aortic orifice, the natural outlet having been made an inlet also: and this is abundantly common. We attend, as before, to the spot and the track in which the murmur is the loudest; at the lower part of the sternum, and along the course of the aorta. We listen also for the smart clack of the natural second sound, and if it be not audible, or be muffled and indistinct, we have in that circumstance corroborative evidence of an inadequate aortic flood-gate. Moreover, we are again assisted by the pulse. The pulse of aortic regurgitation is, sometimes at least, very striking and peculiar: sudden, like the blow of a hammer, without any prolonged swell of the artery. This pulse always reminds me of the well-known chemical toy, formed by including a small quantity of liquid in a glass tube, exhausted of air, and hermetically sealed. On reversing the tube, the liquid falls from one end of it to the other with a hard short knock, as if it were a mass of lead. The sensation given to the finger by the pulse, when there is much regurgitation through the aortic valves, is very similar to this. It is as if successive balls of blood were suddenly shot along under the finger. Dr. Hope calls this pulse a *jerking* pulse. The arteries are momentarily filled, but the pulse collapses. And this abrupt pulse makes itself *visible* in the arteries; the wave of blood lifts, and moves, and sometimes even contorts the vessel. When this kind of pulse occurs with a diastolic bellows-sound heard along the track of the aorta, and the short clack of the second sound is absent or muffled, you may be quite sure that the aortic orifice is patulous during the diastole. The reflux of the blood, when the patency is great, is strong enough sometimes to produce a palpable shock or jog, called the diastolic impulse. And this refilling of the ventricle from the artery may even provoke it to a supernumerary contraction. If you do not check the cardiac movements by the apex beat, or by the pulsations of the carotids, you may even mistake the diastolic impulse, and the murmur attending it, for a systolic impulse and murmur.

In a patient by whom I was lately consulted, the hard, sudden, hammering pulse led me to conclude that the blood regurgitated from his aorta: and accordingly, upon applying my ear to his chest, I discovered a loud murmur, coincident with the diastole, and most distinct in the direction of the right clavicle. The shock of this man's artery was plainly to be felt through his clothes, by one's hand laid lightly upon the bend of his arm. His wife told me that, for five years past, this jarring blow had made it uncomfortable for her to take his arm when they were walking together. The same kind of jerking impulse was strikingly perceptible in the femoral arteries, and in the carotids.

Of murmurs from obstruction at the mitral orifice I have still something to say. The close of the left auricular contraction may make itself heard when the blood which it is delivering into the ventricle is obstructed: and we thus may get a murmur which precedes for a little, and runs up to, the *first sound* of the heart, where it stops. This has been well called, by Dr. Hayden, a *presystolic* murmur, or in Dr. Gairdner's language, who has admirably explained these cardiac murmurs, an *auricular systolic* murmur. When, with a flickering weak pulse, you hear this presystolic murmur at or about the apex of the heart, you may be sure that mitral obstruction exists. But the absence of the presystolic murmur affords no warrant for concluding against the presence of mitral obstruction. It is not audible in all such cases; nor always persistently audible in the cases in which it is heard; nor detectable sometimes, when it really exists, without careful auscultation.

When there is *also* a systolic blowing sound, the presystolic murmur may be *continuous* with it; but even then the distinction between the two murmurs is generally made easy by a sudden and marked difference in their tone or quality. ‘The first sound may commonly be detected in the middle of the continuous murmur, splitting it, as it were, into two.’

Regurgitant murmurs have sometimes a loud humming and even a musical character; and are often accompanied by a peculiar thrill, to be felt by one’s hand; meanwhile the action of the heart is apt to be tumultuous, as well as irregular.

As there may be a presystolic murmur, so there may be murmurs which *succeed,* and as it were shade off from, the natural sounds of the heart. Of these I will only say, first, that if a murmur coincides with the first sound, and is prolonged beyond it, it is a ventricular systolic murmur; and, secondly, that if it so succeeds the second sound, it is ventricular diastolic.

Of regurgitant sounds belonging to the right side of the heart I can tell you nothing. I never heard one, that I know of, from the pulmonic valves. Through the tricuspid orifice the blood is sometimes refluent; causing, as I stated just now, turgescence and visible pulsation of the jugular veins. The structure of the valve permits this ebbing movement of the blood under circumstances which might otherwise be perilous. The tricuspid has accordingly been called the *safety* valve of the heart. But the reflux seldom announces itself by a bellows-sound.

Cardiac valvular murmurs may occur in various combinations. Thus there may be a double alternating aortic, or a double mitral murmur; but such combinations will prove useful exercises for your own future study.

All that I have now been saying is easy to be comprehended with a little steady attention; especially if we dismiss the complication of the valvular sounds of the *right* heart, which constitute the luxuries rather than the necessaries of diagnostic inquiry.

Dr. Gairdner has elucidated the subject by appropriate diagrams, such as I show you, and such as you may see in his volume on ‘Clinical Medicine.’ He, and others before him, have attempted the same thing by inventing syllables and combinations of letters, which when whispered resemble in some of their audible qualities the inarticulate speech of the suffering organ. But all these helps—difficult to describe in writing or discourse—may best be learned in your clinical lessons.

We cannot always thus rigidly connect morbid changes with definite signs. Disorders arise of which the symptoms are more cognizable and constant than the nature or exact seat. We assign a name to the peculiar assemblage of symptoms, and make it thenceforth a distinct object of our study; tracing the symptoms as well as we can up to their organic causes and conditions. Now the complaint called *angina pectoris* is one of this kind. It is, moreover, a very remarkable and a fearfully interesting disorder; and I shall devote the remainder of the present hour to its consideration.

This disease was first accurately described, in this country at least, by the celebrated Dr. Heberden, the author of the 'Commentaries.' It had been adverted to by many writers before, but obscurely: and Dr. Heberden's observations were quite original. The description that he has given of the complaint, in the second volume of the Transactions of the College of Physicians, is very accurate and striking. He calls it a *disorder of the breast*; and observes that 'the seat of it, and the sense of strangling and anxiety with which it is attended, may make it not improperly be called *angina pectoris.*

'Those who are afflicted with it are seized whilst they are walking, and more particularly when they walk soon after eating, with a painful and most disagreeable sensation in the breast, which seems as if it would take their life away if it were to increase or to continue. The moment they stand still all this uneasiness vanishes. In all other respects the patients are, at the beginning of this disorder, perfectly well; and in particular have no shortness of breath, from which it is totally different.' Such is the brief description of the malady given by Dr. Heberden. You will observe, that the distress occurs in paroxysms; and the patient, at first, has intervals of apparent health: and even when the disease is more advanced, he has periods of comparative ease between fits of suffering. The paroxysms are especially liable to come on when the patient is walking, and, above all, when he is ascending—going up a hill. He is then seized, all at once, with a very painful sensation, which seems to be, in many cases, indescribable, but which is always referred to the heart or its neighbourhood. Sometimes the sensation is spoken of as being a spasm, as giving the sufferer a notion of constriction. I have been told by one who laboured under this disorder, that he felt, during the paroxysm, as if the sides of his chest were held together by a transverse bar of iron. The impression is constant that to continue the exertion which has produced the attack—to stir another step—

would be fatal. Yet the patient is not necessarily out of breath. It is not dyspnœa that oppresses him; for he can, and generally does, breathe freely and easily. He lays hold of any neighbouring object for support. His face is pale and haggard; and you would suppose, from his appearance, that he was actually at the point of death. But in the early stages of the disease, the pang soon subsides, the distress is over, and the patient is entirely himself again. It is a singular fact, which I cannot at all explain, but of which I have been assured by several persons affected with this disorder, that when the pain and inability to stir a step further have come on after a short walk, and have subsided upon the patient's stopping, he has often been able to resume his walk, and to pursue it for a long while, without any repetition of the distress.

After the lapse of some time, perhaps of some months, the anguish does not so instantaneously cease upon standing still; nor does it always require some bodily exertion to bring it on. It will occur when the patient is quiet, even in bed. He feels as if the action of the heart were arrested: and he is obliged to rise up, every night it may be, for many weeks together. In exquisite cases it may be brought on by causes of any kind that slightly accelerate the circulation: coughing, straining at stool, mental emotion.

The pain, which is at first referred to the left mammary region, shoots backwards often, towards the spine, or across the chest below the clavicles. Frequently it extends, accompanied by a sort of numbness, to the left shoulder, and down the left arm; stopping short, in a curious manner, and from some unexplained cause, either just about the insertion of the deltoid muscle, or at the elbow, or at the wrist. Sometimes, however, it runs down to the very extremities of the fingers; particularly of the last two fingers, following mainly the course of the ulnar nerve. Occasionally similar pains affect the right side and arm; and now and then, all the four extremities at once. There is, (I say) no dyspnœa in the genuine form of the disease; although you will find it stated by some modern writers, of good repute, that the paroxysm is accompanied with difficulty of breathing. In the instances that I have seen, the patient was generally able slowly and fully to inspire and expire, even when the fit was on him. The truth I believe to be, that other affections, more akin to asthma, have been confounded with angina pectoris; and this confusion has led to the belief, that it is not altogether so dangerous a complaint as used to be thought: but in its genuine shape it is undoubtedly a very fatal disorder. Sir John Forbes, by a diligent search among authors, has collected some statistical facts respecting it, which are worth remembering. Thus, out of eighty-eight cases, eight only, or one in eleven, occurred in women. The ages in eighty-four of these eighty-eight cases are recorded; and of the eighty-four, seventy-two were above fifty years; and twelve, or one-seventh of the whole, under fifty years. It is a disease, therefore, in the main, of advanced life: and this alone would afford a strong presumption of its dependence upon some organic change. Again, the event of the disease was recorded with respect to sixty-four of the patients. Of these forty-nine died, almost all of them suddenly; while fifteen recovered or were relieved. And among the forty-nine fatal cases, there were only two of women.

As a rule, angina pectoris has no distinctive pulse. Death occurring apparently in the way of syncope, the radial pulse becomes very feeble, or vanishes, just as life is about to end.

That the seat of the disorder is the heart, or the aorta, and that it consists in some structural change, can scarcely be doubted. Yet some pathologists are disposed to consider it a merely neuralgic affection, 'commencing, for the most part, in the pneumogastric nerve, and spreading in different directions as other nerves become involved.' But this doctrine is scarcely consistent, in my judgment, with the facts—first, that the paroxysm is excited by such causes as are 'especially calculated to disturb the natural action of the heart, bodily exertion, and mental emotion;' and, secondly, that the disease is so very frequently and so suddenly *fatal*. This is not at all the character of mere neuralgic diseases in general. And when we add to these facts the further fact, viz. that, in a vast majority of instances, organic disease of the heart, or of the great blood-vessels, has been discovered after death, I think we shall be obliged to admit, that the symptoms are often (I might say almost always) dependent upon cardiac disease. Complicity of sensory nerve disorder doubtless there is, as there must be in all diseases attended with pain. One theory explains the 'breast pang,' by supposing that the blood, whenever its movement is accelerated by exercise or otherwise, arrives in the heart faster than it can be transmitted onwards; and accumulating in its cavities, painfully distends them. I confess that this commends itself to my mind as being a very reasonable theory. The great Dr. Edward Jenner took a most ingenious view of the matter, which was made public and further enforced by Dr. Parry. He had found, in examining the bodies of some who had died of well-marked angina pectoris, that the coronary arteries of the heart were ossified; converted into bony canals, and constricted in their calibre. He thence concluded that the paroxysms result from the circumstance, that when some increase of the muscular contraction of the heart happens to be called for, the increased supply of blood, rendered necessary by the additional exertion, is not capable of being furnished by the diseased nutrient arteries of the organ; that the heart comes to a stand, because its muscular tissue is not duly injected with arterial blood. The patient is on the very brink of fainting: nay, does at length faint irrecoverably. He accordingly called the disease '*syncope* anginosa.' And this simple and beautiful theory was for some time admitted as the true one. However, later investigations have abundantly shown that angina pectoris may occur in a decided form, without there being any ossification or other disease of the arteries; and, on the other hand, that the coronary arteries may be ossified, and yet no angina pectoris be the result. Moreover, Dr. Jenner's theory does not account satisfactorily for the *pain*.

I may here again avail myself of the researches of Sir John Forbes, and give you a numerical account of the organic changes in the heart that have been found associated with this disease. The total number of instances collected by him, in which the body was examined after death, was forty-five. Of this number there was disease found in the liver only, in two instances: organic disease of the heart, or great vessels, in forty-three. Sir John Forbes, indeed, makes the last number thirty-nine, instead of forty-three, excluding four cases in which nothing morbid was found in or about the heart, except an excessive coating of fat. This Dr. Fothergill considered the essence of the disease and certainly a heart cannot be said to be in a healthy condition which is thus loaded with adipous matter. The fat is generally deposited at the expense of the muscular substance, which is apt in such cases to be thin, pale, and soft; atrophied, in short. Taking, however, the table as it is given by Sir John Forbes, the thirty-nine cases, in which there was no disease except in the heart and great vessels, were thus distributed:—In ten of the cases there was organic disease in the heart alone: in three, organic disease of the aorta alone. In one instance only was the disease confined to the coronary arteries; but there was ossification, or cartilaginous thickening, of the coronary arteries, combined with other disease, in sixteen instances.

Again, there was ossification, or other disease of the *valves* of the heart, in sixteen cases also. There was disease of the aorta (ossification, or dilatation, or both), in twenty-four cases; and in twelve cases there was preternatural *softness* of the heart.

Now I strongly suspect that this last condition, preternatural softness—in other words, fatty degeneration—will ultimately prove to be the main physical condition of angina pectoris. To express what I mean somewhat differently :—that group of symptoms to which we give the name of angina pectoris, is (as I conjecture) an authentic exponent of that physical state of the heart to which we give the name of fatty degeneration. When the examples collected by Sir John Forbes were recorded, this morbid change was unrecognised by medical science, and the diminished consistence which it implies would easily escape notice. Observe that the two things do often assuredly go together. Several of Dr. Quain's instances of fatty disease were also instances of true angina. Disease of the coronary arteries is perhaps the most frequent source of partial fatty degeneration. Disease of the coronary arteries is perhaps the most frequent accompaniment of angina pectoris. The substantial change, and the nominal disorder, both belong to the same advanced period of life; both are prone to end in sudden death, and in sudden death of the same kind—death by syncope. As exacter observations multiply, I expect that angina pectoris will be acknowledged as the surest, as the only sure, indication of a fatty heart.

Pain of a peculiar character; a feeling of instant dissolution, terminated often by actual syncope: these are the prominent and principal elements of angina pectoris. If these depend on fatty degeneration, it is supposable that one or more of them may be wanting. Dr. Quain teaches us that the inexpressible sense of dying is sometimes the only symptom of the disease which he is portraying. Sometimes the pain is added: frequently the syncope. And this is intelligible if (what I think probable) the anguish results from over-distension of the unsound heart. Syncope may occur without any such distension. The softened state of the muscular tissue would admit of its being stretched a little whenever the centrifugal reaction of the contained blood was augmented; augmented by posture, by bodily effort, or by mental agitation. And this facility of yielding under a less and less degree of disturbing pressure, would accord with the ascertained fact that fatty degeneration is a progressive change. The cardiac nerves may be variously implicated in the textural alteration, and may variously resent the distending force. That slight over-expansion of a heart so diseased should excite the breast-pang is conceivable enough. We are familiar with pain of a similar sharp kind when the intestine is stretched by pent-up gas in colics. The *juvantia* of the disorder favour the same view. Stimulants, helping the languid and labouring muscle to contract effectually upon its contents, are effectual also, often, in relieving the paroxysm. This appears to me a more probable theory of the phenomena of angina pectoris than the theory which ascribes them to mere neuralgia, or the theory which ascribes them to spasm of the heart. Yet both these theories have been speciously advocated. To that which I offer you, you may give as much weight as it may seem to deserve.

I have spoken of this disorder as continuing to recur for months; it may even be for years: but it is frequently fatal within a much shorter period. Nay, the time between its first manifestation and its mortal close is, not very seldom, appallingly brief. My friend, Dr. Latham, lately gave me the following sketch of a case of this kind, which had fallen under his own observation. A gentleman, about fifty years old, was recovering from the influenza, of which nothing remained but a slight cough, that troubled him at night. It was to relieve this that Dr. Latham was consulted. The gentleman looked perfectly well. After Dr. Latham had prescribed for this little ailment, the patient begged to see him the next day to talk over with him (he said) a very strange affection he had. Accordingly he then described a paroxysm of angina pectoris in terms that could not be mistaken; dwelling especially upon the præcordial pain, the sensation down the left arm, the feeling of approaching dissolution, and then the perfect recovery. This gentleman had, during the previous summer, performed a walking tour through Switzerland, and returned home in excellent health. The first notice of his angina was less than a month agone, when he was walking up Hampstead Hill. It was then that he had his first paroxysm. In the short period which had elapsed, the attacks had rapidly increased in severity and frequency: occurring now every two or three days, or every day, or several times a day, with or without an obvious exciting cause. Dr. Latham made a careful examination of the chest, and found the respiration perfect, the heart free from all unnatural murmurs, and its beats rhythmical. The only thing that particularly attracted his notice was the exceeding feebleness of its impulse. In the afternoon of the next day Dr. Latham visited him again, when he described a paroxysm he had suffered in the course of the morning, of much greater severity than any that he had hitherto experienced. Dr. Latham saw enough to convince him that his patient's existence was very precarious: and as he had previously been a stranger to him, he inquired about his friends, and took down the address of a brother, intending to call and apprize him of what he feared. On reaching his own home, two hours afterwards, a messenger met him, announcing that his patient had fallen into another paroxysm, soon after he left the house, and was dead. The body was carefully examined by a thorough anatomist, Mr. Stanley. There was no disease of the aorta, or of the heart generally; but the coronary arteries resembled tubes of coral, being completely ossified as far as they could be traced.*

The patient may even expire in the first or second paroxysm. This happened in the case of the late lamented Master of Rugby School, Dr. Arnold.

Twice, in the course of my life, I have known the symptoms of angina pectoris, exquisitely marked, to be followed by perfect and enduring recovery. One of these cases was that of a lady who was about to accompany her husband to India, where he held a high office. She has since returned, and years have passed, and I occasionally see her in the enjoyment of excellent health.

You will perceive, however, from what has been said, that the prognosis of this singular and formidable affection is extremely unfavourable. The cases are very rare in which no disease of the heart has been detected: and the organic changes that have been found are remediless, and for the most part progressive: and, in point of fact, the great majority of those who have laboured under the disease have died suddenly, and prematurely.

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* The history of this case has since been published by Dr. Latham in his admirable *Lectures on the Diseases of the Heart.* He adds the important circumstance that 'its (the heart's) muscular substance was more loose of texture than natural, but not softened in an extreme degree.' He also gives a most interesting detail of Dr. Arnold's seizure and death: and he describes another instance, seen by himself, in which the period between the patient's first paroxysm and his last did not exceed ten days.

In this case 'the muscular substance of both ventricles was so soft, as to be pierced through with the slightest pressure of the finger.' In the spring of 1856 a well-known baronet died in London of angina pectoris; the disease having run the whole of its manifested course in nine days.

It follows also, as another corollary from the facts now brought before you, that there are very few cases in which we can dare to contemplate a cure. Our measures must be preventive when the paroxysms are absent: and our object will be to shorten the fit when it is present and protracted.

Now the preventive measures are simple and obvious. The patient must be cautioned to avoid the exciting causes of the paroxysm; walking up hill; or against the wind, which has also often been known to produce it. Whatever is likely to hurry the circulation, and therefore, among the rest, all mental emotion and anxiety, should be guarded against as much as possible. John Hunter died of angina pectoris: and the fatal seizure was brought on by a fit of anger. The very same precautions must be observed as those which I dwelt upon half an hour ago; and for the very same reasons. Care should be taken also to obtain and preserve a healthy state of the digestive organs. It is observable of this, as I told you before it is observable of other cardiac diseases, that they are often attended and aggravated by flatulence of the stomach and bowels. Persons labouring under a paroxysm of angina often experience great and sudden relief upon getting rid of a quantity of gas, by which the stomach had been distended. The flatulence acts, no doubt, by pressing the diaphragm upwards, and so diminishing the dimensions of the thorax, and impeding the play of the heart. It is upon the same principle that we must explain the fact, that the paroxysms are particularly apt to come on if the patient walks *soon after a meal*: also that they occur in the night, when he is in a horizontal position, and are relieved by his getting out of bed; that is, by his assuming the vertical posture, and taking off the pressure of the abdominal viscera from the diaphragm.

In the paroxysm itself, bleeding has been fairly tried: but, as I think might have been foreseen, it has seldom been followed by any benefit, and sometimes it appears to have done harm. The affection has a nearer relation to syncope, and often to syncope by asthenia, than to anything else. That is the way in which the patients die: and consequently, cordials, stimulants, and antispasmodics, are found to be of service. For the pain, when it is lasting as well as severe, the appropriate remedy is opium; or chloral. Dr. Elliotson thought prussic acid was the best thing you could administer. Dr. Davies had more faith in belladonna plasters than in most other things. Dr. Copland advised stimulant liniments externally; and warm carminative or aperient medicines, as the circumstances might require, internally. Hoffman's anodyne, under my own observation, has proved exceedingly useful. The general condition of the sufferer will suggest, I believe, the proper treatment. Not that it will suggest any particular drug, but it will teach you the main principle on which you are to proceed. If auscultation reveal any of those morbid states of the heart which were noticed in the beginning of the lecture, the means which I pointed out as suitable for remedying or relieving *them* may be put in force.

LECTURE LV.

Pericarditis: its frequent connection with Acute Articular Rheumatism. Rheumatic Carditis. Anatomical characters of Acute Inflammation of the Pericardium; of the Endocardium. General symptoms. Auscultatory signs. Relations of Carditis with Rheumatic Fever.

YESTERDAY I considered, cursorily indeed, but as fully as the limits of these lectures will permit, the effects of hypertrophy, and of dilatation with and without hypertrophy, of the several chambers of the heart; and the means we possess of obviating or alleviating those effects. The chronic changes to which the *endocardium* is liable, especially in those parts where it covers the valves, and the tendinous rings that support the valves, passed next under our review; with the various acoustic revelations of such changes. Lastly, I spoke of that singular and perilous affection to which the name of *angina pectoris* has been given. I proceed this afternoon to the diseased conditions of the *pericardium*. This membrane is often the seat of acute inflammation; and the result of such inflammation is sometimes, though rarely, the speedy extinction of life. But in a great majority of cases the disorder proves fatal at a remote period; destroying the subject of it more slowly indeed, but almost as surely. Pericarditis has, therefore, always been regarded as a very interesting disease; and the more so, that it is in many instances a very *insidious* disease also.

Acute pericarditis may occasionally arise, like most other internal inflammations, from exposure to cold; or when no exciting cause is apparent or discoverable. Sometimes it follows blows received upon the chest, or other mechanical injuries. But, with these exceptions, it is essentially dependent upon some morbid condition of the blood, and is therefore very seldom met with in persons who were previously in sound health. It is a frequent result of that abiding impurity of the blood which may be produced by certain forms of renal disease. It is apt to break out during the progress of specific fevers, which also imply blood-poisoning. But by far the most frequently of all does pericarditis happen in connection with a constitutional complaint that we have not yet had before us—

acute rheumatism; a febrile disorder, characterised by inflammation of a specific character, affecting the structures that lie around the joints, or enter into their composition—the *fibrous* tissues. I shall therefore consider acute pericarditis with reference to its occurrence in rheumatic fever; for in so doing I shall embrace all the practical points which belong to it under any form. But I must tell you that when *peri*carditis happens, in the course of an attack of rheumatism, so also, to the best of my belief, in almost every instance does *endo*carditis. For this reason I shall include, in the account I am desirous to give you of *rheumatic carditis*, both these inflammations: inflammation of the investing membrane, and inflammation of the lining membrane of the heart. I mentioned in the last lecture, that, with respect to the latter, to endocarditis, there were some peculiarities noticeable, which I should reserve for the present occasion.

The pericardium is one of the serous membranes; such too may the endocardium be considered. But the pericardium is also a shut sac; and the primary effects of inflammation upon it are the same, *mutatis mutandis*, as upon the shut sac so near it, the pleura. The second series of effects is however much more formidable. Adhesion of the pleura does not necessarily abbreviate the natural term of the patient's life; adhesion of the pericardium almost always does: and effusion into the cavity that contains the lung is far less serious than effusion into the bag that surrounds the heart. In the one set of organs the mischief may be great, but it is *final*; in the other, it leads, in most instances, with unfailing certainty, sooner or later, to worse changes, which at length prove incompatible with the further continuance of life.

You will understand, then, without any necessity for my going again into much detail, that the pericardium, under acute inflammation, may undergo the same changes which, on a former occasion, we saw that the pleura might undergo. Coagulable lymph may be poured forth from the entire membrane, and abolish the cavity by glueing the whole of the pericardium to the heart: or serous fluid may be effused, distending the bag of the pericardium, and keeping its opposed surfaces more or less asunder: or both serum and lymph may be effused together: or fibrin, in some shape or another, may be *deposited*, for aught I know, from the homogeneous fluid which is thrown out by the inflamed membrane in the first instance; and one result of this mixed effusion may here also, as in the case of the pleura, be the *partial* adhesion of the opposite surfaces of the membrane.

In most instances the inflammation spreads over the whole membrane, as it is apt to do in serous membranes generally; and one of these two things happens: either there is a large quantity of liquid effusion, which is not reabsorbed, and then usually the patient dies in a few days; or there is not much liquid effusion, or the liquid part is absorbed, and the pericardium becomes permanently agglutinated to the heart, and *apparent* recovery takes place.

In the cases that have proved fatal at an early period, when the inflamed membrane has been unadherent, it has been found to contain serous fluid; sometimes clear, oftener turbid, frequently tinged with blood; and it has been seen to be covered with a coating of the fibrinous or albuminous part of the blood; what we have called plastic or coagulable lymph. The deposited lymph assumes, in different cases, a variety of forms; but in every case that I have seen, the prevailing character of the unattached surface has been that of *roughness*; and this is a circumstance of some importance, as we shall presently see. The lymph is not arranged in smooth layers; but it is rugged, villous, or reticular. According to the fancy of different observers, it has been thought to resemble lacework, a sponge, a honeycomb, some kinds of coral, or the interior of a calf's stomach. Sometimes it bristles with a multitude of small, short, pointed papillæ; less frequently it is softer and shaggy; always it is rough and uneven. Dr. Hope, following Laennec, states that the surface looks something like that which would be produced by suddenly separating two flat pieces of wood, between which a thin layer of butter has been compressed. To my own eye, the appearance presented by the membrane, in its recent condition, has been more like the rough side of the pieces of tripe which you see in the butchers' shops, than anything else.

When, on the other hand, the patient dies, as he sometimes may do, soon after the whole of the membrane has become adherent, you will find the medium of adhesion to consist of lymph, in which a number of bloody points or streaks are visible; but still the connecting substance is soft, and the agglutinated membranes can readily be torn asunder.

Such is the state of things on the *outside* of the heart in such cases. But what do we find *within*? Why, here also, in *all* cases *probably*, certainly in by far the majority of cases, we may discover evident traces of inflammatory damage; and we discover them chiefly on the valvular apparatus. There does not appear to be such a tendency in endocarditis to diffuse itself over the whole membrane. Occasionally that naturally transparent portion of it which covers the muscular fibres is rendered whitish and opaque; and occasionally some of the deposits that are common on the valves encroach also somewhat beyond them, and even stud, here and there, the interior of one or more of the chambers of the heart, and especially of the left auricle. But the valves, or the fibrous rings from which they spring, are the parts first and chiefly implicated, especially the aortic valves, and the mitral valve, not uncommonly the tricuspid valve also; and sometimes even the semilunar valves of the pulmonary artery. Inflammation thus affecting both the external and internal membranes of the heart, in acute rheumatism, I would call *rheumatic carditis*.

The inflamed valves undergo two kinds of change, distinct from each other. They become thicker than natural; they lose their transparency and pliancy, and are puckered. These changes depend upon the deposit of lymph *beneath* the membrane; between the membrane and the fibrous substance which it covers. Sometimes they are folded down, and glued as it were, to the opposite surface. This must be by coagulable lymph deposited on the *outer* side of the membrane. But more frequently than all, they present more or fewer of those wart-like projections, or fibrinous granulations, which I spoke of in the last lecture, and which are of course *upon* the free surface of the membrane. Sometimes these granulations are scattered irregularly over the convex surface of the valve, or in its immediate neighbourhood; much oftener they have a more definite and curious distribution; an arrangement which I had never seen noticed by any author, but which it had been my lot so many times to observe, that it led me to remark an anatomical peculiarity with which it is connected; and this piece of minuter anatomy I long looked for in vain in books, and in vain sought information about among anatomists of my acquaintance. They none of them seemed to be aware of it, though they acknowledged that the fact was so when I pointed it out to them. I have now learned, however, that the peculiarity of structure to which I allude is somewhere indicated by Morgagni.

It becomes necessary, therefore, that I should describe to you this discovery of mine (if it deserve so grand a name), as I was, for several years, in the habit of showing it to the pupils of the hospital in the dead-house. It derives its chief interest from the light which it throws upon the morbid appearances to be spoken of presently. You will find, then, if you examine closely the semilunar valves of the aorta, or of the pulmonary artery, that in each valve there may be distinguished two parts: one thicker, the other thinner. The thicker part lies next the base of the valve; the thinner next its edge. And the valve does not become thin by degrees, but the difference is marked by a manifest line of separation between the thicker and thinner portions; and this is not a straight, nor even one sweeping curved line, but it forms a double curve. It consists of two semicircular lines, running each from the centre of the edge of the valve, from the sesamoid body there situated to either extremity of the edge, where the edge joins the side of the aorta. So that there are two segments, of a crescentic shape, thinner and less opaque than the remaining part of the valve, and lying near its free margin. This peculiarity of structure is uniformly present. It is less distinctly visible in the valves of the pulmonary artery than in those of the aorta, and it is much less apparent in some individuals than in others; but it is always to be seen when it is looked for.

The anatomical account of this arrangement is not far to seek. The cardiac valves consist of a loose duplicature of the delicate endocardium, between the folds of which is received a thin prolongation of fibrous tissue, from the tendinous rings surrounding or constituting the several orifices that are furnished with a valvular apparatus. In the semilunar valves this fibrous substance does not interpose itself between the entire space of the folded membrane. It reaches the free edge of each valve at three points only; namely, at the centre, where it forms the *corpus Arantii*, and at the two extremities. Between these points it stops short, and has a definite limit and outline—a scalloped edge—and so leaves two crescentic portions of the valve, formed merely by the doubled endocardium. The crescentic margins are thin and transparent: the remaining shield-shaped portion of the valve is more or less thick, firm, and opaque.*

And the physiological reason of this arrangement is also apparent enough, though I failed to perceive it until it was

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* This formation of the arterial valves is described and delineated by Morgagni, in his *Adversaria Anatomica*; as the late Dr. Todd, after the first publication of these Lectures, was good enough to point out to me.

explained to me by Mr. Thurnam. Each valve, when opened out, is convex towards the ventricle. The three valves do not merely meet by their edges. Their common purpose would be but insecurely provided for if such were the case. They meet and bend up, and come broadly into contact with each other. Each valve during the diastole has its right and left crescentic portion applied respectively to the corresponding portion of its right and left fellow-valves. The thin segments are pressed mutually together, and lie *dos-à-dos*, as dancers say; while their edges look in the direction of the vessel. All this you may convince yourselves of by injecting the aorta of an ox with wax, and picking out the wax when it is cold; or, more simply, by cutting the aorta across an inch or two above its origin, and then pouring water into it towards the heart. The pressure of the water throws the valves into the position which they assume during the diastole of the ventricle. This last experiment furnishes an easy means of testing the efficiency of the valves.

Now the curious fact which first led me to remark this natural structure is, that the minuter granulations, which form upon the aortic valves in acute rheumatic carditis, most commonly arrange themselves in a row, like a string of beads, along the line of union between the scalloped edge of the thicker scutiform portion of the valve and the inner convex margin of the two thinner crescentic portions. Sometimes they follow that double festoon very exactly and completely; sometimes the continuity of the line is broken, and the granulations straggle from it a little; but still the general tendency to adhere to it is evident. No one that I know of had publicly noticed this fact; yet that it is a fact, a good many persons who had been for some time about the Middlesex Hospital became perfectly aware. The truth is, that death seldom happens early in these cases; and perhaps the valves had not always been carefully examined when opportunity did offer. My friend Dr. Latham had been watching for such a case in vain for some years. At length, however, two of his hospital patients died in the first attack of rheumatic carditis; and he tells me that, looking with great interest for the morbid appearances within the heart, he found them such as I have been stating. I have chanced to see eight or ten such early fatal cases.

The arrangement just described is the most common one, so far as the aortic valves are concerned; but sometimes even there, and generally upon the mitral and tricuspid valves, the little wart-like bodies have a different position; jagging the free edge of the valve with numerous fine serræ, like the teeth of a small saw; or being disposed, just within its border, in one continuous line.

After what has been said, you will readily detect the physical cause of this curious distribution of the little wart-like masses. The membrane suffers inflammation. Soft lymph exudes from it, or is deposited upon it; and as fast as it is formed or deposited, it is pressed aside, by the repeated concourse of the opposed surfaces, from the crescentic portions of each valve, and heaped up along the boundary lines of contact: just as a thin layer of butter on a board would be displaced, and heaped up in a little curvilinear ridge, by the pressure of one's thumb. The double festoon, and the little marginal teeth, are obviously both formed in this way.

If my verbal descriptions have been insufficient to make all this clear to your apprehension, the drawings before you speak, I hope, in plainer language.*

These, then, are the appearances commonly seen within and without the heart, when the patient does not long survive the first attack of rheumatic carditis. When death takes place at a later period, you find more than this. You find the consequences which flow from these primary lesions operating as mechanical causes of further change: hypertrophy and dilatation in their various degrees and combinations; or, sometimes, atrophy.

You will please to bear the primary changes in mind; for they satisfactorily account for the *physical signs* of pericarditis and of endocarditis which are displayed in these cases, and which I shall describe and explain after I have shortly inquired into the *general symptoms*.

The symptoms, then, of pericarditis, as set down by authors, and such as I have myself frequently noticed, are the following. There is often, very early in the disease, a singularity of manner, and peculiar expression of countenance, difficult to describe, yet strikingly manifest to the observer; a strangeness of deportment mixed somehow with an aspect of distress, or of alarm. To this are frequently added, pulsation within the chest; a sense of oppression in the epigastrium; a catch in the breathing; a dry cough; inability or unwillingness on the part of the patient to lie on his left side; pain in the situation of the heart, increased by a full inspiration, by pressure upon or between the corresponding ribs, and more particularly increased by pressure upwards against the diaphragm by means of the fingers thrust beneath the cartilages of the false ribs; stiffness and pain in and about the left shoulder, and extending thence down the left arm, and stopping short perhaps at the elbow or the wrist. This last circumstance, however, the pain shooting down the arm, is more common in *chronic* affections of the heart. Sometimes jactitations occur, like the jactitations of chorea. And I have yet another symptom to mention, and a very important one; and that is delirium, sometimes quiet, but often wild and furious delirium, not dependent upon any obvious disease of the encephalon.

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* Dr. George Johnson has recently put forth a somewhat different view of the manner in which the so-called vegetations are distributed. Observing that they occupy the most prominent and rigid portions of the altered valves, he holds that the stream is whipped of its fibrin by the hard and uneven surface which it strikes against. It is noticeable that the deposit is always most plentiful on the surfaces that look *stream-ward.* There is no great inconsistency between the two explanations—his and mine.

Of course there are also the febrile symptoms which accompany the acute rheumatism; or if the pericarditis occur independently of acute rheumatism, there will usually be fever symptomatic of the local inflammation.

Now each of these symptoms I have repeatedly observed; but they seldom all concur in the same case. If they did, there would not be much difficulty in the diagnosis: nor would the cardiac disease be so often overlooked as it has been. The diagnosis of pericarditis has been confessedly uncertain and obscure. Not unfrequently, nearly all the symptoms that I have been enumerating are wanting; or are so indistinctly marked as to attract no attention. It is therefore an important matter to ascertain what help we may derive, in these equivocal cases, from auscultation.

In truth, the help which we sometimes get is peculiarly valuable and satisfactory. There are characteristic morbid sounds to be heard when the heart is beginning to labour under rheumatic carditis.

The morbid sounds which may reach the ear applied in such cases to the precordial surface are two: very distinct the one from the other, and for the most part very distinguishable; depending upon different causes, and denoting diversities of operation and of site in the morbid processes going on within the chest. But they are not both audible in all cases.

One of these sounds I have been accustomed to call a *to and fro* sound. It conveys to the ear the notion of the rubbing of two rough surfaces, backwards and forwards upon each other.

It seems near to the ear, and therefore near to the surface of the patient's body. Like all the other morbid sounds heard within the chest, it is subject to much variety in tone and degree. Sometimes it very closely resembles the noise made by a saw in cutting through a board. Sometimes it is more like that occasioned by the action of a file, of a rasp, of a nutmeg-grater. But its essential character is that of *alternate rubbing*; it is a *to and fro* sound. This very peculiar sound I had noticed and described, and explained, before I was aware that it had attracted the attention of any other persons. Others, however, had remarked it, and had correctly interpreted its meaning. I claim no credit, therefore, for the discovery of a very important symptom; but I claim for the symptom itself that additional weight which accrues to it from its having been originally perceived by different observers, independently of each other. The physician who, in this country, without my being aware of it, had noted and published some cases in which this phenomenon occurred, is Dr. Stokes, of Dublin. There is a good deal said about it by Bouillaud also; and he too appears to have discovered the sound, without any previous knowledge of its having been noticed by others. I have heard the *to and fro* sound now in some scores of cases. In a few of these it never ceased except with life. The patients died during the primary attack, and the *to and fro* sound remained as long as the heart continued to beat. In all the other cases, the *to and fro* sound was audible for a few days only, and then ceased entirely, and probably for ever: the patients recovering more or less completely.

The other of the two morbid sounds, is the ordinary bellows-murmur, with which you are already familiar. In the case in question it is a single sound; a deep-seated rush, or whiz, accompanying the systole of the heart. It usually continues long; often for life.

These two sounds, the superficial *to and fro* sound and the deep-seated *bellows*-murmur, may sometimes be heard, by a careful listener, to exist together. Sometimes the bellows-sound begins to be distinguished when the rubbing sound ceases; appears to supervene upon it, or to take its place; perhaps it then first becomes audible, simply because it was previously drowned in the louder superficial sound. Sometimes there is no *to and fro* sound, but only the deep blowing noise, or (what in many cases is extremely probable, nay, what I may venture to say is certain) the *to and fro* sound has *come* and *gone* unnoticed—unlistened to.

Now of these sounds, which I repeat are perfectly distinct, and capable of being easily discriminated the one from the other, the first mentioned, viz. the *to and fro* sound, is always indicative of inflammation of the *external* membrane; the other, the bellows-sound, is always, as I believe, in these cases, indicative of inflammation of the *internal* membrane of the heart. You will bear in remembrance, that I am speaking of these sounds as they somewhat suddenly occur for the first time, and especially as they are apt to occur in *rheumatic* carditis at its *first* accession.

Those of you who have seen the thorax opened in an animal whose heart still continued to palpitate, may have observed, as I have done, that the pericardium lies closely in contact with the heart, but that a considerable extent of slipping motion between them goes on at every successive act of systole and diastole. They glide over each other evenly and without noise; but this is only while the surfaces are smooth and healthy. When they are already made rough by inflammation and the deposit of lymph, which lymph always, as I have shown you, *is* rough in such cases, then the attrition will be no longer noiseless: it will give rise, in the alternate movements of the organ, to the harsh and superficial *to and fro* sound. But why does that sound, when once it has occurred, at length cease; and why, in many cases, having once ceased, does it never by any accident, when the inflammation has been universal, recur? Clearly because the pericardium has become adherent to the heart: after which there can be no motion of the one membrane upon the other, and therefore no sound indicative of such motion.

That this is the true explanation of the occurrence, and of the permanent cessation, of the *to and fro* sound, I am now (March 1837) convinced. It was a matter of inference with me for some time. A few of the patients died during the primary attack. By much the majority recovered. I do not mean got perfectly well as they had been before; but they regained a great share of their usual health, perhaps seemed, and *thought* themselves *well*, and left the hospital where they had been under treatment. Now, of those who died the pericardia were non-adherent. The opposite surfaces of the membrane were rough, and like tripe: and the to and fro sound never ceased in these persons. Such cases are always soon fatal. But, in the others, did adhesion take place? I make no doubt of it. Within the last twelve months I have had demonstrative proof of it in two instances. One of these occurred in a hospital patient, whose case has been published in the 'Medical Gazette.'* He was a painter, nineteen years old; and he became my patient last May, with acute rheumatism and carditis. From May 26 to June 13, a *to and fro* sound was distinctly audible, as well as a bellows-sound which had preceded it. After that date, the bellows-sound continued, but the rubbing sound was no longer to be heard. The patient improved; and was about to be discharged: when, on June 29, sixteen days after the sound of friction had finally ceased, he suddenly dropped down dead in the garden of the hospital.

Here I had concluded that the pericardium was adherent; though I had not expected to have so soon the opportunity of verifying my opinion. And accordingly, except over a small portion of the posterior part of the right ventricle, the union between the heart and its investing bag was complete at all points. The agglutination was evidently the work of recent disease. The medium of adhesion was of considerable thickness; and consisted of coagulable lymph, and coagulated half-organised blood. The pericardium was stripped off as I have seen a poulterer skin a rabbit, and with about the same ease. This was a very interesting case to me, for it was the first in which I had had the privilege of examining the heart after having witnessed the peculiar succession of phenomena that I have been describing.

But since that time, I have met with another such case in private practice. The particulars of it are sufficiently important to warrant my relating them.†

In the month of October, 1836, I was taken by Dr. Sweatman to see a patient of his, whom I found sitting up in bed, pale, with sharp features, breathing shortly and laboriously. His legs were anasarcous, and his belly was tense and fluctuating.

I learned that he had been for years given up to intemperance in drinking, and to indolent and low habits. He told me, that the wind troubled him, shooting up through the whole of the left side of his chest. On further inquiry I found his meaning to be that he had much pain there. There was loud wheezing over the upper lobes of both lungs, both sides of the thorax were dull on percussion at their lower part, and on the right side no breathing whatever was audible below. These latter symptoms were indicative of dropsical effusion into the pleuræ also. The jugular veins were swollen and tortuous on both sides of the neck. On applying my ear to the præcordial region I at once heard a very loud and distinct *to and fro* sound. This was equally manifest when he held his breath. Dr. Sweatman, who was not so much accustomed as I have been to listen to the sounds of the heart in disease, recognised instantly the peculiar character of this sound. I ventured to express my certain conviction, that the patient was labouring under recent and acute pericarditis. I added, that he had also hydrothorax; and that, whatever chronic changes might have taken place in his heart previously to his present illness, dilatation of the right cavities constituted at least one of them.

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* Vol. xviii. p. 701.

† *Medical Gazette*, vol. xxi. p. 544.

He had been attacked by his present urgent symptoms three days before I saw him, viz. on October 8. On that day, in all probability, the inflammation of the pericardium commenced.

This was his history. In the spring of the year, having, from indolence, kept the house for months before, he crossed from the Isle of Man to Liverpool, and was sick, and suffered a good deal during the passage. After landing, he had a mile or more to walk. His companions outstripped him, but were called back to him, and found him very pale, breathing with difficulty, and unable for a time to proceed. He attributed all his subsequent complaints to that exertion: and he had been ailing, though not confined to his room, till October 8. Some leeches were applied to the præcordia, and he took diuretics.

I did not see him again till October 15, four days after my first visit. The rubbing sound was still there, though less loud, less harsh, and less extensive. The leeches had given him much relief. His pulse was very small. A blister was now applied. On the 20th, I saw him for the third and last time alive. The rubbing sound was quite gone. Of this Dr. Sweatman also satisfied himself. There was a dull systolic bellows-sound in its place. The pulse was scarcely perceptible: but he continued apparently improving, making a vast quantity of urine, while the dropsical swellings fast diminished, till the 31st; when, after talking a short time oddly, and in a peculiar, loud voice, he sat up to take some medicine; and having done so, reclined his head against the nurse, and expired. Mr.

Shaw assisted in the subsequent examination of the body. I omit giving an account of the condition of the lungs and pleuræ, which was what we had anticipated; and confine myself to the state of the heart. That organ was large. The pericardium was adherent universally by means of lymph, mottled with blood, and it was easily separated, so that the adhesion must have been recent, as was proved indeed by the symptoms. The right cavities of the heart were very large; and the aorta was diseased.

The existence of the *to and fro* sound in these cases no one can doubt who has once listened for it when present: and the facts respecting it, which have been established beyond the reach of controversy, are these:—1st, That when it occurs *de novo*, it always and surely denotes acute inflammation of the pericardium. I say *de novo*, because (as I stated in the last lecture) a *bellows-sound* may accompany *each movement* of the heart, in consequence of *internal* disease of some standing: and this double, sawing, bellows-sound might possibly be confounded with the alternating noises produced by the attrition of the opposite surfaces of the inflamed pericardium. If any doubt should ever arise in your minds respecting the meaning of sounds which are sometimes thus similar in character, while they result from very different conditions, it may aid your diagnosis to remember, that endocardial murmurs are often plainly traceable along the direction of the blood-current through the great arteries of the thorax; and that the exocardial rubbing sound, though it may be audible over nearly the whole of the chest, is not heard with any special distinctness in the arteries. 2ndly, The *to and fro*, or rubbing sound, is never of long duration, but soon terminates in one of two ways. Either the patient dies in a short time, the sound continuing to the last; and then the pericardium is found coated with rough lymph, but throughout the far greater part of its extent, or altogether, *un*adherent: or the sound ceases, never to return, while the condition of the patient improves; or he even seems to himself and to others to recover his perfect health. In these cases, the sound ceases from a physical impossibility of its continuance, viz. from adhesion of the pericardium over the whole, or the greater part, of the surface of the heart. And in this category of apparent but unreal recoveries, I cannot doubt that many of Bouillaud's cases of '*pericarditis terminating in health*' ought to be included.

It is stated in books that the distension of the pericardium with fluid beyond a certain amount stops, for a while at least, the mutual attrition of the opposite inflamed surfaces, by separating them; and stops, therefore, the rubbing sound: and that when, from absorption of the fluid, the distension sufficiently diminishes, the parted surfaces come together again, and the to and fro sound, now denominated a *redux* friction sound, again becomes audible.

My own experience would lead me to demur to the accuracy, or at least to the general exactness, of these statements. It has *never* occurred to me to witness such suspension and renewal of the rubbing. As the alleged sequence of phenomena has been observed by others, there is an end, of course, to all question of fact: but I cannot help suspecting that it may have been more often inferred as probable than actually observed; inferred through a supposed analogy with the friction sounds of pleurisy. The two cases differ materially. In pleurisy with effusion the yielding pulmonary surface is held apart from the fixed and rigid costal surface during the successive acts of respiration: whereas the tilting movements of the heart against its loose containing bag can seldom fail to bring the roughened surfaces into partial collision, especially towards the base of the organ. In pleurisy with effusion the dulness to percussion, and the sound of rubbing, are not simultaneous, but alternate; in algebraic phrase, the one varies inversely as the other. In pericarditis with effusion it is not so. I find that Dr. Latham's experience led him also to say that serous effusion within the pleura *always* obliterates the attrition sound, and that serous effusion within the pericardium *generally* leaves it unaltered. Dr. Walshe, for whose opinions I have a very great, and for whose facts an absolute respect, thinks that less than eight ounces of fluid will generally suffice to annul the friction murmur; but he adds that in a remarkable case under his own observation 'friction murmur was well heard a few hours before the death of an individual whose pericardial sac contained sixty fluid ounces (three imperial pints) of liquid effusion.'

This is one of many unsettled points which you must test by your own observation in future.

The increase, or the diminution, of the pericardial fluid effusion may be measured with much precision by percussion of the surface of the chest. The natural, somewhat triangular, area of precordial dulness—the area corresponding to that portion of the heart's surface which is not overlapped by the lungs—enlarges; and, enlarging, takes the shape of the distended pericardial sac, becomes pyramidal, with the apex of the pyramid looking upwards: its base corresponding with the lower edge of the sixth rib, its apex rising to the second left cartilage. Even these dimensions may in extreme cases be transgressed.

If you hear the to and fro sound, and note that it disappears as the area of the distension-dulness enlarges, and reappears with its subsequent diminution—the physical conditions of this remarkable association and sequence of phenomena may fairly be said to have been *demonstrated.*

Another notable effect of the distension of the pericardium is a twisting upwards of the heart's apex; the displacement varying with the varying amount of the effusion. If the actual displacement, in its several degrees, has been watched and seen, this symptom will of itself be conclusive.

The to and fro sound being, like the friction sound of pleurisy, a fugitive sound, may escape observation simply through its not being sought for at the opportune time. It may have ceased from general agglutination of the pericardium to the heart; or, according to the experience of others, from the distension of the sac by a sufficient quantity of fluid.

It follows as a necessary consequence from the facts now brought before you, that acute and general pericarditis, so far advanced as to occasion the pathognomonic rubbing sound, does not admit of a perfect cure; and that its best event is the adhesion of the membrane, and the obliteration of its cavity.

And even then, I say, the change is not final. An adhering pericardium does so embarrass the movements of the heart as to cause at length, sometimes rapidly, sometimes slowly, further changes, affecting the muscle thus held in its morbid embrace. It has indeed been shown, by Dr. Barlow and Dr. Chevers, that this restricting cover has no direct tendency (as had commonly been supposed, by myself among others) to produce hypertrophy. On the contrary, when the adhesion takes place during youth, as in connection with acute rheumatism it is very apt to do, it seems to prevent the further growth of the heart, and virtually leads to atrophy of that organ, or a disproportionate smallness of its cavities, vessels, and general size. Dr. Barlow points out a mode in which simple adhesion of the pericardium may indirectly bring about hypertrophy of the right ventricle, through its influence upon the functions and development of the lungs. Instances are numerous, however, of considerable hypertrophy of the left chambers, and of the whole heart, coexisting with an adherent pericardium. But in these cases the hypertrophy is really due to the impediments opposed to the free passage of the blood by valvular or other endocardial disease. It is held, too, and I believe justly, though I am not so sure of this as of some of the other points I have been dwelling upon, that the inflammation which begins in the membrane sometimes dips into the muscular substance of the heart, weakens its elasticity and cohesion, and so leads ultimately to dilatation of its cavities.

I need not occupy much of your time in speaking of the other morbid sound that is audible in these cases, the bellows-sound: which sometimes may be heard before the to and fro sound commences; which I have frequently heard *through* the *to and fro* sound; and which often remains after the superficial rubbing sound has ceased. This depends, no doubt, upon those alterations in the lining membrane, and especially in the valvular apparatus of the heart, which take place from inflammation, at the same time with the alterations of the pericardium. And when it is met with in such cases, it may be set down as indisputable evidence of the existence of endocarditis.

I am anxious that you should take an interest in the disease of which I have been speaking at so much length this evening, and that you should keep it in mind in your future practice: for I am certain that it is a fertile, but often unsuspected source of chronic disease of the muscular substance of the heart, and of its consequences; asthma, dropsy, sudden death. The number of patients that come into the hospitals of London affected with acute rheumatism is annually very large: and I am sure that I do not exaggerate when I say that more than one-half of them have the heart or its membranes implicated. The cardiac affection may easily be overlooked both by the patient and the physician. The recovery may appear to be perfect. But after some time, palpitation begins to be occasionally felt; and, by degrees, other symptoms, marking disease of the heart, declare themselves: but their origin is unsuspected or forgotten. You will be surprised, if you search back into the past history of all the patients who apply to you having disease of the heart, especially among the lower classes of society—you will be surprised to find how many of them will acknowledge that at some time or other of their lives they have been laid up with rheumatic fever.

It is no part of my purpose to treat at present of that specific disease of the joints to which we give the name of acute rheumatism: but I may as well complete what I have to say of carditis as it occurs in connexion with that disease; and then I shall not need to repeat myself when I come at length to rheumatic fever.

In the first place, then, I would say a few words more respecting the nervous disturbance which is apt to supervene in such cases, and to mask the real disease, and to mislead the unwary practitioner. Patients labouring under rheumatic carditis very frequently become affected with delirium, or violent mania, or stupor and coma, or convulsions, or all of these in succession; and you might suppose that they were labouring under inflammation of the brain, or spinal cord; or of their membranes. Such cases are in fact spoken of as cases of *metastasis* to the brain. It may sometimes be so, nay, I know that it sometimes *is* so; but not often.* Again and again, when death has occurred, and the delirium had been extreme, no traces of disease have been discoverable within the skull, nor within the vertebral canal, while marks of violent and intense inflammation have been visible in the pericardium. It may be that the acute cardiac affection interferes somehow with that regulated supply of blood to the head, which is necessary for the due performance of the cerebral functions. It may be that the morbid quality of the blood in rheumatism disturbs them. It may be that the cerebral or spinal symptoms are purely reflex phenomena, of eccentric origin, and excited by the irritation of incident nerves, pertaining to the heart. Or it may be that embolism of the minute blood-vessels of the brain gives rise to the cerebral symptoms; certain it is that in many instances in which such cerebral disturbance had been present, fibrinous deposits upon the cardiac valves were present also. Whatever the explanation, recollect the fact; and whenever, in acute rheumatism, you find your patient flighty and wandering, or more distinctly delirious, or affected with any form or degree of convulsion, examine carefully the condition of his *heart*.

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* The accuracy of this statement has been questioned. It was founded upon the single instance of a female patient of my own, who died in the Middlesex Hospital after symptoms of cerebral inflammation, supervening upon acute rheumatism. Unequivocal pus was found smeared over the hemispheres of her brain. In the 29th volume of the *Medical Gazette*, Dr. Fyfe of Newcastle has related the history of a very similar case; a third example is recorded by Dr. Fuller, as having occurred in St. George's Hospital under the care of Dr. Seymour.

As this is really a point of great importance, and as you will not find much information respecting it in books,* I shall take leave to quote, here, some part of a clinical lecture delivered by myself at the Middlesex Hospital, in the year 1835, and printed in the sixteenth volume of the 'Medical Gazette.' My subsequent experience has been quite in conformity with what I then stated.

'The functions of the brain not unfrequently become disordered in rheumatic fever: and disordered in such a manner and degree as would lead, and has led, many to believe in the presence of active inflammation of that organ, or of its enveloping tissues. Yet this affection of the brain is not, I believe, inflammation, but some secondary affection of the circulation therein; resulting from disturbance at the central organ of the circulation, capable of producing a corresponding derangement in the cerebral functions. I can best explain what I mean by reciting a few examples.

'The first case of this kind that I ever saw or heard of, occurred in St. Bartholomew's Hospital, many years ago. I took notes of it at the time, and will read you the substance of them. Charlotte Rankin, aged 17, was admitted there on August 12, 1824, under the care of Dr. Roberts, with acute rheumatism of the joints. Her illness had come on suddenly a week before, after unusual exposure to cold and wet. The pain and swelling had shifted much from joint to joint. She had been bled, on account of pain in her left side, two days before admission. On the 14th, she complained of much difficulty of breathing, and of pain when even slight pressure was made upon the chest. These symptoms were entirely removed by a blister. On the 16th, she was observed to be odd in her manner—peevish, querulous, restless, without sleep, and desirous of getting out of bed. Her pulse was then 100. On the 20th the pulse had risen to 120; it was quite regular. She said she felt no pain, except the soreness occasioned by the blister. She slept very little. On the 21st, the pulse was 128. Some jactitation of the left arm was now observed, which, she said, had never happened before. No sleep. On the 22nd, about nine in the evening, she became furiously maniacal, and it was necessary to confine her by a strait-waistcoat. She continued in that state for upwards of four hours, and then died.

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* This statement is no longer true. Dr. Burrows has fully considered it in his valuable essay, published in 1846, *On Disorders of the Cerebral Circulation, and on the Connexion between Affections of the Brain and Diseases of the Heart.* It is gratifying to me to find in Dr. Burrows' observations upon this interesting subject so striking an accordance with and confirmation of my own, which at that time had not fallen under his notice.

'Twelve hours afterwards the body was examined. The brain was found quite healthy: its vessels seemed, indeed, somewhat fuller of blood than is usual, but there was no effusion, nor any other vestige of inflammation.

'The pericardium was glued to the heart, in several places, by recent adhesions; and it was universally coated, where not adherent, by a layer of rough reticulated lymph, remarkably harsh to the touch.

'Now here the most prominent symptoms were such as we are accustomed to refer, with tolerable confidence, to inflammation of the membranes of the brain; whereas, in fact, the inflammation was strictly confined to the heart. If no examination of the body had been made, the case might have been quoted, with much show of reason, as a well-marked example of metastasis to the brain. It *was* so considered, before the brain was inspected.

'There had, indeed, been symptoms which indicated, and that not obscurely, the cardiac disease. At that time, however, I did not know how frequently carditis is combined with acute rheumatism. Auscultation had not yet come much into fashion in this country; at any rate, I knew little or nothing of its use; and I had supposed (and it had been supposed by others who witnessed the case) that the chest symptoms resulted from rheumatism of the intercostal muscles.

'Another instance, in which the course of the symptoms was somewhat different, yet equally calculated to mislead, you have lately seen in this hospital.

'William Wilkins, a post-boy, 28 years old, was admitted on the 25th of last November.

'He complained of pain in most of the large joints, shifting from one joint to another. There was no visible redness or swelling, but he had much fever. The pain was greatest at night. He had profuse perspirations, during which the pain was not mitigated.

'He had been ill eight weeks; and at first his joints (according to the statement of his friends) were both swelled and red. He appeared to be recovering at one time, but relapsed. For three or four days previous to his admission he had coughed a little, and spoken of pain at the pit of his stomach. He lay more comfortably on the right than on the left side, but this was habitually the case. He had never had acute rheumatism before.

'He rambled a good deal during the night of the 26th, and on the 27th he began to refuse to take his medicine, appeared confused and stupid, and answered questions tardily and imperfectly. He was bathed in perspiration, which had the strong acid smell so common in cases of acute rheumatism.

'During the next ten days he remained in a singular state of quiet delirium, rejecting medicine and food, saying he had had enough; getting out of bed, especially in the night, and declaring that he was going home. When questions were put to him, his lips moved, and his limbs began to stir and fidget, as though he were about to answer; yet he said nothing. He understood what was said to him, and put out his tongue when desired so to do; imperfectly, however, and with slowness and apparent difficulty. His bowels were costive, and he passed his stools, when purgatives acted, in the bed. His pulse was small and frequent; and when his wrist was taken hold of that the artery might be felt, he always resisted, and forcibly contracted his arm.

'Then for three or four days he appeared to improve; his countenance became more clear and lively; but he still showed the same restlessness, and maintained the same dogged silence when spoken to, and obstinately refused to swallow medicine. He was somewhat cunning, too, for he would take pills into his mouth, and then, when he thought he was not observed, chew and spit them out again.

'His pulse became at last very frequent, and his strength diminished rapidly. He died on the 18th December, and the body was examined on the following day.

'The cerebral veins were gorged with dark blood, and there was a considerable quantity of serous fluid beneath the arachnoid, and in the lateral ventricles.

'The pericardium was free from disease; but upon the mitral valve, near its edge, there was a perfect row of small, slender, bead-like warts.

'A few weeks ago I was consulted in a case of a similar nature, which occurred in the practice of a gentleman who was formerly a pupil here, and who was fully aware both of the frequent occurrence of carditis in acute rheumatism, and of the anomalous symptoms with which it is sometimes attended. The patient was a young man, 24 years of age.

'On the 22nd of December he was seized with pain and swelling of several of the larger joints, and with fever. The attack was ascribed to exposure to cold the day before; he had previously enjoyed perfect health. The inflammation shifted rapidly from one joint to another. He was confined to bed for six days: then feeling better, he got up, changed his room, and presently underwent a relapse. Mr. Elwin tells me that, after that time, he was never comfortable about this patient; his countenance was pale, and his aspect unpromising; his pulse frequent; and more than once he complained of slight pain in the epigastrium, increased by a full inspiration. This was removed by a mustard-poultice. No morbid sound was detected upon a careful examination of the præcordial region by the ear. He remained low-spirited, but slowly mending, till the 3rd of January, when in the evening, without any notice or obvious cause, he began to be restless and delirious.

'On visiting him the next morning, Mr. Elwin found him with an anxious countenance; a frequent and irregular pulse, which occasionally intermitted; his mind wandering; the action of his heart strong, and attended towards the sternum with a loud bellows-sound. The next day his breathing was difficult, and "catching;" the pulse 120, hard and wiry. At that time I had the opportunity of seeing him. He was lying in a sort of stupor, yet not unconscious, for he put out his tongue at my request, and answered pertinently one or two questions, after they had been frequently repeated. He had the air of a person obstinately determined to say as little as possible. He became more distinctly delirious towards evening; and the next day his pulse and breathing were both so frequent (148, and 78, in the minute respectively) that he was thought to be dying. A distinct bellows-sound was audible near the left mamma. This state continued, with slight fluctuations, till the 8th, when his condition appeared somewhat more hopeful. He was calm, had no dyspnœa, and conversed more readily,—saying sometimes that he felt as if he were "dead;" sometimes that he was "burnt up." He complained, for the first time, of pain in the right temple; his gums were slightly under the influence of mercury; his pulse scarcely exceeded 100; the bellows-sound was very manifest.

'On the 9th he again became, first restless, and then violently and wildly delirious, screaming out, refusing to take medicine, or to open his mouth when it was offered; yet he evidently knew what was said to him. During the night, general convulsions came on in occasional spasms, of a tetanic character: in the intervals between them he lay in a state of coma. He survived in this condition till the 12th.

'I was present at the inspection of the body ten hours after death. Some of his family insisted on being in the room with us; but we were able to make an accurate examination of the head, and of the heart.

'The veins of the brain seemed somewhat fuller of blood than is common. The arachnoid was slightly elevated by a clear serous fluid collected in the pia mater. There was but a small quantity of a similar fluid in the lateral ventricles. The lungs appeared quite healthy.

'There was no fluid in the pericardium. Its surface was everywhere exceedingly vascular, but it presented no appearance of lymph, except where it adhered to the posterior side of the heart, over a space of about two inches and a half in length, and upwards of an inch in breadth. The lymph which formed the medium of connexion was firm, but evidently of recent formation; and a very slight degree of force sufficed to separate the adhering membranes. The heart was rather small, and the left ventricle had a singular wrinkled appearance externally. Towards the edge of the mitral valve there was a profuse crop of little wart-like vegetations, of the size of millet-seeds; and numerous red lines converged towards them from the base of the valve. The aortic valves all presented curious festoons of similar excrescences, larger, however, and more prominent, than those upon the mitral valve.

'In the beginning of the year 1832, a girl nineteen years old, Frances Kirk by name, was a patient of mine in the hospital, with acute rheumatism of the joints, and carditis, manifested by many of the most usual symptoms—by pain in the situation of the heart, dyspnœa, great frequency of pulse, and a distinct bellows-sound. She lived two months from the commencement of the cardiac disease. During that period she was at times wildly delirious—at times stupid, taciturn, and almost idiotic—and at times quiet and rational. The brain in that case was found perfectly healthy, except slight serous effusion beneath the arachnoid. The pericardium was everywhere adherent to the heart. By some mismanagement the opportunity of inspecting the inner membrane of the heart was lost.

'In each of the three last-mentioned cases there was more or less serous fluid found in the meshes of the pia mater, and in the lateral ventricles. You may ask, perhaps, whether this effusion was not good evidence of previous inflammation there? whether it did not show that the metastasis, which I have spoken of as seldom happening, really did happen in these very cases?

'I apprehend not; and for the following reasons.—In one only of these cases was the amount of the serous accumulation at all considerable. There was no *other* trace of inflammatory action in any of them; no redness, nor pus, nor lymph; none of the *unequivocal products* of inflammation. What quantity of serous effusion beneath the arachnoid, or in the ventricles of the brain, is requisite to establish its morbid origin—within what limits such effusion may be considered natural—whether it may not be ascribed wholly, or in part, to mechanical transudation after death; these are questions which have not yet been definitely settled among pathologists. For my own part, whenever I see the veins of the pia mater full of blood, I *expect* to find serum between that membrane and the arachnoid. How much of it may have been poured out before death, and how much afterwards, it would be difficult to estimate. In each of the cases before us there was evidence, not to be mistaken, of cardiac inflammation. Now that acute inflammation, fixing itself upon some portion of the heart, should embarrass its action, and modify the condition of the circulation through the cerebral blood-vessels, is not only conceivable, but highly probable. Any retardation of the venous circulation in the head—any engorgement or congestion of that system of vessels —would be likely, if we may reason from the analogy of other parts, to produce effusion. I have seen, in the brain of a criminal who had been hanged while in a state of perfect health, as much serum collected in the same parts as we found in the patients whose cases I have been relating. It is possible that, in them, the disorder of the sensorial functions depended upon simple disturbance of the cerebral circulation; it is possible that the same disorder depended upon the serous effusion; and it is possible, and (I think) probable, that it depended in part upon both these causes. It is very certain that similar symptoms have occurred in similar cases, when there was no appreciable effusion; and, apparently, from mere derangement of the natural circulation of the blood in its vessels. On the other hand, we know that an equal, or a greater amount of effusion, has often been observed, when no such cerebral symptoms had manifested themselves. I conceive, therefore, that the symptoms referable to the brain, and the quantity of serum found effused there (whether these bear to each other the relation of cause and consequence, or not), are both to be regarded as secondary effects of the cardiac disease; that they denoted no inflammatory condition of the brain, or of its membranes, but were the common result of that inflammation of the heart, concerning the existence of which the inspection of the bodies left us no room to doubt.

'That this view of the matter is correct, is the more probable because (as I just now stated) the same symptoms have been known to accompany carditis, although no serous effusion was met with in the head. There was none in the case of the girl Rankin; none in a case related by Dr. Davis; none in a remarkable case detailed by Dr. Latham; none in a striking example of a similar kind which fell under the observation of that accurate and most accomplished pathologist, Andral. I shall take the liberty of citing these two instances.

'"One of the children of Christ's Hospital," says Dr. Latham, "had, in the opinion of all who saw him, the severest inflammation of the brain. The attack was sudden, with great heat and frequency of pulse. He had delirium and convulsions, and pointed to his forehead as the seat of his pain. In three days he died, and, upon dissection, not a vestige of disease was found within the cranium; but the heart was exclusively the seat of the disease, and no other part of the body discovered the slightest morbid appearance. The disease of the heart was not confined to its investing membrane. It was the most intense inflammation pervading the pericardium and the muscular substance."

'Andral's case, which is referred to by Dr. Latham, occurs in his "Clinique Médicale."

'A woman, twenty-six years old, was brought to La Charité, in a state of delirium, and no account could be obtained of her previous condition. The delirium was remarkable for the obstinate taciturnity which attended it. When questioned, the patient turned a fixed gaze upon the person who spoke to her, but made no reply. Her face was pale; her pulse small and frequent. During the two following days the head was frequently drawn backwards, the trunk was shaken at intervals by convulsive movements, and she had subsultus tendinum; but she now spoke, and appeared to comprehend what was said to her, but talked incoherently. The pulse was very frequent, and intermitting. On the fourth day the delirium ceased; she complained of nothing but great debility. The muscles of the face were almost continually agitated by convulsive twitchings, and the arm from time to time presented a sort of tetanic stiffness. On the fifth day the delirium returned; the patient then fell into a state of coma, and died the next morning.

'Neither the brain, nor the spinal marrow, nor their membranes, presented any appreciable morbid appearances. The pericardium was lined with coagulable lymph, and its opposite surfaces were connected, in some places, by recent bands of adhesion. It contained also some ounces of a greenish flaky serum. No other trace of disease was discoverable.

'Now if you are not made aware beforehand of this strange course of the symptoms arising, sometimes, out of rheumatic carditis, you will be apt to overlook the cardiac affection, and to direct your remedial measures wide of the mark. In a second instance mentioned in Dr. Latham's essay, "the whole force of the treatment was directed to the head, from a belief that the brain was inflamed. Upon dissection, the brain and its coverings were found in a perfectly healthy and natural state; and the pericardium, towards which during life there was no symptom to direct the slightest suspicion of disease, discovered the unequivocal marks of recent and acute inflammation." Dr. Davis also, in reference to a case published by him so early as 1808, has the following remark:—"The restlessness in the case of Miss H. C. was also attended with delirium, a symptom not previously noticed as belonging to pericarditis by any writer whom I have consulted. It was so prominent a feature of the disease under which this young lady laboured, as to divert the attention of her medical attendants from its actual seat."

'This occurrence, in the course of rheumatic carditis, of cerebral symptoms calculated to perplex and obscure the true nature of the disease, is probably not so rare as has been supposed. In less than three years three instances of it have fallen under my own notice; and I have been informed by a medical man residing in the neighbourhood, that a friend of his, who has a very large general practice among the middle and lower classes, attended within the last year or two not less than twenty cases of acute rheumatism, in which a metastasis, or an extension, of the inflammation appeared to take place to the brain.

'In all the detailed cases of this kind that I have met with, and in those which I have myself watched, there were certain general points of similarity which you will do well to bear in mind. In all of them the pulse was extremely rapid; the delirium, though violent and active at intervals, was characterised for the most part by a singular, and, as it seemed, perverse taciturnity; even when the patient was evidently able to speak, and understood the questions that were put to him, he maintained a sullen silence. In most of these patients, also, not long before the fatal event, a brief interval of amendment took place, and encouraged some hope of recovery. In many of them various convulsive movements were observed; and in two of the cases the head symptoms, and probably the heart disease also, supervened after a *relapse* of the rheumatism of the joints.'

It is a curious and instructive circumstance that rheumatic carditis is sometimes the first step in the whole disease. The cardiac symptoms do sometimes, I mean, precede those of the joints; even by two or three days. For example.—A lad was brought to the hospital with acute articular rheumatism, and with unequivocal symptoms, which I need not detail, of carditis. He gave the following distinct history of his illness:—He had been on a visit into the country several days before, and there, after having felt poorly for nearly a week, with a sensation of 'sinking within him,' he ate largely of oysters, and drank more porter than he was accustomed to. On the same day he was seized with pain in the left side of the chest, and violent beating of the heart. The attack was probably a severe one, for he applied to a medical man, who immediately bled him. In the course of the ensuing night he began, for the first time in his life, to feel some stiffness beneath and about his knees, but he was able to walk about the next day. On the evening of the second day the joints became so painful and swollen, that he could not leave his bed, and the pain of the side and the palpitation diminished. This boy has several times since returned to the hospital with acute rheumatism, and on each occasion presented manifest indications of some permanent affection of the heart,—slight, probably, in amount, but aggravated upon every return of inflammation of the joints.

I have met with one or two other instances in which the cardiac disease appeared to have preceded the arthritic; but none so well made out as that which I have just related. In the 'Edinburgh Medical and Surgical Journal' for 1816, Dr. Duncan gives a case of 'inflammation of the heart' which *began* with symptoms of *pectoral inflammation*, succeeded *the next day* by rheumatic affections of the joints. Dr. Fuller, in his excellent book on Rheumatism, states that three instances of this sort have fallen under his own observation: and he refers to several others, recorded by various writers.

One law respecting the connexion between the cardiac and the arthritic symptoms may be stated with confidence, namely, that the *younger* the patient is who suffers acute rheumatism (and I have seen it so early as the third or fourth year), the more likely will he be to have rheumatic carditis. The chance of the combination appears to diminish, after puberty, as life advances. I have known only three persons pass through acute rheumatism with an untouched heart prior to the age of puberty; and in two of these I am by no means certain that the articular disease was genuine rheumatism. In each of the two, the large joints became painful, and swelled, for a day or two only, towards the close of scarlet fever:—a circumstance not unusual. I was dreadfully apprehensive of carditis, but it did not occur.

I have observed, also, that when a patient has come under my care who has had *repeated* attacks of acute rheumatism, in him I have generally found reason to believe that some organic affection of the heart was present. Probably the disposition to such repetitions of the disease, so remarkable in some individuals, may be kept up by the cardiac complication.

With respect to the period of the actual attack, and the circumstances under which the extension of the disease to the heart occurs, no fixed law has been observed. Sometimes the cardiac affection declares itself as the inflammation of the joints declines. Quite as often, however, they proceed together, and are aggravated or mitigated simultaneously. On this point my own experience nearly agrees with that of Dr. Latham, who says:—'It (the cardiac affection) is incident to all the degrees and all the stages, and all the forms (?) of acute rheumatism. It is not more to be looked for when the disease is severe than when it is mild; more at its beginning than during its progress and decline; more when it is shifting and inconstant in its seat, than when it is fixed and abiding.'

There are some other symptoms that I must not omit to mention as occurring in some cases of pericarditis. In one of the fatal instances which fell under my own notice, there was a very strong purring tremor felt by the hand placed upon the region of the heart. This is not a constant, nor even a frequent, symptom; but it has a certain degree of corroborative value when it does occur.

When the fluid products of the inflammation predominate, when there is much serum poured out, the symptoms, as well as the danger, will be different from those which are remarked when there is not so much serous liquid. If the pericardium be distended, percussion, as I have already said, will furnish a dull sound over an unusually large space; much beyond the natural limits of the præcordial region. But the general symptoms will vary also. The pulse will be feebler, and more disposed to falter, and to become irregular, in proportion as the liquid effusion is large; and at the same time the patient will frequently be fixed in one position, and unwilling or afraid to change it, lest that small exertion should further excite the action of his heart, and hurry his respiration. He will lie, perhaps, always upon one side; or he will remain immoveable on his back, with his head elevated; or he will sit up continually, with his body leaning forwards; and he will not dare to alter his posture. But when the solid products of the inflammation predominate; when there is coagulable lymph, and but little serum; when the pericardium, instead of being distended, becomes attached to the heart; then the pulse will retain that force and regularity with which the disease commenced, the dull sound yielded to percussion will not transgress its natural limits, and the patient will not in general experience any absolute necessity of accommodating his body to one constrained position.

Of a merely adherent pericardium there are no diagnostic signs to which, so far as I know, we can *trust*, either auscultatory or general. None, I mean, presented by the body at the time. If we are accurately acquainted indeed with the history of the patient's disease, and if we know that, at any time, a *to and fro* sound existed, which *to and fro* sound soon ceased, and has never recurred; then our conclusion that the pericardium is adherent will scarcely be open to any source of fallacy.

When the opposite surfaces of the membrane have been once united, they never separate again; the adhesion remains for life. But the lymph interposed between them, if the inflammation be not renewed, becomes less and less thick; until at length, in some cases, a mere layer of firm, but thin, areolar tissue is left, through which the heart is visible.

But when inflammation has stiffened the valves of the heart, or studded them with little wart-like masses of fibrin, or rendered the lining membrane of its chamber thick and opaque, how far do these morbid states admit of perfect recovery? It is not easy to say. I am not aware of any facts which would forbid altogether the hope that here, as in iritis, the re-absorption or removal of the lymph may sometimes be total, and the restoration of the parts complete. On the contrary, the *comparative* infrequency of wart-like deposits in the slowly fatal cases of rheumatic carditis leads to the opinion that such deposits may disappear as readily and entirely from the valves of the heart as from the iris: and the success of remedial measures directed against recent hypertrophy, dependent apparently upon chronic or subacute inflammation of the endocardium, is corroborative of this opinion.

One perilous way in which these little wart-like bodies may be removed I have several times adverted to. Portions of them, large or minute, may separate from the subjacent membrane, or be detached and washed away by the current of the blood. And serious, and even quickly fatal consequences, may result from this accident (for so it may be called) of endocardial disease. A fragment of fibrin, thus carried along in the circulating blood, may stick in some blood-vessel which is too small to permit its further progress. If one of the cerebral arteries happen to be thus suddenly plugged, that portion of the brain to which its branches are distributed is deprived of much, or all, of its nutrient blood; white softening ensues, and consequent palsy. Dr. Kirkes, to whose sagacity we are indebted for almost all that we know on this interesting subject, has detailed, in the thirty-fifth volume of the 'Medico-Chirurgical Transactions,' some well-marked examples of this disorder. Analogous evils may follow the blocking up of a considerable artery in other parts of the body. I will mention one instance that I have lately seen. A sickly girl, in her fifteenth year, was attacked with acute articular rheumatism about Christmas-time, in the year 1854. She was attended by Mr. William Squire, who carefully and constantly examined the condition of her heart. No evidence of its implication occurred for the first ten days of the disease. Then began a mitral murmur, and with it signs of pulmonary congestion. Five or six days after this, an aortic murmur also became audible; and nearly at the same time her right leg, from the hip downwards, turned suddenly cold, without any loss of power, or of sensibility; and it was found that all pulsation had ceased in the right external iliac artery and its branches. She spoke of slight pain in the inguinal region on that side. Presently vesicles, containing a dark fluid, formed upon the great toe, and upon the third toe, of the right foot. Fear naturally arose that the whole leg would mortify; and the girl's life was despaired of. By great care, however, by lapping the limb in wool, and by supporting her strength, she was carried safely through that cold winter. By degrees the temperature of the leg and thigh were nearly restored; but no pulsation could be felt in the larger arteries when I saw her with Mr. Squire in October, 1855. A rough mitral bruit was then very plainly to be heard both in front and at the back of the chest; and also a less distinct aortic bellows-sound. The girl died in the following May. Unfortunately permission to examine the limb could not be obtained: but I cannot doubt that its main artery had been sealed up by solid matter, derived from one of the cardiac valves.

Minuter portions of fibrin thus detached from the lining membrane of the left side of the heart, may pass unchecked through the arteries, yet be entangled and stopped in the capillary vessels, and lead to changes, transitory or permanent, in the affected tissues. You will call to mind what was said of the possible agency of a *spray* of this kind, in producing some cases of chorea. It is in this way that many of the yellowish or buff-coloured masses of fibrin originate which are familiar to morbid anatomists as being of frequent occurrence in the spleen and in the kidneys.

After a similar manner branches of the pulmonary artery, or parts of the pulmonary capillary system, may be obstructed by portions of fibrin proceeding from the *right* chambers of the heart. The primary effects of these dislodgments of fibrin from the interior of the heart are mechanical: but it is very conceivable that the whole mass of the blood may in certain cases be contaminated by the admixture of some of the fluid products of endocardial inflammation.

Since I lectured upon this subject last year, two examples of rheumatic carditis have occurred among my hospital patients, differing in some remarkable points from any that I have ever seen. A brief description of them will complete my personal experience of this terrible disease.

The histories of the two cases are curiously similar. The patients were young women; their ages respectively twenty-one and twenty-two. They were admitted during the same week; one a day after the other. Both were suffering under a first attack of rheumatic fever: both had also acute pleurisy, with effusion into the chest; and both died; one of them three weeks, the other a month, after her admission. In both cases there were symptoms referable to the heart: pain, and unnatural sounds; but in neither case was there any friction-sound; nor were any traces of pericarditis discovered after death. But the inflammation had fallen, partially, upon the aortic valves; whence it had extended (so I imagine) to the muscular substance. I show you the morbid appearances represented in these drawings, made by Mr. Lonsdale at the time: and, better still, I show you the parts themselves, which are preserved in our museum.

The whole of one cusp of the aortic valves was, in each case, a mass of ragged ulceration; and the adjacent portions of the two other cusps were, in a slighter degree, implicated in the mischief. What remained of the tattered valve was encrusted with rough irregular shreds of lymph, or vegetations. In one of the cases, the ulcerating process had penetrated through the valve, and into the muscular substance beyond, and had eaten a hole completely through the septum. A portion of lymph protruded just below the valves of the pulmonary artery through the channel of communication thus formed between the left and right sides of the heart. In the other case, an abscess as large as a hazel-nut was found in the muscular substance of the septum, immediately opposite the disorganized valve.

Suppuration in the heart is very rare. With such mischief in rapid progress within the heart, it is easy to see how the blood may be polluted, and charged with a new poison in its very fountain. In these two instances, the cardiac affection was complicated with acute pleurisy. I should have mentioned before, that the pleura very often participates in the inflammation when pericarditis occurs. You will not wonder at this if you consider the close vicinity, and the similarity in texture, of these two serous membranes. Nor will you be surprised when I add, that the pleurisy is often associated with pneumonia also. Upon these accessory disorders the immediate danger of the case not unfrequently hinges.

With respect to the comparative frequency of these various complications of acute rheumatism, I may state briefly the result of Dr. Latham's computed experience, which is in general accordance with my own. Of those who suffer acute articular rheumatism, not less than two-thirds suffer also some form of cardiac inflammation. Of these forms, endocarditis is the least formidable, and much the most common; occurring nine times as frequently as pericarditis. Again, it is with the rarer, and at the same time the most perilous form of cardiac inflammation—with pericarditis, or with pericarditis and endocarditis combined—that pulmonary inflammation (including bronchitis, pleuritis, and pneumonia) is most apt to be associated.

I must defer what I have to say respecting the treatment of acute pericarditis and endocarditis, to the next lecture.

LECTURE LVI.

Treatment of Acute Pericarditis, and Endocarditis: blood-letting; mercury; blisters. Chronic and partial Inflammation of the Pericardium. Disease of the Aorta. Thoracic Aneurisms; their various situations, and symptoms: plan of treatment.

I TRUST that I made distinctly apparent, in the last lecture, the great danger which belongs to every case of acute inflammation of the pericardium. First, there is the danger of *speedy death.* When the inflammation results in liquid effusion, and the collection of serous fluid is so large as greatly to distend the pericardium, the action of the heart is oppressed by the liquid surrounding it, falters and flutters, and at length stops, and goes on no more. Secondly, there is the danger that (the pericardium having become adherent) other structural changes may soon, or slowly, develop themselves; and first render life burdensome and full of suffering; and then consign the patient to an earlier grave than might else have awaited him. Again, if the endocardium alone be affected, there is the danger of such permanent valvular damage as may obstruct the onward current of the blood, or destroy the natural and necessary bar to its reflux: and therefore the danger of gradual hypertrophy and dilatation, with all their distressing consequences. There is also the further risk, that the seeds of disease may be conveyed to other organs of the body, from the interior of the heart, with the circulating blood. When both the lining and the investing membrane of the heart are involved in the morbid process, the hazard is obviously doubled.

Now what can we do to prevent, or to diminish, these evils? I once thought that if we caught the inflammation at its very commencement, we might hope for a perfect cure, from first bleeding the patient freely, and from, secondly, putting him as speedily as possible under the specific influence of mercury. But I am compelled to say, with deep regret, that the more I see of this formidable malady, the more reason I find for fearing that it is seldom within the possibility of thorough repair. Bring the inflammation to a stop, you perhaps may; or nature will do it for you; and you may greatly assist the natural powers in effecting this. But that alone can be called a cure, which either leaves the structure of the part affected in its original integrity; or, at any rate, leaves no spring or source of further changes for the worse: and such complete recovery as this I seldom dare to hope for, in cases of acute and general carditis.

There can be no use in deceiving ourselves in this matter; but we may very easily deceive ourselves. In a large proportion of cases, whether they be treated well, or ill, or not treated at all, the patients will *seem* to recover. But I say that the recovery is so far unreal, that it involves the germ of future destruction. If any of you have read Bouillaud's heavy, yet instructive, work on diseases of the heart, you will know that he boasts of the success of his treatment in acute pericarditis. He declares that by the bold use of the lancet he *extinguishes* the inflammation; jugulates (as he calls it) or slaughters the disease at its birth; and restores the patient to the full condition of health, or to the state in which he was before the disease came on. You must hereafter judge of this question for yourselves; but it is my duty to caution you against crediting these statements. Not that I would insinuate a doubt of M. Bouillaud's veracity; but I believe that he has been deceived by false recoveries; and I would not have you beguiled, by his representations, into the indiscriminate adoption of that 'enlightened hardiness' which he endeavours to inculcate.

But if we look closely at his statements, we do not find, after all, any such wonderful success. Of 18 patients, 6 died: a very large proportion, 1, viz. in 3. To be sure, with some ingenuity he makes the proportion to be 1 in 7. For three of the fatal cases occurred, he says, before he took to his heroic plan of blood-letting; and excluding these 3, he has 15 cases, and only 3 deaths; or 1 in 5. But one of these three proved fatal from the supervention of tetanus; therefore setting that also aside, there will be 14 cases of the disease and two deaths. Now, I have not, hitherto, been able to look through my case-books in reference to this point, but I am quite certain that the mortality in the Middlesex Hospital has been nothing like so great as that—the *immediate* mortality, of course, I mean—either among my patients, or among those of my colleagues; and I know that, until within the last twelve months, Dr. Latham had not lost a single case of rheumatic pericarditis, in the course of the first attack of that disease, for several preceding years.

But what I most doubt about, is the *real* recovery of Bouillaud's surviving patients. I say such patients do *apparently* get well. In some of them, indeed, a bellows-sound remains, sufficiently indicative of the damage that the organ has sustained: and I have already told you that *any* amount of change, however small, which alters the healthy proportion between the cavities and their outlets, or which interferes with the natural play of the heart, is a seed from which further changes will at length be found to grow. But patients will get so far well that you can detect nothing wrong about them. Follow them, however, in their subsequent lives; and you will learn that many of them very soon begin to find that they are incapable of doing or enduring all that they could do or endure before their illness: and if this do not soon happen, it happens at last. The disease of the heart (if the patient be not cut off by some other malady) becomes at length obvious: and when he dies, the source of the ultimate changes is commonly to be detected. There is adhesion of the pericardium; or there is disease of the valves; of which no other account can be given than that these had continued to exist since the primary symptoms of carditis ceased; and had caused all the rest—the hypertrophy, I mean, the dilatation, or the wasting.

The remarks that I have now been making bear upon the question of blood-letting in acute pericarditis. If we may ever hope to achieve a complete cure by the early abstraction of blood, it must be in those cases in which strong tokens of the presence of the disease declare themselves—(palpitation, to wit, pain expressly felt in the region of the heart, tenderness in the præcordial interspaces, frequency of pulse, distress and anxiety referred to the epigastrium, *all springing up together in the course of an attack of acute rheumatism*)—and yet no attrition sound or other murmur can be detected. But if the *to and fro* sound have once been heard, the consequences of the inflammation can never be abolished by blood-letting. The best event which can then happen is adhesion. Nevertheless, unless peculiar circumstances forbad, I should always take blood by leeches from the præcordial region; but I should never think of taking it in the lavish manner recommended by Bouillaud. I know that his treatment has been fairly tried in this country, and has failed. And a peculiar risk may attach to the induction of syncope in this affection. There is, almost always, endocarditis (in the rheumatic cases at least) coincident with the pericarditis: and the observed tendency towards a deposit of the fibrin of the blood, in the shape of minute granules, upon the inflamed valves, would perhaps be promoted by retardation of the blood; and still more by its temporary stagnation. In the experiments on the ass, referred to before, the circulation (kept up by artificial breathing) became languid and sluggish, and granulations were deposited upon those valves which had been irritated by the wire. Hence there is, possibly, a danger in bleeding to such an extent in these cases, as to bring the heart's action to a pause *in deliquium*. But no objection of that kind lies against the application of leeches, which gives unequivocal and immense relief to præcordial distress when that happens to be present and urgent. Bear in mind the arterial connexion which subsists between the surface here, and the deeper parts beneath it. The internal mammary artery feeds both the pericardium and the præcordial integuments. The suction of leeches applied over the site of the heart *abstracts* blood from the superficial branches, and thereby and in the same proportion *diverts* blood from the deeper pericardial branches of the same artery. Its strongest known curb is thus placed upon the local inflammation. The inflamed part is *pro tanto* eased of its load; and this diversion of the blood is calculated and likely, when effected sufficiently *early*, to abate the intensity, to limit the extent, to shorten the duration, and to lessen the products of the inflammation. From a series of carefully observed cases it was found, by the late Dr. John Taylor of Huddersfield, that the duration of pericarditis increases as the number of days increases between the commencement of the disease and the first abstraction of blood. The duration of the cases in which blood was taken away after the first four days was greater by *one-half* than of those in which that measure was adopted before the same period.

The amount, and the repetition of this topical blood-letting, must be regulated in accordance with the progress and symptoms of each particular case. You will not persist in repeating it if you find that the pericardium becomes distended with fluid. Your object then will be to get rid of the fluid. And you may sometimes effect that object with marvellous rapidity by laying a large blister over the heart. In the early stage of the disease, for reasons heretofore given, blisters are to be avoided. After the leeches, warm fomentations or poultices are at that time the proper applications. In the one case we are combating the inflammation, in the other we are dealing with the products which the extinct inflammation has left behind it. The only disadvantage theoretically contingent upon blistering the surface for the removal of liquid effusion, is that the cantharides may sometimes produce strangury, and so bar, for a time, one main channel of elimination of blood impurities. In all stages of the malady rest is imperatively to be enjoined; and when liquid effusion is manifestly present in appreciable quantity, the patient should be cautioned against any sudden change of posture, and especially against rising suddenly from the recumbent to the erect position, lest fatal syncope should follow. So also any posture should be avoided which has already been found to cause much disturbance of the heart's action, or of the breathing.

Diuretics are fit medicines when the pericardium is distended with fluid; and it will often be expedient and necessary to give stimulants, in order to sustain the oppressed and flagging heart: wine, or brandy, as well as good nourishing diet. Fortunately in *rheumatic* pericarditis, the general treatment of the whole disease, which I shall speak of hereafter, will go hand in hand with the local.

Hitherto I have said nothing of the employment of mercury in this disease. If—what perhaps never happens—the pericarditis should seem to have any dependence upon the syphilitic poison, then I should press the use of mercury. But in this case, as in many others, the hope which I once cherished that the inflammation could be controlled by the constitutional influence of mercury has faded away. Pericarditis has been known, not seldom, to spring up while the patient was already under mercurial salivation. I am obliged, therefore, to recant the advice which I was formerly in the habit of giving in respect of mercury as a remedy for acute pericardial inflammation. I recommend you to abstain from giving it with the view of obtaining its peculiar effects upon the gums and the general system. But I should never scruple to prescribe it with other aims; and especially for its tendency to quicken and promote the action of some diuretic remedies.

If the patient should be in danger of perishing from the amount of the pericardial effusion, the *ultimum remedium* of tapping the distended bag must be adopted. I have never seen this done, nor met with any case in which it appeared to be demanded: but it has been done, always with great present relief to the patient, and in some instances with most gratifying success. The puncture should be made with a very fine trocar, and the fluid let out very gradually, but completely: and the best place for introducing the trocar, after a preliminary incision of the skin—supposing the heart to be in its natural place—

is, as Dr. Walshe instructs, at the upper angle of the fourth left costal interspace, or a little lower than this, the patient lying on his back.

Even when all the symptoms have departed, previous disappointments have taught me not to be sanguine as to the permanency of the recovery. I believe that months, and years even, may elapse before the secondary effects of the mischief left behind by the inflammation begin to be palpable. But in many instances they show themselves very early. Others have noticed all this; especially Dr. Latham, who truly remarks, that 'in acute pericarditis there is no medium between complete cure and certain death.' He deemed, at the time when the lectures from which I quote were given, that the early and vigorous use of mercury might be equal to the complete cure. But in his more recent work, 'On Diseases of the Heart,'—in which the whole of this subject is most instructively treated in detail, and which I cannot too strongly commend to your diligent study—I find, with a melancholy sort of satisfaction, that his final belief is in no respect different from my own. I have several times already expressed my conviction that when the *to and fro* sound has manifested itself, that is, when the inflammation has gone so far as the effusion of coagulable lymph, if the patient do not die outright, he survives at the expense of an adherent pericardium; and he survives only for a time. *Hæret lateri lethalis arundo.* Should he again suffer acute rheumatism, he may again have heart symptoms. Not, of course, the *to and fro* sound; but pain, palpitation, and dyspnœa. Now it is of some practical importance to be aware that this renewal of morbid action does not require the active treatment which the primary inflammation demanded. The effect of such renewal will be to augment the existing mischief; but the morbid process is much less vigorous, and much more easily subdued. It will generally yield to the application of a few leeches over the situation of the heart, in addition to the constitutional treatment.

Although acute inflammation in this, as in other serous membranes, shows generally a strong disposition to spread all over the affected surface; yet does the pericardium seem readily susceptible of slight and partial inflammation. Upon laying the bag open, you may frequently observe small white streaks or patches of variable size and shape, scattered irregularly over the surface of the heart. I have examined many of these patches; and I believe they mostly consist of a thin flake of lymph lying sometimes beneath, but oftener upon, the serous membrane. They may, in fact, be peeled off sometimes, and the subjacent membrane be left smooth and sound. On one occasion, I met with a long riband of lymph passing from the centre of one of these white spots, to connect itself with the loose bag of the pericardium. I conclude, therefore, that these spots are really the result of a very limited inflammatory process: but under what conditions they arise, or whether during their formation they furnish any symptoms, I do not know.

Such is the view which I had long taken, and taught, of these white spots upon the surface of the heart; and I am glad to have it confirmed by the observation of Mr. Paget, who has adduced (in the twenty-third volume of the 'Medico-Chirurgical Transactions') conclusive evidence, both of their frequency, and of their inflammatory origin.

It is supposed, however, that a certain milky whiteness, very commonly to be seen after the middle of life upon the anterior surface of the heart, of about the bigness of one's finger nail, and of indefinite outline, differs from the other spots, and is *not* of inflammatory origin, but results from mere opacity of the membrane, from which it cannot be separated.

Strange fancies have been entertained about this familiar white spot. It has been called the soldier's spot, and attributed to friction produced by the bending posture of the men in marching under the load of their knapsacks. But the spot is in no way peculiar to soldiers; nor, if it be independent of inflammation, have I any plausible explanation to give you of its source.

You may ask me whether inflammation of the pericardium, even when it is slight and partial,—such as might account for these spots—can ever take place without giving rise to a friction sound. It is highly probable that it cannot. Yet as such inflammation is attended with no febrile disturbance or general distress, and probably with no severe or abiding pain, or no pain at all, it is not brought under the scrutiny of the physician, and may be scarcely noticed by the patient himself. So that the friction sounds are not heard, because they are not hearkened for. In this respect the formation of the white spots on the heart has some analogy with the formation of those pulmonary adhesions, which are such common results of dry and partial pleurisies.

I have spoken of acute pericarditis as it presents itself in frequent association with articular rheumatism; and I have shown you how fearfully serious a character is imparted by that complication to a disorder which, however painful, is otherwise devoid of danger; and I have told you that the same complication arises sometimes during the progress of the febrile exanthemata, and especially of scarlet fever. In both cases there is a blood-poison at work: in both cases the course, the clinical history, the gravity, and the appropriate treatment of the supervening disease, are essentially the same. And the patients are of the same class, being mostly young, or not old, and of previously sound health.

But inflammation of the pericardium is exceedingly common under very different circumstances; in persons advanced in life, and with constitutions broken by previous disease. We trace it, often unexpectedly, in its effects—in the presence of lymph recently effused, and smeared over the surfaces of the membrane—after death by various chronic maladies, and above all, after death from that renal malady which has given to the name of Bright an immortality of reputation. In that disease also the blood may well be said to contain a poison. The frequency of this insidious form of pericarditis at the close of other diseases was fairly brought to light by the laborious and accurate researches of the late Dr. John Taylor. Its comparative lack of interest has been well weighed and set forth by Dr. Ormerod. The importance of the pericardial inflammation in these cases is practically but little. Dr. Ormerod truly observes of it, that it falls more within the province of the morbid anatomist than of the physician. Its symptoms are so slightly and uncertainly marked, that the fact of the inflammation is often recognised only in the corpse. It accompanies, rather than causes, death. It does not destroy life, but takes place because the patient is already dying. Other serous surfaces are apt to suffer inflammation under precisely similar circumstances. This kind of pericarditis scarcely submits itself to treatment. It is proper that you should be aware of its frequency, and of its nature: but it requires no further consideration in these lectures.

So much then for the heart itself, and its membranes. There still remain to be considered the morbid conditions of the great vessels that spring from it, and lie in the thorax, and especially of the aorta; those morbid conditions, I mean, which declare themselves by symptoms, and which become the object of medical treatment.

The aorta is very frequently indeed found diseased, and its disease, as I have already explained, is a common cause of organic changes in the left ventricle of the heart. You will find that its interior surface, instead of being smooth, and of a uniform yellowish white colour, is rendered very uneven by a great number of yellow opaque projections, of cartilaginous consistence, lying immediately beneath the membrane. And in a more advanced stage of the same diseased condition, you may perceive that some of these projecting little masses consist of irregular scales of bone, having sharp edges; and sometimes these plates of ossific matter are quite bare; the inner membrane is gone, and the exposed bone is washed by the current of blood. Now the necessary effect of these changes is to diminish and destroy the natural elasticity of the vessel; and as there is a perpetually recurring strain upon it, by the blood sent out from the heart, the vessel dilates, becomes larger than it should be. This, if you please, you may call aneurism; but a simpler name is dilatation. In other cases, the enlargement is not general, but partial. A pouch is formed on one side of the artery, and this pouch may be very small or very large. It appears to result from the giving way, the rupture in short, or the ulceration, of the inner and middle coats of the artery, and then the blood, passing through the broken part, presses against the cellular coat of the vessel, and distends it into a sort of bag. There have been curious discussions as to what should be called true aneurism, and what should be called false aneurism; discussions upon which I have neither time nor taste for entering. It is enough for all practical purposes to state, that the artery sometimes dilates only, sometimes throws out a pouch. I know that you have received, or will receive, from my colleague Mr. Arnott, all the information that is requisite concerning the modes in which aneurism may arise. These are matters of the highest interest in surgery, for surgery can cure an aneurism; an achievement which is but seldom within the skill of physic.

I have known three or four cases in which the diseased artery all at once cracked across—its inner and middle coats, I mean—and death very rapidly ensued. In one of these instances, the crack extended round a considerable part of the circumference of the aorta. It looked exactly like a clean cut made by a sharp knife. The blood, in this example, dissected its way (if I may so say) between the middle and external coats of the aorta, and got at last into the pericardium, and coagulated round the heart in a uniform layer: so that a *bag* of coagulated blood was enclosed in the bag of the pericardium. In another case, which I mentioned in a former lecture, the dissecting blood shut up the right carotid artery, and fatal hemiplegia from white softening of the brain was the result. Of course, nothing can be done for such cases as these.

Most commonly the aneurismal tumour goes on enlarging: and often it becomes lined, and sometimes it is nearly filled up, by layers of coagulated blood, which form in its interior. At length the tumour bursts, and the patient perishes.

Aneurisms of the thoracic aorta are met with chiefly in the earlier portions of that vessel, in its ascending part, and in its arch. There seem to be two reasons for this. One is, that the diseased state of the coats of the artery (to which the rupture and subsequent aneurismal pouch, or the dilatation, as the case may be, are owing) is more common, and more advanced generally in that part of the aorta; and another reason is, that the momentum of the blood, as it is forcibly propelled from the left ventricle, is sustained chiefly by the same part.

Mere disease or dilatation of the commencing aorta constitutes, as I formerly endeavoured to explain, a physical impediment to the due emptying of the left ventricle. It is a common cause, therefore, of hypertrophy and dilatation of that ventricle; and consequently, the *signs* of hypertrophy and dilatation of the left ventricle of the heart will at length result from disease and enlargement of the aorta near its mouth.

When aneurismal pouches form, as they often do, at the very entrance of the aorta, or in the coronary arteries, they often defy detection. I, at least, know of no sign of their existence upon which a physician can rely, or which can lead him even to suspect such a state of matters. But all at once the patient drops down dead: and upon searching for the cause of this sudden extinction of life, you find the pericardium distended with blood, and the source of that blood you find to be the ruptured aneurismal pouch, so near the root of the aorta, as to project *within the pericardium.* In the preparation which I hold in my hand, an unbroken aneurism actually bulges into the *right ventricle* of the heart.

When the aneurismal tumour occupies a portion of the ascending aorta a little more distant from the heart, or is formed at the transverse arch itself, it sometimes attains a large size, and the evidence of its presence is derived from the effects its enlargement produces on the surrounding textures; and these effects are apt, for a while, to be obscure and equivocal, until an external pulsating swelling makes its appearance, or a sudden gush of arterial blood through the mouth discloses the true nature of the malady.

In these cases we *infer* the existence of aneurism sometimes from peculiar symptoms. Aneurism at the arch of the aorta may come to press upon the trachea, and impede the breathing; or by its effect upon the recurrent nerves, it may cause a very accurate mimicry of laryngitis, producing raucous voice and stridulous inspiration. The operation of tracheotomy, as I told you before, has more than once been performed, to relieve the supposed inflamed condition of the larynx, while the sole disease was aneurism at the arch of the aorta. Such mistakes are always discreditable; and the lesson they furnish should not be lost upon us. An aneurism may come to press upon one or the other bronchus—most frequently it is the left—and so impede the entrance of air, and render the respiratory murmur over one side of the chest comparatively feeble. Whenever we find that a wheezing dyspnœa has gradually arisen, which no apparent affection of the air-passages satisfactorily accounts for, and the patient has a sense of pulsation within the thorax, we may suspect that an aneurism is at the bottom of these symptoms.

The effect of aneurismal enlargements of the artery in causing *absorption* of the neighbouring tissues, upon which the tumour presses, is very curious. You know that even the solid bone is removed, worn away as it were, before an advancing aneurism. Hence it not unfrequently happens that the trachea, or some of the larger bronchi, are at first flattened, and then give way; the aneurism breaks into the air-passages; and the patient, overwhelmed by a torrent of blood into and from his lungs, perishes in a few seconds. Or the tumour may contract adhesions with the pulmonary tissue, and destroy it to a certain extent, and *so* cause mortal hæmoptysis. But such cases are not always *fatal at once.* Not unfrequently the blood bursts into the sac of one or of the other pleura.

Again, according to its situation and extent, an aneurism of the thoracic aorta may press upon the œsophagus, and cause the ordinary symptoms of stricture of that tube. Hence cardiac disease, and pulsation within the chest, accompanied at length by the signs of a constricted œsophagus, form strong presumptive indications of the existence of an aneurism; and in such cases, the œsophagus may at last ulcerate through, and then copious and fatal hæmorrhage ensues. Hæmatemesis it may be called, though the blood is vomited not from the stomach, but from the gullet. A patient in the Middlesex Hospital, with symptoms of stricture of the œsophagus, one day brought up from his throat a red mass, which, at the moment, was supposed to be a bit of meat that he had been trying to swallow. It really was part of the clot from an aneurism; and it was speedily followed by a stream of red blood, and by death.

Again, aneurism of the thoracic aorta does frequently obstruct, by its juxta-position and pressure, the vena cava superior; nay, it may even obliterate that vessel, of which I have seen two instances. To one of these cases I alluded before, as a most curious example of dropsy. It illustrated exceedingly well the effect of venous obstruction in causing serous effusion. The man was a patient of Dr. Hawkins'. He presented a most extraordinary spectacle. His face, neck, and arms were tumid and anasarcous to an enormous degree; while there was not the least trace of swelling or œdema anywhere below the ribs. He looked as if his upper half had been stuffed; and except that it was distressing, his appearance was extremely comical. His countenance was livid; his eyes seemed starting from their sockets, like those of a person affected with exophthalmic goître; and even the areolar tissue beneath the conjunctiva was œdematous. The integuments of his neck and chest were quite brawny; and his arms were so swollen that they projected from his sides. The surface of the thorax in front was embossed by numerous veins, which were turgid with blood; and here and there patches of ecchymosis were visible. You may form some notion of the degree of mechanical congestion that existed, when I tell you that, upon the scarificator being applied, after a cupping-glass was taken off, upwards of twenty ounces of blood escaped in two minutes. The epigastric veins were visible and tortuous, and a free communication by anastomosis existed between these veins ascending from the inguinal region, and the mammary veins. There was a bellows-sound, which increased in loudness and harshness, from the root of the aorta to the top of the sternum. The patient soon died; and a large aneurism of the aorta was laid open by lifting up the sternum, to which the artery had adhered, and into which, indeed, it had eaten a little. Not far above the right auricle, the vena cava was totally impervious; its sides having been gradually pressed together, as the tumour grew. The other case, of the same kind, which occurred in one of my own patients, I shall have occasion to refer to hereafter.

But aneurism of the thoracic aorta, and especially aneurism of its descending portion, may exercise its pressure in another quarter, and wear away the bones of the vertebræ, and cause pain in the back, radiating often in the direction of the intercostal nerves, and ultimately palsy perhaps of the parts below that portion of the spinal cord; so that pain in the back, with or even without a sense of pulsation, may justly awaken suspicion of aneurism making its way backwards. I remember hearing the late Dr. Farre describe a case of this kind, to inculcate the necessity of paying attention to the *sensations* of a patient. A man came to him for advice, having been told by another physician that there was nothing the matter with him—that he was fanciful. But when an adult person makes constant complaint of certain morbid feelings in a part, the probability is that he *has* something the matter, and we must investigate the case with what helps we can get. In the instance in question, there were two signs of disease, and two only; a white tongue, and pain in the back. The whiteness of the tongue soon disappeared under the use of some medicine addressed to the digestive organs. The pain in the back remained. Dr. Farre interrogated his patient minutely every time he visited him, till at last the man got vexed and tired, and said, pettishly, 'I know that if you split me down the middle, I am sound on my right side, and diseased on my left.' Very soon after, he was found dead in his bed. What he had said was perfectly true: there was an aneurism pressing on the left side of the dorsal vertebræ.

Another consequence of an enlarging thoracic aneurism, sometimes observed, is pressure upon the *thoracic duct*, causing engorgement of the absorbent vessels and glands, and inanition. In short, whatever parts the aneurism may reach, and subject to its pressure, may have their function thereby suspended or disturbed, or their structure spoiled.

One sign, which I have myself frequently verified, of aneurism of the arch of the aorta, is a difference in the force of the pulse in the two radial arteries. The pulse in the one wrist (more commonly the left) will be extremely feeble, or even disappear. This happens when the state and position of the arteria innominata, or of the left subclavian artery, become altered in consequence of the enlargement of the aorta; and sometimes the one or the other of these arteries is completely closed up. But inasmuch as a similar difference of the pulses may arise from other causes, we can only look upon this symptom as one which may help to solve an ambiguous case. To give you an example of a difference in the beating of the arteries in the two wrists from other causes, I may mention a case in which the subclavian artery was thrown forwards, and compressed, by an exostosis on the first rib. The case is related in Mr. Mayo's Pathology. It occurred in a patient of mine, the husband of a nurse in my family. I had a girl for some time in the hospital, in one of whose arms no artery could be found to pulsate. Why, we none of us could make out.

Some time ago, a surgeon from the country came to my house, desirous (he said) to consult me about a sense of discomfort in his head; and particularly about the state of his vision. When erect, he saw things obscurely. At three yards' distance he could see my face, but could not distinguish the separate features. What he thought very strange was that he could see perfectly well when in the horizontal posture.

On my proceeding to feel his pulse, he said, in a careless manner, 'By-the-by, that is another thing wrong with me; I have no pulse.' Nor could I detect any, in either arm. He then told me that, four or five years previously, a medical friend, intending to feel his pulse in the left wrist, could find none. He was confident that pulsation had existed a short time before that. After a while, the movement of the radial artery returned, in a very slight degree; and then finally ceased. Within nine or ten months of this discovery, the right pulse, after growing less and less distinct by degrees, had vanished also. Though somewhat weak, and subject to faintness, this gentleman had not wasted; nor had the muscles of his arms lost either bulk or vigour. Their veins were full enough of blood. His hands were often cold; and he felt altogether worse during cold weather.

Failing to detect any pulsation in the brachial and subclavian arteries, I next felt for the carotids: but I could perceive no beating in the track of their course. I had placed my finger, for a few seconds only, in front of the left sterno-mastoid muscle—when I saw that his head drooped, his cheeks became white, and he was on the brink of fainting. But he recovered immediately. Then I made similar pressure, for a moment, on the right side of the neck, and the same phenomena were instantly repeated, with the addition of convulsive jerking movements of the head and arms. He rallied again directly upon my removing my finger, and was scarcely aware of what had happened. For a second or two he had been unconscious.

His femoral arteries throbbed as usual. You will call to mind here what was said in a former lecture as to the immediate cause of an epileptic fit.

I next examined his chest. There was no external irregularity or want of symmetry. Percussion gave a clear resonant sound everywhere in front. The heart was heard, beating with frequency, but without any bruit, over the greater part of the thorax. Its impulse in the præcordial region, below the nipple, was feeble: but a strong jarring impulse was communicated to the ear when the stethoscope was applied to the upper part of the sternum.

The patient complained of pains affecting his shoulders, clavicles, and the back of his neck; and of slight difficulty of swallowing.

From the intelligent physician who had attended this gentleman in the country, I learned the instructive fact that, twenty months before, a loud rasping bruit had been audible, without impulse, at that part of the sternum where he, as well as I, now found no bruit at all, and a very considerable impulse.

I could not doubt that in this painfully interesting case there was aneurismal disease of the aorta, interfering with and lessening, but not absolutely excluding, the stream of blood through the arteries which spring from its arch.

The patient continued to live on, incapable, however, of any exertion, for upwards of two years; when one evening, upon his raising himself from the sofa to cough, arterial blood suddenly poured from his mouth and nostrils, and he was presently dead. I am indebted to Dr. Durrant, of Ipswich, for an account of the morbid appearances discovered upon opening his thorax.

The ribs and sternum being raised, the aorta was seen enlarged, slightly projecting forwards, and overlapped by the lung. When the lungs had been removed, the whole arch became visible, enormously dilated, firm, inelastic, and adherent to the bodies of the second, third, and fourth dorsal vertebræ. More than two-thirds of the interior of the dilated vessel was filled up with dense fibrin, looking like muscle. The lining membrane of the aneurismal part was ossified throughout; the earthy matter lying in separate pieces, many of which resembled concave shells. The bodies of the third and fourth vertebræ, and the left half of the body of the second, were absorbed, the intervening cartilages remaining entire. The vertebral canal was bounded, in part, by the posterior wall of the aneurism. The arteria innominata was slightly dilated, the subclavian and brachial arteries were pervious, but attenuated; their fibrous coat being softer than natural, and much less elastic. The heart was atrophied; weighing probably not more than five ounces. Both lungs were congested, and their lower borders were emphysematous. The aneurism communicated with the trachea by an aperture about as big as a quill.

It is an interesting fact, deduced by Dr. Sibson from the analysis of nearly 900 instances of aneurism, that those cases which end by rupture of the sac are attended during life with less formidable symptoms than those which kill without such rupture; and sometimes with no symptoms at all. The patient may seem, and may believe himself to be, in perfect health. The reason of this is obvious enough. Rupture is often prevented by some opposing part, upon which the enlarging tumour makes distressful pressure.

In the numerous specimens upon the table, you will find ample evidence and illustration of almost every one of the effects which I have described as apt to result from the pressure of thoracic aneurisms of the aorta. But similar effects would ensue from the same degree of pressure, however caused; and other morbid tumours, cancerous tumours in particular, are not uncommon within the thorax. Hence these same effects, considered as symptoms, are in themselves of equivocal import. If they occur in conjunction with signs of disordered circulation, or of a diseased heart, we may reasonably conjecture that they are produced by an aneurism. But we can seldom be quite sure of this, until the advancing aneurism comes near the surface, and causes an external prominence or tumour which pulsates visibly, or of which the pulsations are perceptible by the touch. And even then it may require some care and tact, to avoid mistaking an enlarged gland or a malignant growth, lying over a sound artery, and receiving an impulse from it, or communicating to it some unnatural sound, for the diseased vessel itself.

The pulsating tumour, if the aneurism have formed in the ascending aorta, makes its appearance, usually, on the right side of the sternum. If the aneurism be situated in the forepart of the arch, it produces a bulging at the sternal extremities of the upper ribs of that side. When it springs from the summit of the arch, the tumour rises above the sternum, and the sternal ends of the clavicles; and when the disease occupies the descending portion of the thoracic aorta, it will sometimes destroy the ribs and the bodies of the vertebræ, and push forward the lower portion of the left scapula: or it may show itself in front, beneath the left clavicle.

When such a tumour presents itself, and is attended with a steady, heaving pulsation, synchronous with the systole of the heart, the doubt and obscurity which may have previously hung over the nature of the patient's disorder is cleared away. A little attention to all the circumstances of the case will generally suffice to determine its true character.

There are, however, some errors prevalent respecting these pulsating tumours, which errors I shall glance at in passing. In the first place, the pulsation of the tumour is frequently attended with a rough bellows-sound; and some persons rely upon this as distinctive of the nature of the pulsating tumour. They hold that this harsh bellows-sound is always discernible in an aneurismal tumour; and that when such a sound cannot be heard, the tumour is not aneurism. But this is a mistake. There have been in the Middlesex Hospital within the last six months (1837) two instances of pulsating tumours in the fore-part of the thorax, unattended with any bellows-sound; yet they were both ascertained, after death, to be aneurismal tumours. One of the aneurisms is before you. It results from Dr. Sibson's researches that, in reality, a bellows-sound is more often absent than present. In the case of the country surgeon, a bellows-sound was heard at a certain period of the disease, but ceased at a more advanced period. I may say the same of the purring thrill. It is a common, but by no means a necessary attendant upon thoracic aneurisms. Again, much stress was laid, by Laennec, upon the circumstance of the aneurismal pulsation being single, being unattended by any second sound. But this is not a true rule if taken universally. In the instance which furnished this very preparation, the sounds were double, just like those of the heart. The second sound heard *is*, no doubt, the second sound of the heart, conveyed from the place of the aortic valves, where it originates, along the course of the vessel, to the aneurism; which often indeed lies in contact with the heart, and could scarcely fail to have the diastolic sound propagated through it. It is a fact not so easily explained, yet it certainly is a fact, that a double sound *may* be audible in aneurisms very distant from the heart. Dr. Davies states, that he never heard a second sound in abdominal aneurisms; yet I presume that, under favourable circumstances, the sound of the closing of the floodgates at the root of the aorta, may be heard far along its channel. I can account in no other way for the second sound, heard by myself and by many others, in a *popliteal* aneurism. I mentioned before a patient whom I saw in St. Bartholomew's Hospital, and in whom an exceedingly loud diastolic sound, like the sharp whining note of a dog, was audible by the ear placed upon his arm, over the brachial, and even over the radial arteries.

There are some judicious remarks made by Dr. Hope upon the sounds that are apt to be heard in these pulsating aneurismal tumours to the right of the sternum; showing how they may be distinguished from the natural sounds of the heart itself, conveyed to that spot through some dense conducting medium. He observes (and all that I have seen has been consonant with this observation), that the first of the aneurismal sounds, when there are two, the sound that coincides with the pulse, is always louder than the natural systolic sound of the heart, and generally louder than any of the morbid systolic sounds; and that instead of increasing in intensity, as the stethoscope is moved gradually towards the præcordial region (as it ought to do, if it were the conducted sound of the heart itself), it diminishes in loudness, until it is gradually lost in the actual systole of the heart. Whereas the second sound heard over the tumour does augment as we get nearer the heart, for it is, in truth, the diastolic sound of the heart, and therefore is more audible as we approach the point where it is generated. The sounds of aneurisms of the aorta are usually audible in the back also; and if a very loud bellows-sound be heard there, where the natural sounds, if heard at all, are always much abated, that circumstance furnishes a strong additional ground for suspecting the presence of an aneurism, or of some great change in the aorta.

To give you some notion of the course which aneurism of the thoracic aorta may run, I will describe another instance of that disease which occurred under my own observation. The subject of it supplied the preparation to which I last referred.

He was a stout, healthy-looking man, forty years old, a private coachman. He became my patient in the hospital on September 8, 1836. He complained of pain and tenderness around and above the right mamma. The pain was increased by a full inspiration: and when lying on the opposite side, he felt as though he was tied in the painful part.

He had been ill a month only. His illness commenced with severe rigors, and fever, and sudden pain in the side, for which he was bled three times with much relief. He attributed the attack to having lain, upon his right side, in a damp bed.

There was scarcely any projection at the spot where the pain and tenderness were experienced. By careful examination several times repeated, I satisfied myself upon the following points.

On the right side of the thorax no vesicular breathing could be heard; and the whole was dull on percussion. On the left side percussion gave a hollow sound, and the respiratory murmur was clear and strong. In the tender spot, an inch and a half above the mamma on the right side, a strong pulsation could be felt, and two sounds were distinctly audible, the first of them keeping time with the pulse at the wrist. But there was no bellows-sound. M. Sanson, the celebrated French surgeon, was then in London, and went round with me one day, and examined this patient; and he expressed his opinion that it was not a case of aneurism, because there was no *whiz* or bellows-sound to be heard. Of course his examination was a cursory one, and I mention this circumstance merely to show you what importance has been attributed to the presence or absence of a *bruit de soufflet* in such cases. M. Sanson suggested that the heart might be displaced, and pushed over to the right side. However, it was clear to me that this could not be the case, because the breathing was deficient, not on the left, but on the right side; and, above all, because the apex of the heart could be both seen and felt beating in its proper situation, in the præcordial region on the left side. Also on the left side, percussion made on the edge of the ribs gave a tympanitic sound, indicating the place of the stomach; on the right a dull sound, pointing out the situation of the liver; so that it was not a case of transposition of the viscera, such as had been found, not long before, in one of my patients. In the course of the disease, a slight bellows-sound did become perceptible over the right mamma, when the patient sat up; but even then, the natural sounds of the heart, without any morbid quality, could be heard in the natural position of that organ. Œgophony was audible at the back part of the right side of the chest.

This patient had repeated attacks of pain, dyspnœa, restlessness, and inability to lie down; and these attacks were always most sensibly mitigated by the application of leeches to the diseased part. By October 11 he was so comfortable that he wished to go out; and he went to his master's in Connaught Place.

Two days afterwards, he sent to beg that I would go to see him there. He had brought up, on the preceding evening, during a paroxysm of coughing, about a pint of bright red blood; and he had continued to cough, and to expectorate small quantities of blood. I had him again brought to the hospital on October 14, where he remained, apparently much the same as before he went out. But on the 19th he suddenly expired. The whole duration of his illness had been nine or ten weeks.

We found the heart natural in size and in appearance; the pericardium healthy, and containing no more than the usual quantity of serum. All the cavities were natural in their dimensions, and in the thickness of their walls; and all the valves healthy, excepting one white spot on the mitral valve, which could not have interfered with its motions.

The aorta at its origin was also natural in size; but it began to dilate just before it escaped from the pericardium, and the dilatation continued to the giving off of the left subclavian, where the vessel resumed its proper capacity. The arteries arising from the aorta did not partake of the dilatation; but the sac overlapped and adhered to the external surface of the innominata, for about a quarter of an inch from its origin. This explained a symptom I omitted to mention, viz., that the right radial artery beat much more feebly than the left.

The pouch formed by the aneurism adhered in front, for the space of two inches, to the inner surface of the third rib; and close to the edge of this adhesion there was a small irregular aperture about two lines in diameter, by which the interior of the pouch communicated with the right pleural cavity. Nearly a pint of loosely coagulated blood was found in that cavity, together with a greater quantity of serous fluid than could have belonged to the coagulum. Just above the adhesion to the rib, the pouch adhered to the substance of the lung, over a space about an inch square; and here the parietes of the artery seemed wholly wanting. This doubtless had been the channel of the copious hæmoptysis a week before his death: and it is interesting to observe that the opening of the aneurism into the lung was not immediately fatal.* The artery was much diseased, in the usual manner. The right lung was nearly all of it 'carnified' by the compression it had undergone.

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* In the case of the late Mr. Liston, who died of aneurism of the aorta, the fatal issue of the disease was delayed for more than six months after a single occurrence of profuse hæmorrhage from the mouth. The aneurism, springing from the arch of the aorta, was in contact with the trachea, the front of which was thinned, and in three or four places perforated by apertures each large enough to admit a pea. These openings had been effectually stopped by laminæ of coagulated blood; with which one-half of the cavity of the aneurism was filled.

If this case had not terminated as it did, no doubt the aneurism would have made its way outwards through the ribs, as happened in the very remarkable specimen before you; in which you see that the sternum and five of the ribs have disappeared before the pressure of an aneurism in the ascending portion of the aorta. Sometimes, the tumours that form in this manner, project and attain the size of the head of a full-grown fœtus before they burst.

What can we do in these melancholy cases? Not much. Certain points of practice are so obvious that it is almost superfluous to mention them. I mean the observance of quiet, and the religious avoidance of everything likely to excite or quicken the circulation: bodily exertion, therefore; straining of all kinds; mental emotion; stimulating food and drink. These are not only likely to aggravate the existing mischief, but prove often the immediate cause of the rupture of the aneurism, and of sudden death.

I mentioned, in describing the morbid anatomy of aneurism, that when the diseased vessel begins sensibly to dilate, and more especially when it is protruded into a sac or pouch, the blood begins to coagulate upon the diseased membrane. And it continues to do so, from time to time, in successive layers, so that upon dividing the aneurismal sac, you will see concentric laminæ of firmly coagulated blood. This is clearly a strengthening of the weak place—a reparatory and compensating process analogous to others which we have already had occasion to notice. And our object, here as in other cases, must be not to interfere with the natural attempts towards repair, but to assist and promote them, if we can: till, peradventure, a spontaneous cure has been performed.

This principle has long been distinctly recognised in the treatment of aneurisms that are incapable of relief by surgical means.

But it is much to be doubted whether the principle, so sound in itself, has been judiciously followed out. You have probably heard, or will hear, a good deal of Valsalva's and Albertini's mode of treating aneurisms. It was simply that of bleeding the patient repeatedly, and keeping him perfectly still, and upon as low a diet as was barely enough to prevent his perishing of inanition. The object of this plan of treatment was to facilitate the coagulation of the blood by diminishing its force and velocity, in the hope that at length such a solid barrier might be built up and organised, as would, in some sort, furnish a new wall to the artery in the dilapidated part. When this object had had the best chance of being accomplished; when the patient had been so reduced as to be scarcely able from weakness to raise his hand from the bed, to which he was strictly confined; then Valsalva increased his quantity of nourishment by degrees, until the necessary strength was restored.

Now I quite agree with Dr. Copland in thinking that this practice may be carried, and has been carried, to a hurtful extent. He says that he has seen cases 'in which aneurismal tumours had existed for some time without any increase, so long as the patient avoided any marked vascular excitement, and continued his accustomed diet; but when repeated depletions, and vegetable or low diet were adopted, great augmentation of the tumour, and fatal results soon followed.'

In truth we shall perceive reason to expect that this would be the case, when we consider, first, that the starving system, and the frequent abstraction of blood, diminish the quantity of red corpuscles in that fluid, rendering it more watery, and less disposed to coagulate; and, secondly, that what is called reaction—or a violent palpitating action of the heart—is very apt to follow repeated losses of blood; and this forcible action of the heart must tend rather to sweep away the existing coagula, than to cause an additional deposit.

A more reasonable and hopeful plan of management, therefore, would, in my opinion, be one which should keep the action of the heart gentle and moderate, and the motion of the blood as slow and languid as possible, without impoverishing that vital fluid. We should husband the materials of repair, and promote the deposit of them where they are wanted. A nutritious but unstimulating diet, consisting chiefly of solid food; perfect repose of mind and body; and a due regulation of the natural functions; with the abstraction of so much blood only as may be necessary to alleviate pain, or to subdue *excessive* arterial action, or to unload vessels which are manifestly oppressed by their contents; these, I humbly conceive, constitute the most rational means of furthering the endeavours of nature towards a cure. Few cures, indeed, can be hoped for in any way. Yet life may be prolonged in these cases, by great care; and the extension of existence even for a month or two, or a week, or a day, may sometimes be an acquisition of the greatest moment.

I have little to say concerning particular drugs. Digitalis may, perhaps, be sometimes of use; and the acetate of lead is well spoken of by those who have tried it. I have not had sufficient experience of either of these remedies in the treatment of aneurism to enable me to state anything to you, confidently, in respect of their value.

LECTURE LVII.

Diseases of the Veins. Phlebitis; Pyœmia; consecutive scattered Abscesses. Treatment. Effects of the obstruction of large Venous Trunks.

Yesterday I concluded what I had to say, as a physician, respecting diseases of the *arteries*: and this seems as fitting a time as any for taking a final notice of some of the morbid conditions of the *veins.* I am about to speak of the effects of the intermixture of animal impurities with the blood which is carried by the veins: and, again, of the effects of the local occlusion of veins by coagulation and adhesion of the blood within them, whereby their carrying function is defeated; whether such occlusion be a consequence of inflammation of the vein, or not. Both these topics were dealt with in former courses of these lectures—and have been briefly touched on in some earlier lectures of the present course—under the common head of *Phlebitis and Pyœmia*, words indicative of theories which the advance of medical knowledge is daily showing to be too narrow, and even inexact. The morbid condition expressed by those terms is one of immense importance, whether we consider the large amount of mortal disease which it comprehends, or its wide and intimate relation with general pathology. It gives to many fatal injuries, and to many, nay to most, of the fatal operations of surgery, their mortal character: it is of surpassing importance, therefore, to the surgeon. It lies at the bottom of the deadliest cases of puerperal fever: it is consequently of the deepest interest to the accoucheur. It presents itself also, not seldom, in the practice of the physician, appalling him by its insidious, its rapid, and too frequently its resistless course. Moreover, its pathology, which has been gradually ascertained only within these few years, furnishes a key to that of other disorders of scarcely less moment.

It has long been known as a matter of fact that mechanical injuries, wounds, and surgical operations, which for a time may seem to be going on prosperously towards recovery, are apt to be followed in a great number of instances by sudden and severe disturbance (to be presently described) of the bodily functions, and by speedy death: and that scattered collections of puriform matter are found in various parts of the dead body; most commonly in the lungs and liver, but not unfrequently in or near the joints also, in the serous cavities, among the muscles, in the brain, in the eye, and elsewhere; and further, that associated with these abscesses there are almost always evident marks of recent disease and obstruction in one or more of the larger veins.

I have more than once told you, that minute foreign matters entering the blood, and failing to pass out of it again through the natural emunctories of the body, are liable to be stopped when they arrive at the first network of capillary vessels that lies in their course. Now the blood, circulating in the veins, reaches (much of it at least), in each of its circuits, two such great networks, the hepatic and the pulmonary. Through the pulmonary network all the blood must pass, through the hepatic some of it; and it is there, in the capillary tissue of these organs, that particles of pus, and other material substances, foreign to the blood, and incapable of being eliminated with the customary excretions, are apt to stick, or to be entangled, and to excite inflammation. Some of them however, in general, pass on, and arriving at the left side of the heart, are transmitted, with the arterial blood, to various parts of the body, there to exercise a similar deleterious influence.

Let me remind you of Cruveilhier's experiments on this subject. He introduced *quicksilver* into the veins of animals; a metal which is liquid, and divisible into very minute particles, and which exerts no chemical agency upon the vital fluid. When the mercury was inserted into the veins which concur to form the vena portæ, the whole, or the greater part of it, was arrested in the liver. In that organ, the animal being killed a certain time after the introduction of the metal, small, roundish, red spots were always discoverable, which passed gradually into little abscesses surrounded by a halo of inflammatory redness; and in the centre of each red spot, and of each abscess, lay a minute globule of mercury. A few similar points of suppuration were usually to be seen in the lungs also. But when the quicksilver was put into the blood in its direct course towards the vena cava, then it was in the lungs that these points were either exclusively detected, or at any rate most numerous.

It was a natural, and doubtless a sound inference from the results of these experiments, that *pus corpuscles*, finding their way into the veins, might be carried by the blood into various parts of the body, and there become the foci of scattered abscesses. And the theory which, professing to account for, gave at the same time a name to, the morbid condition called pyæmia, assumed the soundness of that inference.

But how, in the cases in question, can pus, with its corpuscles, get into the blood? Pus in substance—pus as pus—is never taken up by the absorbents. Large abscesses, no doubt, do sometimes quietly disappear; but it is the serous element of the pus which is then carried off by absorption, while its solider corpuscular part either remains a harmless mass, or, undergoing gradual degeneration, liquefies, and is finally itself absorbed. The more liquid ingredient of pus would suffer no mechanical detention in the capillaries. The amputating knife may indeed happen to cross and to sink into an existing abscess, and to divide a vein into which pus might be sucked. I showed you, in the last lecture but one, that suppuration *may* occur within the heart, and pus be poured directly into the circulating stream of blood. But these are merely rare and exceptional accidents. The theory alleges that pus is furnished by phlebitis; by suppurative inflammation of a vein which pours forth pus from its inner surface into the blood current. In cases less fatal, it supposes that adhesive inflammation of the vein blocks up a portion of its channel.

But it has become gradually more and more probable that the inner surface of veins does never undergo suppurative inflammation, or any inflammation at all: that it is not, in that respect, analogous to the serous surfaces of the body. Whenever thrombus of a large vein is met with, the obstructing solid coagulum may always be detached from the lining of the vein, which is then seen to be smooth and unchanged. In phlebitis, the outer and middle coat of the vein, and the connective tissue in contact with it, suffer inflammation, of which the innermost coat seems to be incapable.

That fresh and pure pus may mix and circulate, in considerable quantity, with the blood in the veins, without necessarily producing any perceptible detriment to health, and without necessarily causing (as it had been supposed always to cause) any coagulation of the blood, has been conclusively shown by some remarkable experiments reported by Dr. Hughes Bennett. The same conclusion has been reached by Mr. Savory, who has made a careful inquiry into the whole subject. He found, from a series of well-devised experiments upon animals, that none of the effects which are ascribed to pyæmia are necessarily produced by the injection (into the veins) of healthy pus. 'But,' he adds, 'pus may become putrid, more especially if exposed to air, in a wound; and the putrid fluid may be absorbed, and this, like any other putrid fluid, will produce the effects to which the term pyæmia is applied. It will poison the blood.'

After showing that the passage of putrid and decomposing animal fluids into the venous blood—even when they have been filtered of all minute solid particles—is followed by the establishment of spots of stagnation and congestion in the capillaries of various organs, and especially of the lungs; and that the natural tendency of these petechial spots or patches is to pass on into suppuration; Mr. Savory maintains the probability that pyæmia is, most commonly, produced by the absorption of putrid or tainted *pus*. It is, as he says, a rare thing to meet with a case of well-marked and fully-developed pyæmia without any evidence of the previous existence of pus in some part of the body. 'Pyæmia may indeed be due to the absorption of any putrid or morbid fluid, but such a fluid is hardly ever, can hardly ever be, poured out except under the circumstances in which pus will be formed. Nay, the poisonous morbid fluid is often, no doubt, the changed or partially decomposed fluid of pus. At the same time there is nothing peculiar to pus upon which the production of pyæmia depends.'

There is another way in which the phenomena of pyæmia may sometimes originate. The clot in an obstructed vein may become a permanent decolorized solid plug; but it may also, under certain circumstances, undergo other changes, soften and degenerate into a fluid which, from its sensible naked-eye qualities, may rightly be called puriform, and *thus* the venous blood may be contaminated. Sometimes, again, the internal lining of a vein, in phlebitis, may perish and break down from the cutting off of its nutrient vessels, and so pus from without may find direct admission into the vein.

The previous existence of an external wound is not essential to the development of pyæmia. Mr. Savory says that it often occurs, in young persons, in connexion with acute necrosis. But in these cases there is *internal* suppuration. Cruveilhier's experiments bear on this point also. He introduced crude mercury within the hollow shaft of the thigh bone of a living dog. When the quantity was considerable, death occurred in a few days, and the metal was found strewed thickly through the lungs, each globule occupying a pulmonary capillary, and surrounded by a small sphere of inflammatory redness. When the quantity was minute, the animals lived longer, and little *abscesses*, enclosing each a particle of mercury, were then discovered in the same organs. The mercury he supposes to have found a direct entrance into the blood, in these cases, from the cancellous portion of the bone, and through the same channel pus may sometimes enter the circulation. You may remember my relating some fearful examples of scattered abscesses, supervening upon chronic disease of the bones of the ear. Cruveilhier states that, having been present at the examination of the body of one who had sunk after amputation of the leg, and whose lungs were full of little abscesses, he sought, without success, for some inflamed vein: but upon dividing the tibia and fibula, he found the spongy extremities of these bones infiltrated with pus. Here, beyond question, had been the source of the visceral mischief.

It seems that, for the production of pyæmia in any of these ways, some predisposing flaw in the health, some feeble or unsound state, is a very frequent and perhaps an essential condition: some pre-existing illness, or other debilitating influence —such as the exhaustion of parturition, the mental shock and dread of a severe operation, unhealthy occupation, long-continued exposure to hardships, or the like. Mr. Callender quotes a remarkable illustration of the last-mentioned influence from M. Mounier, who says that during the Crimean War no pyæmia was noted among the first two thousand amputations, while afterwards it became of quite ordinary occurrence.

If, then, the terms phlebitis and pyæmia be accepted in the senses in which I have endeavoured to construe them, they may well retain their accustomed place in our medical vocabulary, and spare us the adoption of the uncouth terms, septicæmia and ichoræmia, used in the German schools—

> Names which would make Quinctilian stare and gasp.

In our standard 'Nomenclature of Diseases,' pyæmia is well defined to be 'a febrile affection, resulting in the formation of abscesses in the viscera and other parts.'

The general symptoms by which this formidable secondary disorder is ushered in and accompanied are of this kind. A sudden change takes place in the aspect and manner of the patient. The formation of pus in separate and often distant parts is rapid, and frequently unannounced by any local pain. When however the joints, or parts near the joints, become the seat of suppuration, much soreness is complained of, and the malady is liable to be mistaken for rheumatism: and when the serous cavities are implicated, the pain is sometimes severe. Pyæmia is commonly attended in its progress with repeated shiverings, which are sometimes periodical, and which occasionally run into convulsions; with a high temperature of the body; with profuse sweats, and occasionally with vomiting, or with copious and very unnatural discharges from the bowels. These last have been noticed in animals soon after the introduction of pus, or of putrid matters, into their veins. Nature seems to attempt to eliminate the poison in this way: and where the quantity of pus so introduced has been small, the attempt may now and then be successful. But in general there is a continual supply of the noxious substance, and the system is irrecoverably infected. Almost always albumen may be detected in the urine. The skin acquires a yellowish hue, as if the patient were faintly jaundiced. Here and there upon the surface of the body patches of erysipelatous inflammation are apt to appear; and, sometimes, of superficial gangrene. The pulse is almost always rapid, and feeble. In most cases, but not in all, symptoms occur resembling those which mark low forms of typhus fever. Very constantly there is great agitation, and a signal disturbance of the nervous system. After death, shapeless or circumscribed puriform deposits are found in various parts of the body; in the viscera; in joints, with ulceration and destruction of the articular cartilages; in the shut serous sacs, with effusions of coagulable lymph and serum.

I have called this infection of the blood a formidable disorder: in truth it is almost always a fatal disorder. Yet that it is not inevitably mortal I know by a case which has recently occurred in Mr. Arnott's practice at the Hospital, and which he permits me to mention. He had occasion to amputate the fore-arm of a man whose hand had been crushed by machinery. Two or three days after the operation, the patient's pulse quickened, and he had a severe rigor. These two circumstances led Mr. Arnott to apprehend the supervention of phlebitis; and accordingly one of the large, superficial, visible veins of the fore-arm became swollen, hard, and tender. Leeches were applied along its course; and the parts were kept covered with water dressing. In no long time an abscess formed in the other arm: next, a large one in the back, from which twenty ounces of pus were removed; then one beneath the glutei muscles of the buttock, on both sides—each of these two contained about sixteen ounces. In short, dating between the beginning of October and the middle of December, no less than seven collections of matter presented themselves in various places. The last of them was in a very unusual part, beneath the man's tongue, in the ordinary situation of ranula, for which, indeed, it was at first mistaken. In every instance the pus was let out as soon as possible, and the main feature in the general treatment was the administration of opiates, and of wine, with a liberal allowance of good beef-tea in the earlier stages, and of meat afterwards. This man recovered; and was seen in the month of May following, in perfect health. The case is extremely interesting. It shows, I say, that pyæmia, even when it strews consecutive inflammation and suppuration throughout the body, is not absolutely and hopelessly fatal. Whether abscesses, from this cause, distributed in the lungs or liver, are capable of repair, I cannot tell you. Under the treatment employed, the inflammation of the vein in the arm gradually subsided. All outward evidence at least of its existence, all induration even, disappeared; and presumptively all inward evidence too. So that, had this patient sunk, late in the course of his disorder, under the multiplied secondary abscesses, his venous system might possibly have been searched in vain for any remaining traces of phlebitis: and yet we know that at one time he *had* unequivocal inflammation of a vein of considerable magnitude.

In most instances acute pyæmia runs its fatal course with terrible rapidity, extinguishing life within a very few days. But, as Mr. Paget has well pointed out, in the first volume of the 'St. Bartholomew's Hospital Reports,' there is such a disease as *chronic* pyæmia, which may extend, continuously or with relapses, over many weeks, or months. Occasionally, even the acute form assumes a very slow but steadily destructive progress: 'indicated,' says Mr. Paget, 'by slow wasting; all the tissues becoming dry and shrivelled; by increasing pallor; by decreasing muscular and mental power, the voice becoming weak, the mind slow and dull, and, at night, often wandering; by quickness and feebleness of pulse and breathing; by frequent and sometimes profuse sweatings, especially when there is much suppuration; by less frequent chills or rigors; by increased thirst, and usually aversion from food; by dryness and shrinking of granulations.'

But there are other cases which, presenting the essential characters of pyæmia, are much slower in progress, and much less severe and perilous, than its acute form. These are less common among the instances of pyæmia following wounds, than among those occurring in diseases; the local mischief is more often limited to different parts of one and the same tissue, and if the veins are implicated, it is generally towards the close of the disease. Of such cases we may indulge more hope. The most favourable signs, according to Mr. Paget, are long intervals between the successive local manifestations of disease; and the absence of pulmonary complications. 'The slower the pulse and breathing, and the less the sweating, the greater are, in general, the probabilities of recovery.'

Acute pyæmia—with all its frightful results—is liable to arise, not only after severe, but also after slight injuries; from the trivial as well as the grand exploits of surgery; nay, spontaneously, as it were, without any obvious local hurt, under the agency of natural causes, such as exposure to cold. And the part from which the mischief springs has some influence, as you will now understand, in determining the principal seat of these scattered abscesses. When they succeed amputation of a limb, or fracture of the skull, or the interference of surgery with varicose veins, or (as they may) even the simple operation of phlebotomy, they are likely to be most numerous in the lungs. But they are more conspicuous to hasty observation in the liver than in the lungs; and that is why hepatic abscess was formerly supposed to have some special connection with injuries of the head. Morgagni, however, long ago pointed out the fact, that other parts also were affected in those cases. Again, we may expect to find these disseminated abscesses chiefly in the liver, when the poison enters any of the tributary veins of the vena portæ: when the disease supervenes, therefore, upon operations involving the intestines—operations for the release of hernia, for healing fistula in ano, for the cure of piles.

It is, however, very common for the poison to pervade the whole body, and for abscesses to form in many other situations, as well as in the lungs and liver. I once saw a young woman die, in the Middlesex Hospital, from pyæmia, with large abscesses in many parts, and especially in the joints, after the simple excision, with scissors, of some small spongy irritable growths about the orifice of her urethra.

Two or three instances of pyæmia, unconnected with any known hurt, and originating apparently in exposure to cold, have fallen under my own observation: but I prefer giving you the following short case, with the details of which I have been favoured by Dr. Malden, of Worcester.

Miss ——, a teacher in a ladies' school, was attacked, after exposure to wet and cold, with acute pain, heat, and redness, in the front of the left fore-arm. Mr. Cole, an eminent surgeon, of Bewdley, by whom she was at first attended, discovered inflammation following the course of the cutaneous veins. Upon its subsidence the veins were left like hard cords. Soon after, the right arm was affected in a similar way: and next, both the lower extremities, which became anasarcous. All this was attended with paroxysms, simulating those of tertian ague; exhausting sweats; diarrhœa; and a frequent feeble pulse. At the end of a month, deep-seated fluctuation was detected in the right thigh, three inches below Poupart's ligament. The abscess gradually approached the surface, and was opened, and more than three pints of very fœtid pus was discharged. The wound never closed, and she sank, exhausted, a month after it was made.

There was no pain, premonitory or attendant, connected with this formation of matter.

The abscess was traced, after death, upwards, behind the muscles of the pelvis, as far as the sacro-iliac symphysis, where the bones were extensively carious.

Many of the superficial veins, both of the upper and the lower extremities, were found to be completely sealed up by coagula of blood.

Pyæmia is sometimes so remarkably prevalent, as to partake of the character of an epidemic disorder. It may then spread, no doubt, by contagion. The air of a ward may be made poisonous by its presence. And certain other atmospheric influences seem, at times, to predispose the human body towards a ready reception of the malady. During such periods prudent men refrain, if they can, from the performance of surgical operations.

The treatment of acute pyæmia offers to the known resources of medicine a very sad and unpromising problem. The earliest of the symptoms, and the immediate danger, depend, no doubt, upon the poisoning of the blood, and not upon the scattered abscesses, which are yet to come. Have we any reasonable grounds for hoping that art, or nature, may suffice to eliminate the poison? The occasional vomiting, and diarrhœa, and the profuse sweats, would seem to point towards natural efforts in that direction. We know that other blood poisons do thus pass out of the system. No trust can be placed, I fear, in the efficacy of drugs given with the view of correcting, chemically or otherwise, the tainted quality of the blood; the chlorides, I mean, or the sulphurous acid. One great object is plain. To give the efforts of nature time and opportunity, we must support the patient's strength by abundant digestible nutriment, and by the free use of stimulants, of which brandy is probably the best. Opium is indicated to allay the nervous distress. Chloral may perhaps subserve the same purpose. Should the patient rally from the first assault of the malady, the weakening effects of the disseminated abscesses will come into play, and demand a continuance of the upholding and quieting measures. The need of tonics, and the regularity with which the rigors sometimes occur, tempt to the prescription of quinine in full doses. And that remedy is not without some effect: it will stop the shiverings, but it does no other good. I mentioned this curious fact when speaking of the occasional pyæmic results of otitis; and Mr. Paget tells me of similar experience on his part. Local treatment, in these urgent cases, is of minor importance. If there be evident inflammation of a superficial vein, or of a joint, it will be right to apply warm fomentations and soothing lotions; and the contents of accessible abscesses should be removed as soon as that can prudently be done. But our efforts, however well directed and zealous, will commonly end in disappointment and failure.

The local phenomena, when a superficial vein of some magnitude is inflamed, are pain and tenderness in the course of the vessel, which, by coagulation and adhesion of the blood, is soon converted into a tangible, hard, and sensitive cord. Whether the vein be near the surface, or deeply seated, there is usually more or less œdema of the areolar tissue of the part. Phlebitis of this kind has been sometimes confounded, I believe, with inflammation of the lymphatic absorbent vessels. You distinguish the latter by the slenderness of the painful cord; by its position, which is still more superficial than that of a subcutaneous vein; by the number of little knots which diversify its course; and by the streaks and patches of bright inflammatory redness which appear along the same track. Dr. Graves remarks ('Clinical Medicine,' p. 454), that inflammation of the lymphatics 'is seldom continuous, but is developed at certain insulated points.' Velpeau has laid down the following aphoristic distinction between the two. 'Angioleucitis is seen, but not felt, while phlebitis is felt rather than seen: so that the complaints might be discriminated even with closed eyes.'

The *obliteration* of a large vein, whether from phlebitis or in any other way, is perilous in proportion to the magnitude of the vein, to its importance in the vital economy, and to the rapidity with which its complete occlusion has been effected. If the organ mechanically affected by it be not a vital organ, if the system can await the development of a collateral venous circulation, all, at length, may end well. Sometimes, indeed, as the inflammation gradually subsides, the coagulum softens and is reabsorbed; the blood meanwhile drills for itself a fresh passage by the side of the plug, and the circulation is restored in its accustomed channels. But there is always, under these circumstances, a concurrent danger of embolism. The gradual stoppage of even the largest—the primary venous trunks, the venæ cavæ—admits of some degree of compensation. In one instance of this kind, which I briefly described yesterday, and which I myself witnessed, the *superior* cava was flattened, and its channel completely effaced, by the pressure of an aneurismal tumour: in another, which I mentioned formerly, on Mr. Kiernan's authority, an immense varix of the superficial veins of the abdomen supplied to the returning blood the passage denied to it, in its natural course, by the partial obliteration of the *inferior* cava. To impress upon your recollection the ordinary phenomena that result from these grave derangements in the hydraulic machinery of the body, I will state here, from my hospital case-book, the outlines of two additional examples of a similar character.

James Buck, aged thirty-three, was admitted on March 6, 1838. The appearance of this man was very remarkable. His countenance was swollen and livid; his eyeballs projected; his lips, the end of his nose, and the rims of his ears, were of a deep purple colour. It was manifest that the blood did not freely descend from the head. Further evidence of this became apparent when the trunk of his body was uncovered. The throat was very broad, full, and tumid, like that of a goitrous person, yet the swelling was not owing to enlargement of the thyreöid gland, nor to œdema; but felt firm and fleshy. The jugulars were distended; and the whole surface of the thorax in front, with that of the shoulders, and of part of the abdomen, was thickly overspread with a network of prominent veins. The external mammary veins were seen to communicate freely with veins proceeding from the neck on each side, with the veins of both the upper extremities, and with the epigastric veins from beneath. Here and there were patches of minute purple varicose branches, crowded closely together.

He told us that whenever he stooped down—to tie his shoe-strings, for instance—he became giddy, his head swelled, and his face and ears grew black; that he was very nervous, easily flurried, and dreamed much, thinking that he was flying in the air, falling down precipices, and the like. He had not noticed any swelling of the face or throat until three weeks previously; and he had never, he said, had a day's illness before. He knew of no cause for the complaint; had been making no extraordinary bodily effort; had never suffered rheumatic fever. He professed, also, temperate habits, but he had been a soldier, and afterwards a pugilist, and his wife informed me that he had led an irregular life.

The evidence, I say, was strong of some obstruction to the return of the blood through the superior cava. Now, such obstruction is most commonly produced by intra-thoracic tumours—sometimes by carcinomatous, much oftener by aneurismal tumours. There were no circumstance to make it likely that malignant growths existed; but there were circumstances which corroborated my first suspicion, that the symptoms were dependent upon aneurism of the aorta, or of one of its primary branches.

There was indeed no external prominence, no pulsative or other swelling, no aneurismal whiz, to guide us to this diagnosis. Upon careful and repeated auscultation of the chest, the murmur of respiration was found to be in some parts feeble and unequal. This might consist with the presence of any kind of tumour. The heart's action was heard, and felt, strong and heaving, in the proper place, beneath the left nipple. To the right of the sternum also, and near the middle of its upper portion, one's ear was distinctly jarred at each systole of the heart, though with less force than in the præcordial region. But in the space intermediate between these two spots no such jarring sensation was perceptible, although the heart could be heard, beating with a slight bellows-sound. Moreover, the right radial artery was considerably weaker and smaller than the left. This showed that the innominata was interested in the disease. The symptoms, taken together, left no doubt on my mind that there was an aneurismal pouch beneath the sternum, where the jar was experienced. I have gone somewhat into particulars to show you how confidently sometimes, by close observation, you may pronounce upon the condition of parts which you can neither see nor touch.

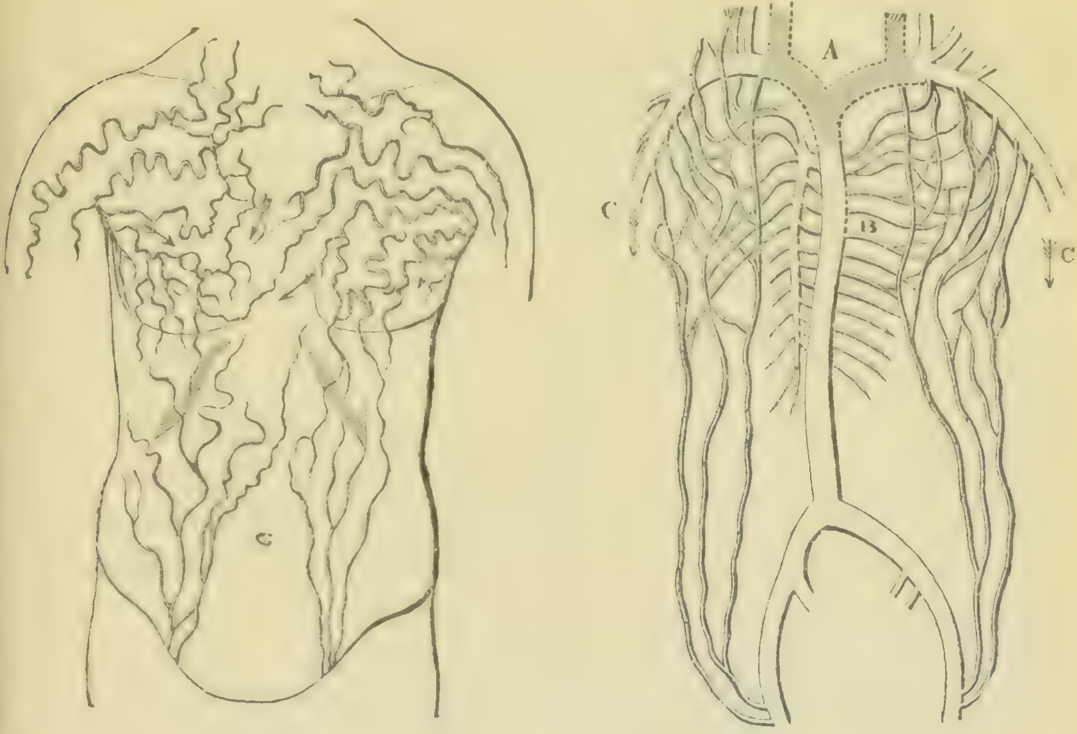
All that could reasonably be hoped for from medicine was postponement of the evil day. To relieve the oppressed blood-vessels by taking away part of their contents, by freely purging the patient, and by setting his kidneys at work—this was what was to be attempted; and this was done. He was repeatedly cupped, and always with most sensible relief to his feelings, the blood flowing copiously. Purgatives and diuretics also acted well; and so much was the man benefited by these measures, that *twice* he left the ward, and became an out-patient.

About the middle of June a new symptom arose—severe pain extending from the right collar-bone across the shoulder. He died on July 10. Three or four days before his death, he had rigors and extreme dyspnœa, complained of pain over the whole thorax, and declared that his 'heart seemed on fire.' These symptoms were caused by the supervention of pericarditis, which speedily proved fatal. A thin layer of recent, reticulated lymph was found covering a considerable extent of the surface of the heart.

The body was examined by Mr. Shaw, after injection of the veins and of the thoracic duct.

There was a large aneurism of the arteria innominata; of which I omit all particular description, my present object being to draw your attention to the state of the veins. The two great trunks that, coming from either side, unite to form the vena cava superior, were completely closed up, as well as the corresponding portion of the cava itself, which was lost and confounded in the walls of the aneurismal sac. The subclavian veins were pervious up to the point where they joined the internal jugulars, but no further. The preparation of these parts, which is before you, and the rough diagrams which I here exhibit, will aid your comprehension of the mode whereby the blood, descending from the head, found its way at length, through many circuitous channels, to the heart. The larger deep-seated compensating veins were not greatly magnified, but the number of the smaller branches was much augmented. The *vena azygos*, for example, was very little above its usual size; yet it was apparently provided with a greater number of considerable branches than are commonly observed under natural circumstances.

As the veins into which the trunks of the absorbent vessels discharge their contents were obliterated, it became interesting to examine the state of these vessels, and of the lymphatic glands. But the condensation and confusion of all the parts around the tumour rendered it difficult to trace the thoracic ducts. The glands were remarkably large, of a purple colour, and gorged with bloody serum. The fulness of the neck, noticed during life, was occasioned by this turgescence of the *glandulæ concatenatæ*. Large glands were seen studding the walls of the aneurism, and adhering to the great vessels connected with it: i.e. in situations where, under ordinary circumstances, such glands, from their minuteness, can scarcely be detected at all.



A Obstructed veins.
B Seat of the right auricle.
C Thoracic, pectoral, and mammary veins, conveying the blood in a contrary course to its usual direction, and anastomosing with the intercostal and epigastric veins.

With this case, contrast the following:—

Harriet Baldwin, thirty-three years old, was admitted December 29, 1840, anasarcous as high as the hips, and with an enlarged abdomen. The swelling had begun, she said, a fortnight before.

She complained of cough, and of expectoration, which was sometimes tinged with blood. She could not lie down in bed for dyspnœa. Her urine was scanty and dark-coloured.

She told us that she had dropsy, quite as bad, five years ago, of which she was cured in St. Bartholomew's Hospital.

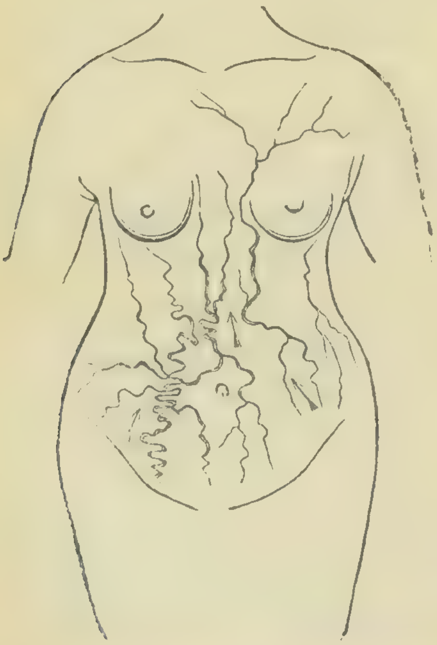
All this we learned in the admission-room. The next day, when she was in bed, we learned a good deal more.

The large abdomen did not owe much, if any, of its bulk to ascites. It contained a palpable tumour, filling the right hypochondrium, and extending thence far beyond the umbilicus to the left, and into the right groin. This tumour, from its situation, and from the continuous dulness elicited by percussion from the right mamma downwards over its whole extent, was evidently formed by the liver, much enlarged, and out of its place.

But, besides the tumour, the abdomen presented on its surface a very singular appearance. Two zigzag lines of large, varicose veins ran up its middle, near the right edge of the linea alba. These, which were evidently the epigastric veins, inosculated above with the mammary. Large but straighter veins wandered over the front of the thorax on both sides.

The swollen legs of this woman were quite purple from innumerable clusters of small varicose veins.

Other symptoms also there were, but I pass them by as irrelevant to my present subject. It was plain that the current of the blood along the vena cava inferior was suffering impediment. That vessel was presumably compressed, perhaps rendered totally impervious, by the superjacent tumour. The existence of the tumour; the great œdema of the legs, compared with the slight amount of liquid in the cavity of the belly; the varicose state of the cutaneous veins of the legs; and, above all, the remarkable condition of the superficial veins of the abdomen:—these were the evidences. The blood from the lower extremities passed mainly by the way of the intercostal and subclavian veins to the heart. Death took place on January 19, 1841. A part of the liver appeared perfectly healthy; another part contained a prodigious quantity of hydatids. When removed from the cyst which had contained them, they filled a large wash-hand basin. The sides of the inferior cava were pressed together by the tumour, and its channel was thus completely closed up for the space of three inches.



In each of these two cases, the closure of the great venous trunk was effected gradually, as the compressing tumour augmented; and time was afforded for the development of collateral supplementary channels. In both cases the superficial veins of the thorax and abdomen contributed largely to supply the growing needs of the system: but the stream of returning blood ran oppositely in the two cases; from above downwards in the first, from below upwards in the second. The direction in which the blood in the veins is moving can, of course, be always readily ascertained; and this might furnish a test, were other tokens wanting, whereby to determine whether the obstruction lay in the superior or in the inferior cava. And there is another circumstance worthy of remark, and of which the same use might be made. In the first case, the dilated veins of the thorax were tortuous, those of the abdomen direct. In the second this was reversed; the epigastric veins were singularly sinuous, the mammary veins were straight. In other words, those veins were, in each instance, contorted and winding, in which the actual course of the blood was retrogade. The vessels were bent and twisted as the current forced its backward way against the opposing but ineffectual barrier of the valves.

It is impossible, I think, to find more clear evidence than these interesting cases exhibit, of the power inherent in the animal body of rectifying, to a certain extent, its own accidental derangements. You cannot, under such circumstances, overlook the existence, or mistake the tendency, of a *vis medicatrix naturæ.*

LECTURE LVIII.

Asthma: its nature; complications; exciting causes; and treatment. Diseases of the Œsophagus: Inflammation; Stricture; Spasm; Dilatation.

I MUST not leave the subject of thoracic disease without saying a word or two respecting *asthma*; a complaint which might have been properly arranged among the *nervous spasmodic diseases*, in a former part of the course. But I purposely deferred speaking of it, because, though in many instances purely spasmodic, and independent of any discoverable faulty structure, it is still more often connected, as cause, or as effect, with organic diseases of the heart, or of the lungs; which diseases had not then been described.

I scarcely need caution you against the vulgar error of calling all kinds of difficult breathing by the name of asthma. You will be constantly meeting with persons who, labouring under some permanent embarrassment of the respiration, tell you they are asthmatic. They conceive that asthma is simply an inconvenient, and not at all a dangerous affection; and they please themselves with the notion—consumptive patients and their friends do this continually—that they are *merely* asthmatic. Asthma *is* dyspnœa, but dyspnœa is not *necessarily* asthma.

Asthma may be defined as being—great difficulty of breathing: occurring in paroxysms; accompanied by a loud wheezing sound of respiration; passing off, after some hours, with more or less mucous expectoration; and unattended with fever. And these paroxysms of dyspnœa are believed to depend upon a spasmodic constriction of the bronchial and pulmonary air-tubes.

To go rather more into detail: the phenomena which constitute and characterise a fit of asthma are somewhat as follows:—The patient, if he have previously suffered under the disease, has usually some well-understood warnings that an attack is hanging over him. Loss of appetite; frequently much flatulence and eructation; languor, irritability, drowsiness, oppression, chilliness; and he goes to bed ill and uncomfortable. The dyspnœa comes on generally after midnight, about two or three o'clock in the morning; often during sleep; and the patient wakes with a sense of tightness and constriction about the chest, and an inability, as it seems to him, freely to expand it. He is obliged at once to rise up; and he sits, leaning forward, with his knees drawn up, his elbows on his knees, and his chin supported by his hands, his shoulders raised, his head thrown back, labouring for his breath, and making a wheezing noise so loud as to be audible at a considerable distance. He experiences a strong desire or necessity for fresh air; opens perhaps the door of his room and creeps out upon the staircase, or crawls, though scarcely able to move, to an open window, even in very cold weather; and remains there, with his head out, sometimes for hours. That he can do so with impunity furnishes a strong presumption that it is the nervous system which is principally affected in these cases. His extremities at the same time are usually cold, and his countenance is anxious and haggard; while the trunk of his body may be wet with perspiration. Sometimes the face is a little flushed and turgid; but more commonly it is somewhat pale and shrunk. The pulse is often small, feeble, and even irregular; and in many instances there is much palpitation of the heart. At other times the pulse remains undisturbed. Notwithstanding the patient's sensation of tightness, his chest is expanded to the utmost, and fixed in that condition by its muscles. The respiration is not short, but difficult; it is not the hurried breathing of a man who has run himself 'out of breath:' on the contrary, it is often less frequent than that of health, and the natural ratio between the length of the inspirations and the length of the expirations is reversed. The little air that gets in, gets in slowly, but it wheezes out again much more slowly—chiefly, it is thought, because the expiratory force is feebler than the inspiratory.

Dr. Hyde Salter—whose exhaustive treatise on asthma I have the greater pleasure in commending to your careful study because he is one of the many distinguished alumni of this College—Dr. Salter mentions, as a curious and common attendant upon the earlier stage of an asthmatic attack, a troublesome itching, which scratching does not relieve, under the patient's chin. This, to me, is a new symptom, and difficult to explain.

If urine be passed, as it frequently is, at the beginning of a fit of asthma, it is copious and watery, pale, clear, and without smell, like the urine of hysterical women. The bowels are also sometimes relaxed, with 'something (as Sir John Forbes observes) of the impatient hurry and imperfection of spasmodic action.' There may be some propensity to coughing, but the patient can hardly achieve a cough; and is so engrossed with his breathing that he can speak, in an interrupted manner, only with difficulty and uneasiness. He has not, however, in general, any misgivings about the event of the attack, but looks forward with hope to the expected termination of the paroxysm.

'These symptoms often continue for many hours together; and particularly from midnight till morning is far advanced. Then, commonly, a remission takes place by degrees. The breathing becomes less laborious, and more full; so that the person can speak or cough with more ease. And if, as is usually the case, the cough brings up some mucus, the remission becomes immediately more considerable, and he falls into a much-wished-for sleep.'

Paroxysms such as I have been describing will often continue to recur for many nights in succession; remitting at length in their severity, and ceasing, for a period, altogether.

During the intervals between these paroxysms, in the daytime, the patient *may be* perfectly well; but he seldom *is* so; though so great is the difference between his condition during the remissions and his condition in the paroxysms, that he declares, and perhaps fancies, that he is quite well. You will mostly find, however, that he is short-winded; that he does not utter many words of a sentence before he pauses to take breath; that slight bodily exertion hurries his respiration; and that he is not easy in a horizontal posture, with his head low.

Although the dyspnœa is thus intermittent, or remittent, you are not to suppose that it is necessarily *periodic*, in the sense in which paroxysms of ague are periodic. So at least it seems to me. The attacks do, indeed, often happen at strictly regular intervals; but this is only when their exciting cause in that particular person—which cause may or may not be obvious—happens to recur at those intervals. There are exciting causes which act habitually every night; every week only, such as an unusual supper on Sundays; every month only, as the catamenia: and these may give to the paroxysm a semblance of periodicity which else is not essential to it. Thus I shall have to show you that certain *places* always bring on an attack in some persons, whether their visits to those places are periodic or irregular. The circumstances of the paroxysm differ in different instances. I may remark also, that when the paroxysm ceases with little or no expectoration, the case is said to be one of *dry* asthma; when the expectoration is copious, it is *humid* or *humoral* asthma.

Now this, I say, is looked upon as being essentially a spasmodic affection. Upon what grounds?

Why, in the first place, the patients have a *sensation* of constriction in the chest. An old gentleman whom I saw lately, and who is subject to fits of asthma, made use of the term *cramp* when he described what he felt about the thorax; and his attacks were always accompanied or succeeded by *actual* cramp of the muscles of the calves of his legs. This is no uncommon circumstance, this co-existence of decided spasm in other parts; and it throws some light upon the nature of the disorder. Again, the rapidity with which the dyspnœa comes on, and the suddenness with which it often abates, resemble the caprice of spasm. The supervention of extreme, sometimes enormous flatulence, and the secretion of hysterical urine, mark also the nervous character of the symptoms. So likewise do the *juvantia* and the *lædentia*, as I shall presently explain further; the affection being suddenly produced by certain causes of irritation, and even by mental feelings—suddenly relieved, sometimes, by medicines which are reckoned antispasmodic. If we add to these considerations the fact that the dead bodies of asthmatic patients have often, on being examined, presented no vestige whatever of disease, either in the lungs or in the heart, we obtain very strong presumptive evidence that the phenomena attending a fit of asthma may be the result of pure spasm.

But if this be so, what are the muscles thus fixed in spasmodic contraction?

You are doubtless aware that the air-tubes are encircled with a series of little fibres, or bundles of fibres. I have more than once shown you these, exaggerated by hypertrophy, in the larger bronchi. They have been traced, by Reissessen, in tubes of very small diameter. Laennec states that he had distinguished them in bronchial ramifications less than one line across. Now, supposing these circular fibres to be muscular, it becomes at once, and *à priori*, likely that they, no less than other muscles, should be liable to spasm. And the phenomena of asthma prove, to my mind, that they are so. Analogy would say that the fibres, thus disposed, are slender muscles, similar to those which surround the intestines and the urinary bladder; and the microscope, scrutinising their minute texture and appearance, asserts that they are actually muscles, of the unstriped kind, like other involuntary muscles subserving the organic life. This fact—which, I am aware, has been doubted—I state upon the authority of Professor Todd and Mr. Bowman; both known to you all as faithful and expert observers. But a test, less fallible than the microscope, has practically settled the question. Dr. Williams has demonstrated, by a set of ingenious and satisfactory experiments, that the lungs and air-tubes are actually *contractile,* to a very considerable degree, under electrical, chemical, and mechanical stimuli. The contractions take place steadily and slowly, and are followed, as soon as the stimulus is withdrawn, by an equally gradual relaxation. This is very like tonic spasm. The contractions were rendered apparent by means of a bent glass tube containing coloured liquid, and adapted to the windpipe of an animal just deprived of life. The column of liquid in the glass tube would of course be readily moveable by any contraction of the lungs and air-tubes, causing pressure of the included air against it. In one of the experiments, 'on passing a galvanic current from the margin of the lungs to the insertion of the tube in the trachea, the fluid rose quickly, but gradually, nearly two inches; sank speedily on breaking the contact; again rose upon completing it; but fell slowly when the current was continued for some seconds:' i.e. when the irritability of the tissues was temporarily exhausted. Temporarily, I say, for on waiting two or three minutes between each application of the galvanism, the liquid was raised again and again for upwards of an hour; till, in fact, the organic life was extinct. Is not all this exceedingly like the behaviour of parts acknowledged to be muscular, under similar influences?

The phenomena were not occasioned by any general shrinking of all the pulmonary tissues. For when the lungs were cut across by sharp scissors, at right angles to the air-tubes, and the open sections of these tubes were galvanised, they were *seen* to contract to one-half of their former diameter; and even to become smaller than that. The contraction was the most distinct in the middle-sized tubes, they being about the bigness of a straw; but it was sensible enough in the trachea, which was sometimes so far reduced in dimensions, that the ends of its cartilaginous rings came together.

A foreign experimenter, M. Valentin, carries us a step nearer to the full solution of this interesting question. He found that the rings of the trachea could be made visibly and distinctly to contract *by irritating the par vagum.*

The natural function of the contractile fibres is probably (as Dr. William Gairdner suggests) that of gradually propelling outwards, by a kind of peristaltic movement, the mucous secretion which is constantly oozing, in small quantities, into the smaller air-tubes.

Upon the whole, we may safely conclude that asthma is one of the spasmodic disorders of the excito-motory system of nerves. I believe, moreover, that, as in most other disorders of the same class, the spasm may be of centric or of eccentric origin. In the eccentric form, the par vagum is doubtless the afferent nerve, either in its gastric or in its pneumonic branches; and the impression it conveys to the medulla oblongata is reflected, through associated motor nerves, upon the bronchial muscles. The centric variety results from a similar impression originating in the nervous centres, which respond, mysteriously, to certain feelings of the mind.

It is interesting to listen to the breathing of a patient under a paroxysm of pure spasmodic asthma. No respiratory murmur, or very little indeed, can be heard. And an attentive inspection of the outside of the chest shows, that amidst all the tugging and heaving for breath, the movement of the thorax is very limited. The patient cannot open his lungs, as it were; and what air does get in has a difficult and narrow passage, as the wheezing noise demonstrates. Laennec affirms, that if the patient, after holding his breath nearly as long as he can, attempt a quiet and gentle inspiration, the spasm may be often overcome as if by surprise; and, for a few seconds, the entrance of the air into the cells may be heard in a clear and even puerile sound. If this be true, it is a strong additional proof that the obstruction to the admission of air was really owing to a tonic contraction of the little muscular fibres of the bronchi and their ramifications.

The hereditary nature of asthma is perfectly consistent with the same theory. It is one of the maladies which are distinctly transmitted—the disposition to them, I mean—from parents to children. And like other spasmodic disorders, it facilitates its own return. When it has once occurred, it seldom fails to happen again and again.

But though I believe, for the reasons I have now mentioned, that asthma, in the restricted sense of that term, is purely a spasmodic affection; yet I know also that it is very frequently indeed combined with organic alterations within the thorax. These changes of structure are to be regarded as so many strongly predisposing causes. They induce a readiness to take on spasmodic action; and some of them are perhaps aggravated, or even produced, by the fits of asthma, upon which they afterwards re-act injuriously. Judging from my own experience, I should say that genuine uncomplicated spasmodic asthma was *rare*. The organic diseases with which spasmodic asthma is often found connected are principally emphysema of the lungs, and structural changes in the heart and great blood-vessels.

It seems probable that, in the outset at least of an asthmatic paroxysm, the lungs, debarred of their needful fresh supplies of air by bronchial spasm, have their blood supply proportionally lessened by contraction of the minute pulmonary arteries. If the spasmodic constriction of the air-passages closed them completely, its effect would be that of sudden strangulation. It is a fact that in dogs killed by plugging the windpipe, as in convicts who have been hanged, the lungs, if examined immediately after death, are found to be remarkably anæmic. Gradually this arrest of the blood in the minute arteries tells in a backward direction, leads to general venous congestion, and therefore to congestion of the bronchial veins and their capillaries; and thus we may account for the mucous discharge and expectoration which follow the relaxation of the spasm, and even for the hæmoptysis which sometimes occurs.

For this explanation of the hæmoptysis of apnœa, Dr. Salter acknowledges his obligation to Dr. Johnson.

I shall hereafter have to tell you that in what is called the *collapse* of cholera morbus there is the same emptiness of the lungs; and the outward marks of the two morbid conditions are very much alike. We see in both the same shrinking of the features, and ghastly corpse-like aspect, the same blueness of the skin and lips, the same coldness and wetness of the surface, and feebleness of the pulse, and apparent proximity to death.

Can any explanation be given of the very remarkable circumstance that the paroxysms of ordinary asthma come on regularly about the same time, namely, in the early morning, during the first sleep? This must depend upon some peculiar condition to which the body is diurnally, or I should say nocturnally, subject. Dr. W. P. Alison has suggested that 'the blood is then perhaps in fullest quantity, its movements slow, and its congestion in internal parts easiest, because it is least solicited to the organs of sense or locomotion.' But there seems to be another reason for this remarkable circumstance.

Respiration is mainly an automatic act; yet it also obeys the will. During sleep this moderating influence of the will is suspended. Those changes of posture, and those voluntary alterations in the rate of breathing, which are wanted to balance and correct the commencing derangement of the pulmonary circulation, and which are prompted at once during the waking state, do not occur: until at length the derangement reaches that pitch at which it provokes spasmodic contraction, and rouses the sufferer.

Many of these asthmatic patients have just healthy lung enough to breathe with, in tolerable ease and comfort, under ordinary circumstances; and dyspnœa is brought on whenever even a slight additional demand upon the respiration any how arises. Hence, as I stated before, flatulent distension of the intestines, undue repletion of the stomach by an excessive meal, the recumbent posture, all of which cause pressure against the under surface of the diaphragm, may suffice to bring on the fit. Hence also, probably, in part, its frequent occurrence in the night-time.

In like manner, any extraordinary task imposed upon a heart which is barely equal to its functions while the body is in repose may induce a paroxysm of asthmatic dyspnœa.

Asthma is a disorder which is incident to both sexes, but it is much more common in men than in women. It is incident to all ages also; but it belongs more to adolescence, and to the middle portion of life, than to its extremes. It is not, I think, a common disease prior to the age of puberty; yet instances of it do occur at an earlier period than that. I have lately seen a boy of eight or nine, who has had several well-marked attacks of pure asthma. Nor does it often *begin* to show itself in old age. Dr. Salter states that 'a few days after birth the infant may give unmistakeable evidence of asthma; or the old man, after spending a long life without an asthmatic symptom, may become its victim.' Sometimes, after plaguing the subject of it for several years, it leaves him altogether. The chronic dyspnœa, with occasional irregular exacerbations, which is so frequent a disorder among old people, and which always depends upon organic disease, is not to be confounded with true asthma. It is said that asthmatic persons are exempt from phthisis; and I understand that one physician in this town, who announces that consumption is curable, maintains the doctrine of the incompatibility of phthisis and asthma; and endeavours to bring about the latter, by causing emphysema of the lungs, in order that he may protect his clients from the former. It *may* be that persons affected with genuine asthma seldom become the victims of pulmonary consumption; but I am sure the rule is not universal. One of my earliest friends had from time to time, while we were schoolfellows, and long afterwards, the most exquisite fits of spasmodic asthma. At length, when he was between thirty and forty years old, they wholly ceased: whereupon he greatly congratulated himself. But they only yielded before a worse disease. He began in a few months to spit blood; and in a few months more he died of well-marked phthisis. Our lamented principal, the late Hugh James Rose, afforded another sad example of the same sequence. And I have known two or three families in which one individual was subject to asthma, while others were scrofulous and phthisical.

The exciting causes of the asthmatic paroxysm are manifold; and some of them curious. They seem to be reducible to two classes. 1. Particular states of the atmosphere, which irritate or offend the mucous surface of the air-passages, or rather, some of the fibrils of the par vagum. 2. Certain subtle influences which affect in a peculiar manner the nervous system. All the known exciting causes of catarrh are therefore likely to bring on attacks of asthma in the predisposed. But there is a singular caprice in asthmatic patients in this respect. Some persons, subject to the disorder, are unable to breathe in the thick smoky atmosphere of London; require a high and clear situation; and respire easiest in 'the difficult air of the keen mountain-top.' Others can nowhere breathe so comfortably as in low moist places: in some of the streets by the water-side in the city, for instance. The friend to whom I lately alluded lived at Newmarket, a most exposed and bleak spot. But if he left it, and attempted to sleep in a strange place, he never was certain that he should not be assailed in the night by his well-known enemy. So that there were towns in which, after experiencing the effect of their atmosphere, he dared not sleep; and there were others in which he knew he might go to bed in security. It would have been difficult, I believe, to point out any essential difference between some of those localities. His lungs, however, formed an infallible eudiometer. Another college acquaintance of mine, much tormented by asthma, is equally sensible to these inscrutable influences. Two inns in Cambridge are named respectively the Red Lion and the Eagle. He can sleep in one of them, and not in the other. Nay, he is thus variously affected within much narrower limits. He assures me that, when in Paris, he never escapes a fit of asthma if he attempts to sleep in the back part of Meurice's Hotel, and never suffers if he sleeps in a front room. Dover Street suits him; Clarges Street does not. He cannot rest in Manchester Square. This he attributes to its being built upon piles. Whether it really has such a foundation I do not know. And agencies still more slight and subtle are enough to set the springs of these seizures in motion. The mere absence of light, for instance. Laennec tells of a man who invariably was roused from his sleep by a paroxysm of asthma, if his lamp were extinguished; or if his chamber-door were shut. The consciousness that the customary preventive remedy was not at hand, has, apparently, sufficed to bring on a fit.

It may seem a strange and an unlikely, but it is an undeniable fact that to many, or to most asthmatic sufferers, the foul and murky atmosphere of this crowded city proves more beneficent than the clear and purer air of the country. Men in much practice here are familiar with the histories of patients who, tormented at home and coming to London for advice, find themselves on their arrival suddenly and thoroughly freed from their accustomed malady; and are sometimes vexed that, how long soever they may wait, they get no opportunity of letting their chosen physician witness an attack. On their return, however, the disease does not fail to resume at once its habitual tyranny. In these cases the densest, lowest, and most foggy parts of the town—Thames Street, for example—furnish generally the surest defence against the assaults of the disease. This sanative quality is not peculiar to London, but is shared with other large, closely-peopled, manufacturing, and smoky towns, such as Manchester, Liverpool, and Glasgow. On the other hand, a certain, but a smaller number of asthmatics are safer when in pure inland air; and a few, and but a few, find a specific remedy for their disease in the air of the sea-coast. Probably every case of asthma is curable by the atmosphere of some place or places, if only they could be discovered: and nothing but actual trials can discover them.

There are many persons who never fail to become asthmatic if they inhale certain effluvia. Particles of ipecacuan floating in the atmosphere, or (what is perhaps the same thing) its mere odour, are insupportable to many. They are thrown into a paroxysm of dyspnœa if they enter a room where that drug is under preparation. I think I mentioned before a certain laboratory-man at St. Bartholomew's Hospital who possessed this peculiar and inopportune susceptibility: he was obliged to fly the place whenever ipecacuan was about. Most persons, probably, who have had much experience in druggists' shops, are acquainted with similar examples: so that the influence of ipecacuan in exciting fits of difficult breathing, resembling asthma, is undoubted, and common to *many* constitutions. We might as well speak of ipecacuan asthma, as of hay asthma, which is a precisely analogous affection. Dr. Marshall Hall calls attention to the familiar but interesting fact, that the same drug, ipecacuan, acting upon the *gastric* branches of the par vagum, excites the reflex spasmodic act of *vomiting.*

Conversely, the mother of a friend of mine was cured of her asthma by ipecacuan administered for that purpose by the great Dr. Darwin. This must be 'nuts' to the disciples of Hahnemann.

Not only do certain *vegetable*—but some *animal* effluvia also, afflict some persons in the same way. One of the grandsons of the celebrated Sir Richard Arkwright was subject to asthma, and he always suffered an attack when he came near a sweating horse, or a man who had been recently riding a sweating horse. Another of the same family becomes asthmatic whenever a cat is in the room with him. Here the hereditary tendency crops out.

The publication of the first edition of Dr. Salter's work brought him a number of communications showing that the agency, in this manner, of bodily emanations belongs to a wider category of animals than had generally been supposed. That cats and rabbits may cause such effects was well known previously: but the list has now been extended to horses, sheep, oxen, dogs, guinea-pigs, hares, probably deer, and wild beasts. All of these have been known to exercise an irritative influence upon certain persons, and certain persons only. It should however be noticed with respect to all these vegetable and animal emanations that they excite something more than mere or pure asthma; the eyes, nostrils, and throat sympathise in the irritation: severe coryza is added to the asthmatic symptoms.

If the asthmatic paroxysm may, by a strong figure of speech, be charged with caprice in respect of its responses to various and even opposite exciting causes, it is chargeable with no less caprice in respect of its submission to remedies. That which in one man will stop the paroxysm at once, has no effect at all upon a precisely similar attack in another. I have given you instances of the caprice of the disorder in reference to different *places* in the same person—in reference to different persons in the same place. It is so with drugs also.

Among the numerous reputed cures for these attacks, *stramonium* has held, and holds, a very high place. This herb, the Datura stramonium, and another species of the same genus, the Datura ferox, had long been employed in India as a remedy for asthma. And when it was introduced into this country, about the beginning of the present century, it was cried up as a specific ; and everybody who called himself asthmatic began to smoke stramonium : for that is the way in which it has been chiefly employed. The leaves and stalks are cut and put into a pipe, and smoked like tobacco. Stramonium cigars, too, are fabricated. The smoke descends, of course, into the lungs ; and when the saliva is swallowed, the remedy is introduced into the system in that way also.

Stramonium thus used, sometimes fails altogether : sometimes calms the paroxysm like a charm. The late Dr. W. Babington told me of a patient of his who had been grievously harassed for a series of years, by asthma, but who declared to him, after he had made a fair trial of stramonium, that he no longer 'cared a fig' for his asthma ; which he could always stop in a moment. So a Mr. Sills, in a collection of communications relative to the Datura stramonium, published in London in 1811, states, that he had been a great sufferer from asthma: that the fits usually continued, with short interruptions, from thirty-six hours to three days and nights successively ; during which time, he had often, in the seeming agonies of death, given himself over, and even wished for that termination of his miseries. But having at length discovered the virtues of stramonium, he uses this strong language :—'In truth, the asthma is destroyed. I never experienced any ill effects whatever from the use of the remedy ; and I would rather be without life than without stramonium.'

So popular has been the reputation of this drug that most patients subject to asthma try it of their own accord. We have still to learn why it is so serviceable in some cases, and so entirely useless, or even hurtful, in others.

It has been suggested, as one clue towards determining the particular kind of case to which the stramonium is applicable, that it succeeds when it causes *expectoration* ; and not otherwise. How this may be I cannot tell you. Sir John Forbes quotes the following passage of a letter from an old and intelligent asthmatic to himself :—'Smoking tobacco or stramonium is sure to give relief, if it produce expectoration ; and it will generally do so if, the moment I awake (i.e. in the incipient paroxysm), I begin to smoke, and continue to do so for three or four hours. Smoking, I am able to say, after fifteen years' practice, and suffering as much as mortal can suffer and not die, is the best remedy for asthma *if it can be relieved by expectoration*. I have been in the hands of all the doctors of the place for fifteen years; and still I say, *smoke*.'

One of our most esteemed and accomplished physicians—now in the retirement of old age—has been kept alive, I verily believe, by the help of *tatula*, a third species of Datura, much in vogue of late years, and in many cases singularly efficacious. A few whiffs of his tatula cigar bring on expectoration, and stop and banish every commencing attack of asthma. And then he ceases to smoke, for else he is presently reminded by vertiginous and sinking sensations that he is inhaling a strong poison.

In this example are suggested two important rules which need to be observed in all cases. Whatever may be the remedy selected, it should be applied at the very earliest possible moment, and it should not be pushed beyond its complete specific effect. An asthmatic paroxysm may be compared to a railway train, which is checked with ease while its movement is just beginning; with increasing difficulty as it goes on towards its maximum of speed; and the steam must be shut off when the terminus is at hand. It is hard to arrest an attack of asthma after it has attained any intensity of violence; it is imprudent to continue a powerful medicine after its immediate purpose is reached.

Of calming vapours thus applied by inhalation to the very parts affected—and operating either immediately upon the nervous fibrils there distributed, or mediately after admission into the blood—the vapour of chloroform, in respect of its soothing power, is one of the chief. Inspired in moderate quantity, far less than is requisite to produce general insensibility, it has been found singularly effective in allaying at once the spasmodic distress of an asthma-fit. But it is manifest that this is a remedy too potent and subtle to be entrusted to the discretion of the patient himself. Its successful use is sometimes followed by immediate nausea, or by discomfort of some kind. Repetition diminishes its virtue, and frequent repetition has been found gradually to induce a condition very near akin to delirium tremens.

Less hazardous certainly, less costly, less difficult therefore of attainment by the many, but less sure also and somewhat less rapid in its result, is the respiration of air impregnated with the fumes of burning nitre. The mode of effecting this is simple enough. Pieces of blotting-paper, each as big as one's hand, are to be dipped in a saturated solution of the nitrate of potash—and then dried. One of these papers, being placed on an earthenware plate, and ignited and allowed to smoulder—the fumes presently diffuse themselves throughout the room; and within a quarter of an hour their influence, in many cases, is rendered evident, in 'clearing the passages, and gradually opening the air tubes.'

Many striking instances of the value of this expedient might be given you, if our time permitted. I will restrict myself to one. A friend of mine, himself a London physician, has a son who almost from his infancy has been very subject to asthma, which is controlled by nitre fumes. They sleep in adjoining bed-rooms. At that time in the early morning when the attack is wont to come on, the wakeful anxious father listens for and hears in his son's altered manner of breathing, the earliest intimation of the coming trouble, rises immediately and lights his nitre paper in the son's chamber, and in five or ten minutes the threatened or incipient paroxysm is extinguished, the sleeper sleeping on in blissful ignorance of what has happened.

Any degree of bronchial inflammation (which I ought to have told you is a very common *predisposing* cause of asthmatic paroxysms) is believed to mar or prevent the success of this nitre fumigation.

For persons who may desire to prepare the fumigating paper for themselves, Dr. Salter gives some useful directions. The porous paper should not be too thin, or it will not hold nitre enough; nor too thick, or the smoke will be too dense and irritating; nor contain any woolly material, or its smell will be offensive and stifling. The common red blotting paper, of moderate thickness, answers very well. The saturated solution should be made with water of the ordinary temperature. If warm water be used, too much nitre is taken up, the paper is too strongly impregnated with it, and burns with a spluttering explosive flame, instead of smouldering away. Lastly, the prepared paper should be kept dry.

The compound spirit of sulphuric æther, Hoffman's anodyne, swallowed into the stomach, is an old and popular remedy. Dr. Salter says that he has given it in scores of cases, but in one case only did it do any good; in that case it always acted like a charm.

He objects to opium; yet confesses that it is sometimes of signal service. Sleep favours the asthmatic paroxysm. Opium causes and deepens sleep. Both sleep and opium strengthen the tendency to reflex spasm. Dr. Salter's experience has led him to the conclusion that opium does harm in pure unmixed asthma; and does good only by calming bronchial inflammation, when that is present and provocative of bronchial spasm.

If sleep encourages asthma, so also it may be conjectured would the sleep-compelling chloral.

Strong black coffee is a common domestic remedy for asthma. The friend and schoolfellow already mentioned used to take it in considerable quantities, and, he assured me, with great benefit and relief. It is a safe, and simple, and grateful remedy, and has numerous testimonies from medical practitioners in its favour. But it is much less sure than many other remedies.

Galvanism was once in fashion. The only patient who ever tried it under my own eye was this same friend. He insisted upon being galvanised when his fits were quite absent,—the galvanism brought one on immediately.

There are yet other drugs which have unquestionable power in quelling the paroxysms of asthma: antimony, tobacco, the lobelia inflata. Of these I have no personal experience. They all act in the same way: loosening the muscles, and therefore relaxing all spasm, by causing deadly faintness and prostration, cold sweats, intermitting pulse, nausea worse even than that of sea-sickness, and persistent vomitings. In fact, they act as poisons. The sensations from the remedy are scarcely less horrible than those from the disease; and the remedy is even more dangerous than the disease. An attack of pure asthma seldom, perhaps never, kills: but there have been in this country several instances of death caused by lobelia, which had been administered by a quack doctor. I should never think of prescribing any of the three for the cure of asthma, unless all milder means had been tried in vain. When they are given at all the patient must be closely watched, and the drug withheld so soon as its poisonous operation begins to show itself. There is this especial objection to tobacco, that habitual smokers of it become seasoned to its influence, and require proportionally large and perilous doses in order to obtain the requisite depressing effect.

The iodide of potassium is a favourite remedy for asthma with some physicians. It is worthy therefore of trial in cases that have proved otherwise intractable.

If we can shorten or mitigate the paroxysms we do our patient a most essential service, and spare him a great deal of suffering. And during the intervals between the fits, we must endeavour to prevent their recurrence.

For this purpose, I can only just hint at the principle on which we should go. In the simple form of the complaint, when it is apparently uncomplicated with organic disease, we must caution the patient against whatever has a tendency to disturb his general health. He must be temperate in all things; he must be careful in regulating his digestive organs; he must set himself to discover what localities suit him best; and avoid those which experience has shown to disagree with him. And if any one preventive measure be likely to fortify him against his malady, I believe that measure will oftenest be found in the shower-bath, employed in the way which I formerly recommended.

If the asthma occur in connection with any obvious pulmonary or cardiac disease, we must, in addition to the means I have now been adverting to, apply ourselves to the mitigation of such superadded disease. And with respect to this I have nothing more to offer.

I go next to the morbid conditions of the *œsophagus*, so far as they concern the physician; and these morbid conditions are not many. The œsophagus lies partly in the chest, and partly in the belly, and therefore may very properly close the subject of thoracic diseases, and introduce those of the abdomen.

The œsophagus is less liable to disease than any other part perhaps of the alimentary canal. It differs somewhat in structure (as you know) from all other parts. Its mucous membrane is provided with a thick epithelium, which extends a little beyond the cardiac orifice of the stomach. Beneath lie a dense web of areolar tissue, and two layers of muscular fibres; the one layer being disposed circularly around the tube—the other longitudinally, in the direction of its axis. In some cases there are pouches found in the sides of the œsophagus, formed apparently by a kind of hernia of the mucous membrane, between the separated fibres of the muscular coat. These are not common, however, and scarcely worth mentioning, except that their existence has been supposed to have a possible connection with a curious phenomenon, peculiar to some persons; the power, viz., of *ruminating*; the power of bringing into the mouth again, by a voluntary effort, food which has been for some time swallowed; as cows, and the rest of the *ruminantia* do. There are but few individuals of the human species who possess this faculty; there are but few who have appendices to their œsophagus. Whether the phenomenon in question belongs to these last few has never been determined; but as the possible connection of the two circumstances has been suggested, it is well for you to be aware of it, that you may refute or verify the notion, in case you ever have the fortune to examine the dead body of a person who had the power of ruminating.*

The covering of cuticle protects the œsophagus from the injurious influence of matters passing over it, which might otherwise be hurtful. The morbid state for which we are most often consulted is stricture, *actual* or *spasmodic*. I believe that the œsophagus is very little subject to inflammation, except from mechanical violence or chemical injury. I have seen a few cases, however, in which I inferred a spontaneous inflammatory condition of the tube, from the symptoms complained of; which were a sense of heat and pricking exactly in the course of the œsophagus, felt between the shoulders, and in front precisely in that track (the patients said) where a potato, swallowed too hot, gives pain while it is descending into the stomach. With these symptoms there was some degree of dysphagia, not explained by anything visible in the throat or pharynx; and some degree of fever. In all the instances of this kind that I have met with, the symptoms have yielded in a few days to abstinence, purgatives, and the application of leeches opposite the track of the œsophagus.

This part of the alimentary canal often suffers severe injury from the deglutition of certain poisons, especially the corrosive poisons; the strong mineral acids, for example; or the caustic alkalies. We have, in the museum of the College, some very interesting specimens of the effects of these destructive substances. Sometimes, when the quantity of the poison has been small, and its transit rapid, the cuticular lining alone of the gullet is destroyed. It is shrivelled up, broken into fragments, abraded. At other times, the subjacent textures are affected, and ulceration takes place, which at length heals, and leaves a permanent, and generally a progressive constriction of the œsophagus: and sometimes the whole of the internal membranes slough away, and are discharged in one continuous tube, from the mouth; and yet the patient survives for some time. My colleague, Dr. Wilson, had a case of that kind. The patient, a young woman, swallowed about a table-spoonful of oil of vitriol. A week afterwards, she brought up, during a paroxysm of choking cough, a complete cast of the gullet, with ragged ends; or rather the gullet itself. Some of the muscular fibres of the œsophagus were plainly visible on the outside of this tubular slough, in its recent state. She lived eleven months afterwards, swallowing all that time with difficulty and pain, and subsisting on slops and soft food. Yet at one period she certainly gained flesh. After her death, the channel, as it remained after the injury, was taken out of the body and examined. They are both before you;—the original slough, and the ultimate gullet. The latter was formed by a surface which consisted of an irregular cicatrix. The tube was contracted considerably in the lower two-thirds of its course.

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* An instance of the occurrence of a large pouch in the œsophagus, with frequent regurgitation of undigested food, has been detailed, in the 30th volume of the *Medico-Chirurgical Transactions*, by Mr. Worthington, of Lowestoft: who refers to another example of a like combination, recorded by Mr. Ludlow, of Bristol, in the 3rd volume of the *Medical Observations and Inquiries*.

When patients have suffered inflammation and ulceration of the œsophagus from these causes, and do not perish at the time, they are very liable indeed to have their existence abridged by the occurrence of stricture of the gullet, which goes on slowly increasing, until no food can pass it, and then of course the patient dies of starvation. I show you here an œsophagus taken from a man whose case I had opportunities of observing from the beginning. He was under the care of Dr. Macmichael, in the Middlesex Hospital. He was brought there in November, 1830, having swallowed, half an hour before, a solution of the impure carbonate of potass, which had been made for the purpose of cleaning paint, and which he had mistaken for beer. Not more than a table-spoonful passed the fauces, and probably none of the poison reached the stomach. He suffered severely, and was in considerable peril for several days, in consequence of inflammation of the fauces and epiglottis; but this gradually subsided, and he went out apparently well. From what I knew, however, of the results of such cases, I ventured to predict that this man would, sooner or later, come back with stricture of the œsophagus. He had always pointed out a spot about half way down the sternum, where he said the oil of tartar had caused him extreme pain at the very first, and below which he had not felt it.

Accordingly, I was not surprised to see the poor fellow at the hospital in February, 1834, attending as an out-patient. He came there, he told me, because in eating some soup, he had accidentally swallowed, without chewing it, a piece of carrot, which lodged in its way down, and which it became necessary to push onwards into the stomach by means of a probang. Morsels of food had stuck in the same spot before, and it was the very spot where he felt the effects of the caustic at the time of the accident. He looked tolerably stout and healthy; but said that since swallowing the potass, he had never been the man he was before.

He continued to make his appearance, from time to time, at the hospital, with similar symptoms, till the 5th of last December (1836), when he was brought there insensible, and evidently dying. We could obtain no satisfactory account of his recent symptoms. He had the mark of a blister, however, on his left side; and upon closely examining him it was plain that that side was full of fluid. It was perfectly motionless in respiration; it was palpably larger than the right side; it yielded everywhere a dull sound on percussion; and no vesicular breathing whatever could be heard there by the ear. The respiration on the right side was puerile; and the beating of his heart, with a systolic bellows-sound, was audible on the right of the sternum.

Although I was certain that the left pleura was full of liquid of some kind, I did not have the thorax punctured: because, in the first place, he was manifestly *in articulo mortis*, and I thought that his death, which was certain, might be attributed to the operation; and secondly, because he was not dying of *suffocation*. His breathing was not laborious or much distressed; but he was dying of coma, and his extremities were already cold, and his pulse was fluttering. I conjectured that an ulcer of the œsophagus had made its way into the pleura, and caused inflammation there. But my conjecture was wrong.

I will mention the main particulars of the examination of the dead body, because the case was, in several respects, an interesting one.

There was a considerable quantity of serous fluid in the meshes of the pia mater, beneath the arachnoid; and there was some liquid of the same kind in the lateral ventricles. No other diseased condition could be detected in the brain. The effusion was sufficient, supposing it to have come on suddenly, to explain the coma.

I had the ribs sawn away on the right side, leaving their cartilages attached to the sternum; and then we saw plainly that the heart and mediastinum were thrust over, about four inches by measurement beyond the mesial line, to the right side. The body was on its back. It was easy to perceive how a pleura thus full of fluid must oppress the lung of the other side, especially when assisted by the force of gravity. The left cavity was distended by a greyish coloured and most offensive fluid, of the consistence of gruel; the pleura pulmonalis was covered by a layer of coagulable lymph; and the lung was flattened against the vertebral column. We could not discover any communication between the cavity of the pleura and the œsophagus or air-tubes.

About the middle part of the œsophagus there was a distinct stricture, occupying about half an inch of the tube. Through this portion it was impossible to push one's little finger; which elsewhere found a loose and ready passage.

In this case, the man did not die of the stricture; but he would have done so had not another disease carried him off. I do not know why the constriction, after it has once taken place, should go on continually increasing; yet it seems to be so. In his 'Surgical Observations' Sir Charles Bell mentions three cases like that just described. In one of them, where soap-lees had been the substance swallowed, death took place by starvation from stricture of the gullet, twenty years afterwards; and Sir C. Bell had no doubt that the stricture had originated in the chemical injury inflicted by the soap-lees.

When the symptoms of stricture come on in these cases, physic can do almost nothing. It may be well, for once, to pass an exploring bougie into the gullet: but any repetitions of that expedient, made with the view of dilating a discovered stricture, or of preventing any further narrowing, would be hurtful rather than beneficial. The patient dies at last of inanition. His miserable existence may perhaps be protracted a little, by injecting nutritive enemata into the rectum. Sometimes the œsophagus ulcerates through, and a communication is formed between it and the neighbouring parts.

But the œsophagus, like the urethra, and like the bronchial tubes,—like every canal, indeed, in the living body that is surrounded by circular muscular fibres,—is liable to temporary constriction and closure, by the spasmodic action of its own muscles; and this affection is, of course, a far less formidable one than the last.

Patients who are subject to spasmodic stricture of the œsophagus experience occasionally, in some point or other of that tube, a sensation as if there were a knot; or sometimes a feeling as if some solid substance were ascending from the stomach towards the pharynx. If they happen to be then engaged in eating, the morsels of food, after mastication, readily pass the pharynx: but, at a certain distance down the gullet, they stop, and occasion pain, which is felt between the shoulders, or distinctly in the passage itself. Great anxiety and distress accompany this stoppage: and the food is often ejected by a reversed action of the œsophagus.

The symptoms, in fact, are identical with those which result from permanent stricture of the gullet, except that *they* are *not* permanent. When the stricture is organic and abiding, the symptoms occur during or after every meal. When it is simply spasmodic, they come and go, capriciously, we often cannot conjecture why or wherefore; after the fashion of other spasmodic ailments.

Spasmodic stricture may be independent of any disease of structure in any part of the body; but it is of some importance to be aware that it may also be symptomatic of very serious organic changes. Mr. Mayo relates the case of a young man 'who had difficulty of swallowing; he could get down liquid food only; and that not without an effort. A bougie being introduced, some resistance was found at the upper opening of the œsophagus, but it yielded: the resistance was spasmodic, and depended upon neighbouring irritation caused by ulceration in the interior of the larynx. The use of the bougie for a few days, with appropriate remedies to the larynx, removed the dysphagia.'

The purely spasmodic cases occur principally in persons of a moveable constitution; in young women whose uterine functions are deranged, and who are liable to hysteria. The remedies for hysteria will prove remedies for the spasm of the œsophagus also. And whatever is calculated to excite ordinary hysterical symptoms, whatever tends to render the system weak and irritable, will tend to aggravate the œsophageal stricture. I alluded to such cases in a former lecture. I give you another, related by Sir Benjamin Brodie. A lady consulted him, unable to swallow the smallest morsel of solid food; and swallowing liquids not without great difficulty. The symptoms had been coming on upwards of three years. A full-sized œsophagus bougie being introduced, entered the stomach without meeting the slightest impediment. This lady's face was pale and bleached: her feet were œdematous. She had long laboured under internal piles, from which repeated discharges of blood had taken place. Under the use of remedies which relieved the piles and the bleeding, the difficulty of swallowing went away.

It is a singular, and it might, if more frequent, be a puzzling circumstance, that very nearly the same symptoms which occur when the œsophagus is permanently or temporarily constricted, happen also sometimes under a totally opposite condition of that tube; I mean its dilatation into a large, inelastic, inert bag. One remarkable example of this I witnessed, in a woman whom I attended in conjunction with Mr. Mayo, some years ago, in the Middlesex Hospital. The case has been fully described by Mr. Mayo, in the third volume of the 'Medical Gazette;' and more briefly in his 'Outlines of Pathology.' She was thirty-three years old. She was brought to the hospital in a state of extreme feebleness and emaciation. They who brought her said that for the preceding month she appeared to keep down nothing. What she took as food seemed to her to stop in the gullet; and, after a few minutes, it returned. A large œsophagus bougie passed readily into the stomach. She could swallow liquids more easily than solid food. When she took a small quantity it did not feel to her as if it reached the stomach; and in three or four minutes it was invariably rejected. The vomiting was not preceded by nausea, although in its progress it had the appearance of ordinary retching. She craved for food and drink, and seemed literally starving. The complaint had begun ten years before, during her pregnancy, and had gradually got worse. The belly was so shrunk that the umbilicus was not more than an inch distant from the spine. There was no enlargement or hardness about the stomach; no particular tenderness on pressure of the epigastrium; nor any uneasiness there. She died, utterly extenuated, sixteen days after her admission.

The stomach was found small, and contracted at its middle to the breadth of an inch and half. The upper part of the duodenum was but half the ordinary size of the ileum. The œsophagus I show you, turned inside out.

It was enlarged to an extraordinary degree of amplitude, as you perceive. At and near each extremity it was healthy, and of its natural size. Intermediately the lining tunic was thickened and opaque, with numerous depressions in it. The muscular fibres, which appeared to have multiplied with the expansion of the canal, were of their natural colour and thickness.

Here is another preparation: a dilated œsophagus with cancerous degeneration of the cardiac orifice of the stomach. I do not know its history; but the mechanism of such dilatation is intelligible enough. The food, unable to pass out of the gullet into the stomach—or passing slowly and uncertainly—the tube behind it is habitually distended, and loses at length its proper contractility. I saw last summer, in consultation with Mr. Mayo, an old gentleman, of seventy, who, for two years, had experienced difficulty in getting food into his stomach. He would eat a few mouthfuls very well; and, then, of a sudden, the next mouthful, after passing the pharynx, would stop just short of the stomach; and a sensation of swelling would arise in the lower and middle part of the œsophagus; and presently up the mouthful would come again. Sometimes, by waiting quietly a little while, the morsel would go on; sometimes he could wash it forwards by a gulp or two of drink: but if once the food got fairly into the stomach he had no further trouble with it. This gentleman had no discoverable disease of the heart or lungs. He gradually grew worse. At last he began to vomit grumous matters, resembling coffee-grounds, and soon died. He was at some little distance from London at the time, and the body was not (I believe) examined. I have no doubt that he had malignant disease of the cardia; and I think it probable that his œsophagus was dilated. I had a female patient about two years ago in the hospital with very similar symptoms; and her stomach was found to be full of cancerous disorganisation. The state of her gullet is not recorded. We are apt, in such cases, to satisfy ourselves with ascertaining the gastric disease, without carefully examining that part of the alimentary canal which lies above.

For maladies like these medicine has no cure. Opiates may give comfort, and promote the euthanasia: and that is all.

LECTURE LIX.

Diseases of the Abdomen; sometimes difficult to identify. Method of investigating these diseases; by the eye, the hand, the ear. Inflammation of the Peritoneum; its symptoms; and causes. Puerperal Peritonitis. Peritonitis from Perforation.

I AM about to consider the diseases of the abdomen. The organs contained in this cavity of the body are not vital organs in the same sense in which the brain, the heart, and the lungs are vital. That is to say, the *functions* of the abdominal viscera will bear to be suspended for some considerable time, without the extinguishment of life. But these parts are subject to numerous diseases, some of which are apt to be quickly fatal, and others carry with them a vast amount, and very severe kinds, of suffering.

The parietes of the fore-part of the belly being soft and flexible, you might naturally suppose that the physical morbid conditions of the organs they cover would submit themselves to an easy diagnosis; that the sense of touch, exercised through these yielding walls, would detect alterations of bulk, of form, or of place, in the subjacent viscera, with much facility and exactness. But the truth is, that since the discovery of the method of auscultation, the diseases of the abdomen are much more hard to discriminate than the diseases of the thorax. The reason of this is to be found in the number and complexity of the parts contained in the abdomen; the loose manner in which some of them are packed; and the consequent readiness with which they pass out of their proper and natural situations. It is necessary that I should say a few words, but I shall not detain you long, respecting the *mode of examining* the abdomen, with the purpose of investigating its diseases.

In the description of symptoms, we are often obliged to speak of particular portions of the abdomen: and it will be of future convenience to us if we make ourselves acquainted, at starting, with such a superficial map, marking out the topography of the belly, as I exhibited to you some time since, in reference to the chest. Draw a horizontal line round the body, touching the extremity of the ensiform cartilage; and this will form the superior boundary of the abdomen, thus roughly defined for practical purposes. Draw another such line round the body, horizontally, touching the lower edge of the last false ribs: and a third touching the crest of each ilium. We then have three horizontal *zones* formed. These must be further divided by vertical lines: one on each side from the anterior spinous process of the ilium perpendicularly upwards. Each zone will thus be subdivided into three regions. The middle region of the upper zone is the *epigastric* region; on either side are the *hypochondria.* The middle region of the middle zone is the *umbilical* region; the *iliac* regions or the *flanks* lie to the right and left of it. The *hypogastric* region is the middle region of the lowermost zone; and the *inguinal* regions are contiguous to it. This is all the division which is ncessary.

Now, independently of the *general* signs of diseases that have their seat in the abdomen, we are greatly assisted in many cases by the *physical* signs. I shall take a very brief survey of the modes by which these physical signs are collected. They are derived from the exercise of the three senses of sight, of touch, and of hearing.

The sense of sight supplies, occasionally, very valuable information; and in all serious and equivocal cases we must not dispense with its use. We are not, indeed, to make an ocular inspection of the *naked* abdomen unnecessarily: and I hold it superfluous to admonish you that when we do avail ourselves of that mode of investigation, especially in the case of women, we are bound to do so with the most scrupulous care not to offend the patient's delicacy. We may sometimes ascertain all that is required concerning the *movements, size*, and *shape* of the abdomen, without removing the undermost garments.

This rule applies, indeed, to all parts of the body that are ordinarily covered by the dress.

I was lately consulted by a lady, who told me she had, on the rear of her person, a painful boil. She thought any physician ought to be competent to prescribe for a boil, without wanting to see it. But she seemed very ill, and her sister told me that the boil had lasted a fortnight, and was a very large one; so that I was obliged to press for an inspection. And I found—a boil sure enough, but of that gigantic and formidable species which we call *carbuncle.*

M. Rostan relates a case still more in point. Going round the wards of his hospital, he came to an old woman, who was complaining of severe pain in the abdomen, towards the left iliac region. Her face was flushed, her skin hot, her pulse strong and frequent, her tongue dry; and she was very thirsty. The abdominal pain was aggravated by pressure, and by the movements of the patient. Upon these data, Rostan founded his diagnosis. He concluded that the case was one of acute abdominal inflammation; and he prescribed accordingly; and with befitting energy. One of the pupils, however, lingered behind him: and having removed the woman's chemise, in order to examine the seat of pain, he discovered that all the symptoms proceeded in reality from a very harmless, though troublesome, disorder, *herpes zoster*; what is vulgarly called *the shingles.*

> Vestis adempta est,
> Quâ positâ, nudo patuit cum corpore crimen.

The size and the symmetry, or want of symmetry, of the abdomen, are discoverable by the eye. The spectacle of numerous or enlarged veins upon its surface may give insight into the character of morbid conditions lying hid beneath.

In the second place, we gather very important instruction from the sense of *touch.* We learn the existence and the size of *tumours*; we approximate to a knowledge of their consistence, whether it be solid or fluid; we determine whether they be moveable or fixed, painful or indolent, hard or soft, smooth or uneven, pulsating or not. We ascertain whether the surface be hot or cold. In order to make palpation most effectual, the patient should be placed in the most favourable posture for its performance. He should lie on his back, with his head a little raised by a pillow, and with his knees up. In this position, the abdominal muscles are relaxed and unstrung: and the patient is to be cautioned not to do anything which may make them tense. Sometimes, in spite of this caution, and in spite, probably, of the patient's endeavours to obey it, the recti muscles remain so tightly contracted as to prevent any satisfactory examination of the parts beneath them. The very occurrence of this instinctive striving against the pressure of our hand may be taken as a ground of suspicion that those parts are not in a healthy state. We must take care, when the muscles are thus obstinately rigid, not to mistake the swelling central portions of the recti, or their well-defined edges, for tumours, or for indications of an enlarged stomach or liver. By a peculiar management of the palpation, we often satisfy ourselves at once of the presence of liquid in the cavity of the peritoneum, or in a cyst: we obtain that sensation which we call *fluctuation.*

The exploration by the sense of touch is very much aided—

often confirmed, sometimes corrected—by evidence which addresses itself to the sense of *hearing*. Sometimes we listen to the natural sounds through a stethoscope: and we may thus decide the important question, whether a pulsating tumour be or be not an aneurism; or the question, sometimes scarcely less important, whether or not a different kind of tumour encloses another living being. Noises resulting from the passage of gaseous or liquid matters through the intestinal canal, often furnish valuable information. But, for the most part, our knowledge respecting the maladies of the abdomen, collected by the sense of hearing, is obtained by listening to sounds which we ourselves produce; in one word, by *percussion*: and mediate percussion, percussion performed through the finger as a ready pleximeter, is *particularly* applicable to the disorders of the abdomen. By this expedient we can tell whereabouts the intestines lie; whether the parts beneath the place percussed be hollow and filled with air, or solid; or, though naturally hollow, distended with liquid. By making the patient change his posture, we are enabled often, through the aid of percussion, to trace fluid effusions hither and thither, when they have changed their relative situation in obedience to the force of gravity; and then we know that they occupy the cavity of the peritoneum. All these points I pass over cursorily, because I must advert to them again when speaking of particular diseases. And I shall proceed, on that account, without further delay, to the consideration of those special diseases.

Consulting your convenience, and my own, rather than any scientific order, I shall take, in succession, the several parts and organs contained in the cavity of the belly, and inquire separately into their diseases; inflammatory, organic, and functional. And I begin with the *peritoneum*; the great serous sac which lines and constitutes the cavity of the abdomen, and in which most of its viscera are wholly or partially enfolded.

Much of what I so lately said of acute inflammation of the pericardium may be said as pertinently of acute inflammation of the peritoneum. The membrane, when healthy, does not manifest any great or spontaneous readiness to take on inflammation. Peritonitis is often ascribed to some known exposure to cold, and especially to cold combined with moisture. But there is reason to believe that in most if not in all such cases some predisposing influence has been at work, some predisposition established, some previous unhealthy state of the membrane itself or of the circulating blood. Among five hundred *post-*

mortem inspections after death from peritonitis, recorded during a period of twenty-five years in Guy's Hospital, Dr. Habershon could not find a single case in which disease appeared to be limited to the peritoneal serous membrane. Peritonitis is very apt to spring up during the progress of what is called Bright's disease.

It often prevails epidemically, and produces great mortality, among parturient women: and this form of the disorder, depending also upon blood contamination, is propagable, and often propagated, by contagion. It is occasionally produced by mechanical injuries inflicted upon the abdomen. Ulceration, irritation from impacted fæces, or other mischief *within* the bowels, may communicate inflammation to their external membrane, and so give rise to general peritonitis. Besides this, a very terrible kind of peritonitis is a frequent result of the extravasation of the contents of the alimentary canal, or of urine, or of bile, or of fluid from a hydatid cyst, into the cavity of the membrane; through apertures that are sometimes made by external violence, but more often are the consequences of the progress of previously-existing disease.

Peritoneal inflammation, beginning anyhow in one spot, is almost sure to transfer itself to any other spot that happens to lie in contact with the first; and is very apt to extend itself rapidly to the whole membrane. It is to this diffused and general inflammation that I now invite your attention. The inflammation tends to the effusion of serum, and of coagulable lymph; it is of the adhesive kind; and its effects are those of distending the peritoneal cavity with fluid—or of gluing its opposite surfaces together so as to obliterate that cavity—or of forming *partial* attachments. In all these respects, the analogy between inflammation of the peritoneum and inflammation of the serous membranes of the thorax—the pleura, and the pericardium—is perfect: and therefore these are points which I shall not dwell upon, except where specific differences arise, from original diversities of structure or of function in the parts affected. I may observe at once, that the morbid conditions which are apt to remain *after* recovery from peritonitis, are sometimes, like some of those which follow pericarditis, *inceptive of further disease*; sometimes, like some of those of the pleuræ, *final*, and limited to their *immediate* influence upon the health and comfort of the individual; or even *protective* against some worse evil.

Formerly, any necessary wounding of the peritoneum was thought to be attended with hazard. It was indeed notorious that it might generally be pierced with impunity by a trocar in the procedure called tapping. Operations for the relief of strangulated hernia are often followed by peritonitis: but other and graver causes than the knife of the surgeon are then at work. Especially was the free access of air to the general membrane dreaded as being highly dangerous. But what happens in cases of ovariotomy is sufficient to show that the peritoneum may be largely exposed to the atmosphere, may be smeared with blood, mucus, and the products of ovarian disease, may be sponged clean, after the operation, as freely as, and much more thoroughly than one might sponge the abdominal cavity in the dead body, without any resulting inflammation. This is now a matter of almost every-day experience. A relation of mine was the subject of ovarian dropsy, for which she was tapped. Shortly afterwards the puncture in the dropsical ovary reopened, and fluid escaped into the general cavity of the peritoneum. Thereupon the diseased ovary was extracted by Mr. Spencer Wells, in the presence of Dr. Arthur Farre. I have been informed by these gentlemen that the whole of the peritoneal surface presented a uniform blush of redness—marking, it may be presumed, that stage of congestion which is just short of actual inflammation. This lady recovered without a single untoward symptom; and has since become, again, the mother of children.

Acute inflammation of the peritoneum is characterised by pain in the abdomen, increased on pressure, and attended with fever. But as these symptoms are common to almost all the inflammatory conditions of the parts contained in the abdomen, we must look for more distinctive circumstances. Cullen defines the disease in this manner: ‘Pyrexia: dolor abdominis, corpore erecto auctus, absque propriis aliarum phlegmasiarum abdominalium signis.’ He concludes that it is the peritoneum simply that is inflamed, when the specific symptoms that indicate inflammation of particular viscera are wanting. It is not inflammation of the liver, for there is no pain of the right hypochondrium in particular, increased by lying on either side, no pain of shoulder, no jaundice, no vomiting perhaps: neither is it inflammation of the bowels or stomach, for there is no disturbed function of the alimentary canal to denote such inflammation.

The pain, Cullen says, is increased when the patient sits up. He might have added, that it is increased also by drawing a long breath, by coughing, sneezing, or straining; and by pressure made with the hand upon the belly. All these circumstances resolve themselves into the same obvious principle; viz., that of pressure aggravating the pain of an inflamed membrane. The erect posture throws the weight of the viscera upon the peritoneum, and tends to stretch parts of it. The pain occasioned by pressure is often excessive: the patient cannot bear even the weight of the bed-clothes. Though the pain is, *at first*, sometimes confined to particular spots, yet generally it soon extends over the whole abdomen, and this is a circumstance of some importance as respects the diagnosis. But *before* the inflammation has become universal, while it is yet restricted to particular spots, the pain is often much increased by pressure made on *other* parts of the abdomen. In truth, in a shut sac of that kind you cannot compress any one part without exercising pressure indirectly upon every other part. The patient cannot sit up, nor, usually, lie on his side; but remains always upon his back: in which position you will perceive that the pressure made by the viscera upon the peritoneum is a *minimum*: is the least possible. He draws up his legs too. And he lies *still*: for *movements* cause pressure, and therefore pain. The descent of the diaphragm in inspiration presses also upon the membrane; and the patient not only complains of the pain thus produced, but, in order to avoid it, gets into a way of breathing by means of his ribs only. So that upon *inspection* of the abdomen, it is perceived that, instead of rising and sinking alternately in respiration, it remains motionless. The phenomenon of *thoracic* inspiration is a symptom of peritonitis. The breathing is necessarily shallow in these cases, and less air being admitted at each movement of respiration, the number of those movements is increased therefore: the breathing is frequent as well as shallow: there are perhaps forty, or even sixty respirations executed in a minute, instead of eighteen or twenty. When we find a person lying only on the back, with the knees up, breathing in this manner, and complaining of tenderness of the belly on pressure, and feverish withal, we may be tolerably sure (unless that person be a hysterical girl) that the peritoneum is inflamed, whatever else may be the matter.

The pain in peritonitis is generally sharp, cutting, or pricking in its character. And independently of any pressure made from without, or caused by any change of posture, this pain is apt to be much aggravated at intervals. This, when the inflammation is general, is sometimes owing to the passage of flatus along the bowel, partially distending it, and stretching the inflamed membrane; so that here also it is really *pressure* which augments the pain.

When you explore the abdomen by pressure, take care not to make the examination *unnecessarily* a source of pain. Press first gently, with the open flat hand; and keep your eyes on the patient's face at the same time. You will perceive by the expression of his features, whether you are hurting him; even before he takes to verbal complaining.

Acute peritonitis generally sets in with well-marked symptoms: smart rigors, and high fever, with a hard and sharp pulse, which very soon becomes frequent, and often becomes feeble, and is sometimes small from the very first. After the disease has continued for a certain time, it is attended with tension and swelling of the belly. The tension and swelling are tympanitic in the earlier stages. You learn this with certainty by mediate percussion. As the disease advances, the enlargement is sometimes occasioned, in part at least, by the effusion of serum: infallible indications of the presence of which may be obtained by the joint employment of the finger and the ear; by palpation and auscultation; and by noticing the difference, as to the results of percussion, caused by alterations of posture.

When the disease is advancing towards a fatal termination, the abdomen often becomes greatly distended; the pulse is exceedingly frequent and feeble; the countenance (which in all the stages of the disorder is expressive of anxiety) becomes pinched and ghastly; cold sweats ensue; and the patient dies at length by asthenia: death beginning at the heart. The mind is often clear to the very last.

Such is the ordinary course of peritonitis. But other symptoms, which I have not mentioned, do sometimes accompany it; arising out of the peculiar circumstances of different cases. Thus sickness and vomiting occur very frequently; and these symptoms are supposed to denote that the peritoneal covering of the stomach is especially implicated: but I question whether this is always a correct inference. When strangury happens, which is not uncommon, that part of the membrane which is reflected over a portion of the bladder is probably involved in the mischief. Inflammation of that part of the peritoneum which lies in the immediate vicinity of the kidneys, may cause, Dr. Abercrombie thinks, suppression of urine.

Respecting some of the points which I have touched upon, I shall make a few, and but a few, further comments.

That awful disorder, *puerperal fever*, is more frequently accompanied with *inflammation of the peritoneum*, than with any other inflammation. This variety of peritonitis necessarily engages the attention of the accoucheur; and it doubtless is more fully considered in the lectures of the Professor of Midwifery than I propose to consider it. Indeed, if you would understand puerperal fever as a whole; its shifting aspects, its single source, and its appropriate management; you must study Dr. Ferguson's masterly and conclusive essay on that subject.

Of forty-four fatal cases of well-marked puerperal fever which fell under the observation of Dr. Robert Lee, and in which the bodies were carefully examined, the *peritoneum and uterine appendages* were found inflamed in thirty-two: i. e., in eight cases out of every eleven. The inflammation commences, no doubt, in the uterine portion of the membrane, and spreads thence over the larger part of its surface. Now this peritoneal inflammation, occurring in women after childbirth, may be accidental and sporadic; or it may prevail in a district epidemically. And a most dreadful and deadly affection it may then become. In either case, the peritonitis may commence a few days, or even a few hours, after parturition. The pain generally begins low in the abdomen, in the situation of the uterus; which may be felt through the abdominal parietes, and is tender on pressure: but soon a universal swelling takes place, and the womb can no longer be distinguished. Cases of this kind, when peritonitis is frequent among women after childbirth in a particular neighbourhood, or in a lying-in hospital, are marked by great depression of the vital powers, and run an irregular course. The nervous system suffers, the sensorium is apt to become affected, and the complaint assumes rather the character of typhus fever than of simple inflammation of the peritoneum. And no wonder; since this variety of peritonitis forms part of a disease which, like typhus fever, is a *general* disease, and results from contamination of the blood. This Dr. Ferguson clearly established. The contamination may originate in the body of the patient herself; the noxious material being supplied by putrid coagula, or portions of placenta, remaining in the uterus. Or some of the products of inflammation may enter the blood-vessels, and constitute the poison. And this it may be difficult, or impossible, to prevent. But, on the other hand, the contamination may arise from without in the way of *contagion*; this horrible malady may be communicated from one parturient woman to another lying in the same room; and, by the intervention of a *third person*, even to a woman in a distant house; and doubtless it is so carried and propagated, in many instances, by midwives and accoucheurs. Now this source of the disorder may be obviated; and therefore it is of the utmost importance that it should be clearly recognised, in order that it may be carefully provided against.

Great differences of opinion formerly existed, and may even still exist, in respect of the contagious quality of puerperal peritonitis; just as great differences used to exist as to the contagiousness of continued fevers, of cholera, of the plague. There are persons who regard the whole notion of contagion as a mere bugbear: and there are others who embrace in their allegations of contagion many more diseases than can be *proved* to be so caused. The same strong assertions are made, the same kind of eagerness is displayed (the same party-spirit, I had almost said), as mark the strife of ordinary politics. It is our serious duty, however, to inquire what is the truth in this matter: for the safety of individuals, and the happiness of whole families, may often hang upon our opinions. I must trouble you, therefore, with a few facts that bear closely upon the subject.

We possess some valuable and highly instructive accounts of epidemics of the kind I am alluding to. One by Dr. Gordon, on the epidemic peritonitis after childbirth, which took place at Aberdeen in the years 1789, 1790. Another by Mr. Hey, on that which happened at Leeds from 1809 to 1812. And a third by the late Dr. Armstrong, on that which was observed in Sunderland and its neighbourhood, in 1813. Dr. Robert Lee has also collected some very interesting facts in reference to the spreading of such peritonitis by contagion. Dr. Gordon had, he affirms, unquestionable proof that the cause of the disease was a specific contagion, and that it did not arise from any noxious constitution of the atmosphere. The disease seized such women only as were visited or delivered by a practitioner, or taken care of by a nurse, who had previously attended patients afflicted with the same disorder. And Dr. Armstrong observed that forty out of the forty-three cases that happened in Sunderland, occurred in the practice of one surgeon and his assistant.

From among other histories—all tending to the same conclusion—brought together by Dr. Lee, I take the following:—

'On March 16, 1831, a medical practitioner, who resides in a populous parish on the outskirts of London, examined the body of a woman who had died a few days after delivery, from inflammation of the peritoneal coat of the uterus. On the morning of March 17 (i.e., the *next* morning), he was called to attend a private patient in labour, who was safely delivered on the same day. On the 19th, she was attacked with severe rigors, great disturbance of the cerebral functions, rapid feeble pulse, with acute pain of the hypogastrium, and a peculiar sallow colour of the whole surface of the body. She died on the fourth day after the attack, on March 22; and, between that period and April 6, the same practitioner attended two other patients, both of whom were attacked by the same disease in a malignant form, and fell victims to it. On March 30 he bled a young woman who had pleurisy: the wound became inflamed after a few days; erysipelatous redness, and swelling, extended from it up the arm; and in four or five days that patient died of phlebitis.'

Mr. Roberton, of Manchester, states the following facts, in a paper in the 'Medical Gazette.'—From December 3, 1830, to January 4, 1831, a midwife attended thirty patients for a public charity. Sixteen of these were attacked with puerperal fever, and they all ultimately died. In the same month, 380 women were delivered by other midwives for that institution; but none of the 380 suffered in the smallest degree. All the sixteen had inflammation of the peritoneal surface of the uterus. So, also, Dr. Robert Lee tells us that, in the last two weeks of September, 1827, five cases came under his observation. All the patients had been attended in labour by the same midwife: and no example of febrile or inflammatory disease of a serious nature occurred during that period among the other patients of the same dispensary, who had been attended by the other midwives belonging to that institution.

Statements of this kind—and they could be largely multiplied—furnish irresistible evidence, that the peritonitis which prevails epidemically among lying-in women, is of a specific nature, and communicable from one person to another. It is observed, also, to reign as an epidemic especially in *Lying-in Hospitals*, and that it occurs at *irregular intervals*, sometimes leaving them quite exempt from its ravages for *years* together.

Indeed, I believe that these cases of puerperal fever occurring in succession to the same practitioner, are examples of something more than ordinary contagion, operating through the medium of a tainted atmosphere. I believe them to be instances of direct inoculation. Recollect, that the hand of the accoucheur is brought, almost of necessity, into frequent contact with the uterine fluids of the newly-made mother. Recollect,—those among you who have examined the interior of the dead body with your own hands,—recollect, with what tenacity the smell, which is thus contracted, clings to the fingers, in spite even of repeated washings; and, whilst this odour remains, there must remain also the matter that produces it. Recollect how minute a quantity of an animal poison may be sufficient to corrupt the whole mass of blood, and fill the body with loathsome and fatal disease. Illustrations will occur to you in the inoculated smallpox, in hydrophobia, in the viper-bite, in the scratches and punctures of the dissecting-room. Recollect the raw and abraded state of the parts concerned in parturition; the interior of the uterus forming a large wound, and presenting, as Cruveilhier has observed, an exact analogy to the surface of a stump after amputation; the more external soft parts bruised and sore. Bear in mind the remarkable fact, that this contagion does not affect other persons, but only lying-in women. Reflecting upon these facts, you will see too much likelihood in the dreadful suspicion, that the hand which is relied upon for succour in the painful and perilous hour of childbirth, and which is invoked to secure the safety of both mother and child, but especially of the mother, may literally become the innocent cause of her destruction; innocent no longer, however, if, after warning and knowledge of the risk, suitable means are not used to avert a catastrophe so shocking.

I need scarcely point to the practical lesson which these facts inculcate. Whenever puerperal fever is rife, or when a practitioner has attended any one instance of it, he should use most diligent ablution; he should even wash his hands with some disinfecting fluid, a weak solution of chlorine for instance: he should avoid going in the same dress to any other of his midwifery patients: in short, he should take all those precautions which, when the danger is understood, common sense will suggest, against his clothes or his body becoming a vehicle of contagion and death between one patient and another. And this is a duty so solemn and binding, that I have thought it right to bring it distinctly before you.

In these days of ready invention, a glove, I think, might be devised, which should be impervious to fluids, and yet so thin and pliant as not to interfere materially with the delicate sense of touch required in these manipulations. One such glove, if such shall ever be fabricated and adopted, might well be sacrificed to the safety of the mother in every labour. Should these precautions all prove insufficient, the practitioner is bound, in honour and conscience, to abandon, for a season, his vocation.

All this I have thus taught in these lectures, from the first. Subsequently to their publication, additional evidence to the same purpose has been promulgated, in the 'Fifth Annual Report of the Registrar-General;' to which I refer, because the collector of that evidence (Mr. Storrs, of Doncaster) points out a wider range of danger than I had indicated, and extends his sound admonitory counsel beyond the cases of puerperal peritonitis, or childbed fever. He shows that the mischief does not always *originate* in the practice of midwifery. The infecting virus is liable to be carried, not only from one parturient woman to another, but from various other sources of animal poison; the circumstances of childbirth rendering the mother peculiarly susceptible of such contagion. More than one series of these fatal maladies have been traced back to the attendance of the accoucheur, at the same period, or just before, upon some case of erysipelas, of sloughing sores, of external gangrene, of typhus fever, and even to his recent presence at the examination of some dead body. In the great general Hospital at Vienna, there are three compartments appropriated to lying-in women. Here upwards of 6,000 births take place annually. One of these compartments is assigned to the instruction of medical men and midwives: another to the instruction of midwives only. In the former of these two, fatal puerperal fever was so very much more prevalent than in the latter, as to attract the notice and the intervention of the Government. It appeared upon inquiry that the male students busied themselves with the investigations of the dead-house and the dissecting-room. A regulation was therefore made and enforced, that every student should wash his hands in a solution of chlorine both before and after every examination of the genital organs in the living subject. From that time the excessive mortality from puerperal fever declined, until it became the same in each of the two compartments. The death rate, which in 1846 had been 13$\frac{2}{3}$ per cent., in 1848 was only 1$\frac{1}{2}$. I give these facts on the authority of Dr. Routh. The hazards which I have been pointing out are hazards which the practitioner in midwifery, especially in the country, cannot always avoid ; but it is most important that he should be aware of them, and should strive to defeat the risk by the most scrupulous observance of every conceivable precaution.

The cadaveric venom here referred to, is well known, and is justly dreaded by us all : and especially by those among us who are oftenest engaged in examining the interior structure, whether healthy or diseased, of the dead human body. Admitted through a cut, a puncture, a mere abrasion of the cuticle, it gives rise to ill-conditioned sores, tenderness of the absorbents, diffused inflammation of the reticular tissue, and great constitutional disturbance. Many promising students, many valuable members of our profession, have perished prematurely of this accidental inoculation. There is reason to think that the virus is produced in the early stages only of the process of decomposition : that when putrefaction has reached a certain point, the point at which sulphuretted hydrogen is evolved, the special poisonous quality ceases. It is during a particular stage of their decay, that German sausages, bacon, and cheese are sometimes found to acquire deleterious and even fatal properties. The claws of carnivorous beasts, and the beaks and talons of carnivorous birds, are apt to be charged with the same kind of poison ; which gives to their bites or scratches a peculiarly dangerous character.

The author of ‘The Bungalow and the Tent,’ speaking of the Adjutant birds, as seen by him in Ceylon, says: ‘They are sometimes very annoying, and even dangerous, and that in a remarkable manner. They frequent, in numbers, the neighbourhood of barracks, or of any other place where they find the means of making themselves comfortable on plenty of garbage ; and being very torpid and lazy, they will not get out of the way of those passing, but prefer pecking at them if they disturb their ease. As they feed entirely on offal or other refuse, their beak is generally tainted and poisoned with decayed animal matter ; and if they manage to break the skin, the wound is often of the most dangerous nature. The same latent poison exists in the claws of tigers, panthers, cats, hawks, and other carnivorous animals. The fact, perhaps not generally known, of the scratch of the old cat being more venomous than that of the kitten, arises from the claws of the former having become hollowed by age, and therefore containing far more poisonous matter.’

That variety of peritonitis which results from perforation of the stomach or intestines, and the effusion of their contents into the cavity of the belly, is full of interest. The inflammation is violent in degree; universal (generally) in extent; and almost always fatal. The attack is characterised by its *suddenness*. All at once intense pain arises in some region of the abdomen, which soon becomes tender in every part. The pain is incapable of removal, and usually even of mitigation, by medicine, and death takes place in a short time. These are the general features of such cases. Occasionally, the symptoms follow some different order. Thus, I have seen a case in which no pain was complained of, and the source of the inflammation was not suspected until the dead body was examined. This was a case of *fever*; and it had been attended with much stupor, which was probably the reason that no indication of suffering was made by the patient. Occasionally, but that is uncommon, the pain intermits. For the most part, however, it resists all treatment, and ceases only with life, or a short time before life is terminated.

Most of the instances of this kind of peritonitis that I have witnessed, have resulted from perforation of the ilium, in the progress of intestinal fever. The glandulæ agminatæ, which are found only in that bowel, and the glandulæ solitariæ, which are scattered over nearly the whole inner surface of the alimentary tube, are very liable, in one species of fever, to inflammation, sloughing, and ulceration: and sometimes the ulcers *go through*: the contents of the gut are poured into the cavity of the serous membrane, and intense inflammation is lighted up. I purposely abstain from going into any particulars respecting these sloughing ulcers. We are no further concerned with them at present than as they furnish the channel by which the cause of the peritoneal disease is introduced. Once, and once only, as I stated before, have I known perforation occur from the extension of scrofulous ulceration of the same glands in *phthisis*. In general, in that disease, the ulcer runs a much slower course. As it approaches the peritoneum, circumscribed chronic inflammation is set up in that membrane; lymph is thrown out; and the bowel becomes adherent to some other portion of the canal, or to some of the other viscera of the abdomen. In this way the perforation is prevented; or, should it take place, the escape of the contents of the bowel into the peritoneal sac is prevented. Occasionally, when two portions of the tube thus adhere together, a communication is formed between *them*, and the contents of the intestine either reach an advanced point of their stated journey by a short cut; or are carried back again perhaps to a spot which they had already passed.

A singular instance of this latter event has been described by Dr. Abercrombie. A man, fifty-six years old, who had shown no signs of serious illness, but had laboured under impaired appetite, languor, and occasional pain in the abdomen, for two or three weeks, was suddenly seized, while taking a walk, with vomiting; and he observed that what he brought up was stercoraceous; and this occurred again and again, at various intervals; the matter vomited being distinctly fæcal, and sometimes so solid that he was obliged to swallow warm water to soften it, that it might be expelled from the stomach more easily. He never vomited his *food*; and no tumour, nor any other sign of organic disease, could be detected by external examination. He lived about three months, and died at last of exhaustion: and then it was discovered that the stomach and the transverse arch of the colon were adherent to each other, and that a ragged aperture of communication between them existed at the place of adhesion.

In the 'Edinburgh Medical Journal' for 1857, Dr. Murchison has given a summary of no less than thirty-three cases, collected from various sources, of this morbid combination, which he names gastro-colic fistula. The primary disease is almost always in the stomach: most frequently a cancerous ulcer; less frequently, yet not seldom, a simple ulcer. Associated with the vomiting of excrement in such cases has often been noticed what may be spoken of as the complement of that symptom, namely, the appearance of undigested aliment in the stools. Though highly illustrative, and naturally to be expected, this discharge of unaltered portions of food from the bowels, is not looked for so often as it should be.

It is a curious fact, that the vermiform appendage of the cæcum is not unfrequently the seat of a penetrating ulcer. I have traced little groups of glands in that slender tube: and I have known perforation to happen from the specific ulceration of typhoid fever; and from the accidental ulceration caused by a cherry-stone lodged there in one instance, and by a pellet of hard fæcal matter in another. The last-mentioned cause is of not unfrequent occurrence.

Sometimes it is the *stomach* that is perforated, either by a common or by a specific ulcer; and the symptoms are exactly the same as when the *bowel* gives way. Sudden, unremitting pain; tenderness, and tympanitic distension of the abdomen; and early death.

Perforating ulcers of the stomach are of various kinds. It is not uncommon to find one small roundish hole, somewhat larger generally on the inner than on the outer surface, the edge of the mucous membrane being as smooth and clean as if a disk had been cut out from it by a punch, and without any surrounding hardness, or other mark of disease. Occasionally the orifice is more irregular, and occupies the centre of a thickened and indurated patch of the mucous membrane. Several instances of this sort of perforation have occurred under my own eye; two within the last fourteen months (1839). Almost all the patients have been young unmarried women, plump, and in good condition; who, up to the moment of the fatal seizure, either seemed to enjoy perfect health, or, at most, had complained of slight and vague feelings of dyspepsia. Ulcers of the stomach are, however, mostly chronic; and the diseased viscus is commonly fenced about and protected by adhesion to the neighbouring parts, before its coats are completely penetrated by the ulcer. You are probably aware that this happened in the body of Napoleon Bonaparte. He died of cancer of the stomach. That organ was strongly adherent to the concave surface of the left lobe of the liver, which formed a part of the wall of the stomach; and this adhesion, no doubt, prolonged his life.

The actual perforation, in cases such as we are now considering, may result merely from the natural progress of the ulcer; but sometimes it would appear that the thin membrane which remains is broken by some accidental force applied to it. Thus the distinctive symptoms occur most frequently after a meal, when the stomach is distended with food, and engaged in the churning movement which attends the process of digestion. They have immediately followed the act of vomiting, brought on by an emetic. Bouillaud relates an instance in which the perforation happened while the patient was straining at stool: and it is conceivable enough, that rough pressure of the abdomen might complete the rupture, when the ulcer had already eaten through all the coats of the bowel except its peritoneal coat. I shall return to this subject in a future lecture.

Occasionally the perforating ulcer has its seat in the *duodenum*. And it is a very curious fact, of which no satisfactory explanation has hitherto been given, that ulcers in the duodenum, at no great distance usually from the pylorus, are of common occurrence in persons who have suffered severe and extensive burns. A number of cases of this kind have been published by Mr. Curling in the 25th volume of the 'Medico-Chirurgical Transactions.' The inflammation and ulceration probably commence in one or more of the glands of Brunner. Frequently the ulcer leads to adhesion between the gut and the pancreas. Sometimes it penetrates into the abdominal cavity, and excites fatal peritonitis.

Less commonly than this, in my experience at least, peritonitis is set up by the escape of urine from the *urinary bladder*, through the extension of an ulcer, or from the forcible rupture of that bag by a blow or a fall, when it was distended with urine. Rupture of the *gall-bladder*, whether by violence or from ulceration, has the same results; so also has, in general, rupture of the *uterus*, which sometimes takes place during the efforts of parturition. *Abscess of the liver*, bursting into the peritoneum, is another occasional source of severe and fatal inflammation of that membrane. Acute and general peritonitis does sometimes arise, also, in consequence of penetration from without; i. e., it succeeds the puncture made by the trocar in the operation of tapping the belly: and these cases, too, are almost all of them mortal: chiefly, I presume, because, in nine instances out of ten, they occur in an unhealthy and debilitated subject.

I formerly offered you some observations respecting a form of hysteria which very closely *mimics* peritonitis, and would most certainly deceive a medical man who was not on his guard against it. We judge by the age and sex of the patient somewhat; by the presence of hysteria in other forms, or of the hysteric diathesis; by the *excessive* tenderness of the abdomen, or rather of its surface; by the coexistence of the same exquisite sensibility in other parts; by the absence of any disease which implies contamination of the blood; and by the incongruity and shifting character of the symptoms. The pulse and the tongue will perhaps be natural, while the abdominal irritation is at its height. Forewarned, you will seldom find much difficulty in establishing the diagnosis. Of the signs by which peritonitis may be distinguished from enteritis, I shall speak when I come to the latter disease.

LECTURE LX.

Treatment of Acute Peritonitis; Bleeding, Opium, Rest. Chronic Peritonitis: Granular Peritoneum. Ascites; Ovarian Dropsy; Diagnosis of these diseases. Other forms of Abdominal Dropsy.

Acute peritonitis, in its simple form, is always a dangerous, yet frequently a manageable disease. When it is complicated with other and earlier organic mischief, and especially when it has been excited by the entrance of foreign irritating matters into the cavity of the belly, it is all but hopeless under any kind of treatment.

The great remedies for peritoneal inflammation are rest, blood-letting, and opium.

Here, again, blood may be withdrawn, directly or by diversion, from the very part inflamed. Cupping is out of the question, because of the tender state of the abdomen. But as early as possible after the inflammation has declared itself, the surface of the belly should be covered with leeches. From twenty to forty may be applied at once. Sometimes no further abstraction of blood will be needed; but in severe cases, a repetition of this local bleeding may be requisite. If the small and feeble pulse, which is characteristic of acute inflammation within the abdomen, should become fuller and softer after the bleeding, you may take that change as one warrant of its propriety.

After the leeches have fallen off, a large warm but light poultice should be laid over the abdomen: or it may be assiduously fomented with flannels wrung out of hot water. These means will encourage the bleeding from the leech-bites; and are generally found to afford great comfort to the feelings of the patient. *Cold* applications have been recommended by some practitioners of high authority. Dr. T. Sutton injected cold enemata, and applied cloths, made wet with cold evaporating lotions, to the abdomen, with good effect; and Dr. Abercrombie has since reported favourably of the same kind of treatment. However, I should think this a more precarious plan than the opposite: and I have always observed so much relief to be given by warm epithems that I have never had the inclination, nor the courage, to employ cold.

Many persons prescribe calomel and opium in this disease, with the view of obtaining the specific action of mercury upon the system. It is doubtful whether the mercury does any good; and if it excite any movement in the bowels, it is calculated to do harm; but the *opium* is essential. It operates not only in soothing pain, but it obtains in its full effect the benefit of the third remedy that I have mentioned—perfect rest to the part inflamed. The vermiform or peristaltic movements of the intestines require to be arrested, as being likely to keep up, or to augment, the existing inflammation. It is superfluous to add that purgatives are not suitable or safe remedies for inflammation of the peritoneum, although they were not long ago recommended and administered. I know of no exception to these rules, except when there is reason to believe that irritation and inflammation have been propagated to the peritoneal covering of the intestines by irritating matters lodged within them. In such cases, and in such cases only, a copious enema of warm water may properly be injected into the large bowel. Certainly, in all cases of well-marked and pure peritonitis, when the inflammation is limited to the serous membrane, it is far better and safer to restrain than to solicit the internal movements of the alimentary tube. In a pamphlet published several years ago by Mr. Bates, of Sudbury, some striking instances are recorded of recovery from severe peritonitis under large and frequent doses of opium, and a rigid adherence to the horizontal posture, until all pain had subsided. The patients were not allowed to raise themselves, on any account, into a sitting position: and the opium was administered sometimes by the mouth, sometimes by the rectum. These cases, related in an unpretending manner by a practical observer, made a strong impression on my mind when I read them. To simple inflammation of the peritoneum, to those perilous forms of peritonitis which occur in women after delivery, and to those still more terrible cases that follow perforation of the serous membrane, this principle of keeping the intestines at rest, is alike applicable. I stated a little while ago, that the last-mentioned cases are all but hopeless. The *all but* I inserted on the strength of some most interesting facts published by Dr. Stokes, in the second number of the 'Dublin Journal of Medical and Chemical Science.' He truly remarks, that in most of these accidents the powers of life sink so rapidly that bleeding, either local or general, cannot be attempted. Neither can we employ mercury internally, for fear of exciting the peristaltic action of the bowels, which action would tend to tear asunder recent adhesions, to keep the communication between the mucous and serous surfaces open, and to cause a fresh ingress of fæcal or other extraneous matter into the sac. Yet in a few instances we find that the patients live for several days, and that a process of organisation commences in the effused lymph. It seems that some years before Dr. Stokes wrote this paper, he had witnessed the admirable effects of opium in low forms of peritonitis, as administered by Dr. Graves; who thus saved, without abstracting a drop of blood, two individuals in whom that disease followed paracentesis. I cannot refrain from quoting to you the particulars of one instance, in which the efficacy of the opiate treatment was conspicuous. The well-known symptoms of perforation of the intestines had existed for two days; the patient was apparently sinking, 'his countenance was collapsed, anxious, and expressive of dreadful suffering; the extremities were cold, and the pulse hardly perceptible. The exhibition of sixty drops in the twenty-four hours, of the preparation called the *black drop*, was followed by the most signal improvement. The pulse regained fulness and softness, the extremities became warm, and the countenance had lost the Hippocratic expression. The patient could bear pressure on the abdomen, which the day before was exquisitely painful. The same treatment was continued for twenty-four hours longer; and by the end of that time every symptom of abdominal inflammation had completely subsided. The belly felt natural, there was no tenderness, the pulse was good, and the patient declared himself well.' At this period of the case, Dr. Stokes omitted the opium, and gave the mildest possible saline laxative, as there had been no stool for forty-eight hours. Four evacuations took place, followed by the immediate return of the symptoms of peritonitis, under which the patient rapidly sank.

'The intestines were everywhere agglutinated together, and adherent to the parietal peritoneum, except in the left iliac fossa, where a quantity of yellow puriform matter was collected. On detaching the caput coli from the peritoneum lining the right iliac fossa, a small perforation of the gut was discovered, by the escape of the contents of the intestines in a jet, &c. &c.'

This example puts in a very strong light the *good* effects of *opium*; the *dangerous* effects of *purgatives*; and the *mode* in which recovery from these frightful accidents *may* sometimes be brought about.

Dr. Stokes gives another instance in which the patient *did* recover; after taking 105 grains of opium, besides what was administered in injections: and he alludes to a third case, in which the employment of opium was successful, when peritonitis had supervened upon the bursting of an hepatic abscess into the cavity of the abdomen.

When the time arrives at which it may seem expedient that the bowels should be emptied, it is better, in the first instance, to employ enemata for that purpose, than to give purgatives by the mouth.

Chronic peritonitis is sometimes merely the sequel of that acute form of inflammation of the peritoneum, which I have just been describing. Plastic lymph is effused, and becomes organised; serous fluid is poured out, and is not absorbed again; the products of the original inflammation remain; a low degree of inflammatory action perhaps remains also, or is re-excited by slight causes; the mischief augments; and the patient is slowly conducted to the grave.

There is, however, another, not at all uncommon, and equally formidable source of chronic peritonitis; the presence, I mean, of a multitude of little granules, lying within or immediately beneath the membrane, or occupying, in countless numbers, those folds of the peritoneum which compose the omentum. These granules occur principally, if not exclusively, in scrofulous persons. Louis, indeed, who considers them to be *tubercles*, affirms that they are never met with in the peritoneum, without being met with also—and usually in a more advanced state and greater abundance—in the lungs; but this rule is not universally true. I have seen more than one instance of well-marked granular disease of the serous membrane of the abdomen, without a single tubercle in the pulmonary tissues. Still the observation of Louis holds good in a vast majority of cases: and when we have symptoms of chronic peritonitis, which were not preceded by those of acute inflammation of the membrane, and when we perceive at the same time indications of phthisis, or of any other unequivocal form of scrofula, we shall seldom be wrong in connecting the chronic peritonitis with the presence of these miliary granulations. Whether they are truly scrofulous tubercles, or whether, as some suppose, they are *su generis*, or again, simply minute spherules of coagulable lymph I do not undertake to determine. I have been in the habit o regarding them as the cause, and not as the consequence, o the inflammation with which they are found associated.

Professor Niemeyer would ascribe such cases to the absorption of cheesy material, itself the residual product of some previous inflammation. You will do well, when you meet with a case of this kind, to search for evidence of such inflammation, and material.

Perhaps we think too despairingly of these cases. When fatal, their true nature becomes manifest: but it cannot be made so when the patient recovers, if he ever do recover. Upon this point an encouraging and very interesting fact is recorded by Mr. Spencer Wells, in his work on the Ovaries. In a case of which the diagnosis was uncertain, an exploring slit was made in the peritoneum, in the presence of Mr. Seymour Haden and others. A large quantity of opalescent fluid was let out. There was no ovarian or other cyst, but 'the whole of the peritoneum was seen to be studded with myriads of tubercles. Some coils of small intestine were floating, but the great mass was bound down with the colon and omentum, all nodulated by tubercle, towards the back and upper part of the abdomen.' This was in December 1862. The patient got quite well, and was remaining so in November 1864.

The *symptoms* of chronic inflammation of the peritoneum are more obscure, in general, than those of the acute disease. And when the disorder is primitive, not the relics I mean of more active inflammation, it often begins, and steals on, in a very insidious manner. The patient complains of abdominal pains: sometimes slight, amounting to scarcely more than uneasiness, but abiding; sometimes occasional only. Usually there is a sensation of fulness and tension of the belly, although its bulk may not be sensibly altered. Sometimes there is a sense of pricking felt. Dr. Pemberton remarks, that you may detect a sort of deep-seated tension; that the skin and muscles lie loosely on the peritoneum, which gives to the hand a sensation as of a tight bandage underneath, over which the integuments appear to slide. The uneasiness, or the pain, is augmented by pressure; or perhaps is felt only when pressure is made. Sometimes the functions of the intestinal canal are disturbed: there are loss of appetite; nausea and vomiting; an irregular state of the bowels, and unnatural evacuations from them. Sometimes, on the contrary, the digestive organs perform their office in a tolerably healthy manner. These differences depend possibly upon the circumstance of the inflammation visiting, or sparing, the peritoneal covering of the stomach and bowels; and of the parts concerned in the secretion of bile. Sooner or later, in most cases, the abdomen enlarges; becomes tight and tympanitic; and fluctuation is felt. All along there is some fever, more or less distinctly marked; with progressive emaciation and debility. The face is pale and sallow, and wears an expression of languor.

Very much the same set of symptoms are apt to result from scrofulous disease and enlargement of the mesenteric glands; and consecutive slow inflammation of the peritoneal membrane.

Accordingly, after death, we often find those glands swelled, and red, and hard; sometimes forming very large tumours: or we discover the whole surface of the membrane to be thickly bestrewed with the small, round, greyish or white granules, of which I have just been speaking; or it is seen to be covered, here and there, or everywhere, with false membranes. The intestines, full of air, are frequently agglutinated into one mass; or they are adherent to each other, or to other parts of the peritoneum, in places only. The omentum is generally thick, red, and fleshy, as if its component parts had been matted together; and there is more or less fluid, commonly turbid and flaky, in so much of the cavity as happens to be left.

These are very unpromising forms of disease, and it is seldom that we can do more than mitigate the most distressing of the symptoms; or retard, perhaps, the march of the disorder. Leeches to the abdomen, in small numbers, and frequently repeated, and followed by soft warm poultices. Blisters, when the pain is not severe, and the tenderness less. Attention to the state of the bowels, which should be regulated by mild laxatives rather than by drastic purges. A nourishing, but unstimulant diet. These are the measures to which we must look for benefit. It has been thought that frictions upon the belly, with ointments containing iodine, have done good: so that it will be well to make trial of such. But do what we may, in nine cases out of ten, our best directed efforts will be disappointed.

When there is much fluid collected in the abdominal cavity in these cases, they take their character from this predominant symptom, and are called cases of *ascites*. But this is only one form of ascites—that form which results from chronic inflammation of the peritoneal membrane. I shall pass, however, by an easy transition, to the other forms of dropsy of the belly.

There is another species of ascites, not very common, which approaches in its character to inflammation, and which is therefore called *active* ascites. I mean that we sometimes see persons, who were previously in good health, become rapidly ascitic, after exposure to cold and wet, and rapidly recover again under the remedies that are used to subdue inflammation. Perhaps it may be said that these *are* cases of inflammation; and it may be so. But they want many of the ordinary symptoms of peritonitis; and if inflammation *be* present, it has no worse effect than the effusion of serum, which, under depletion or diaphoretic remedies, is speedily taken up again. I should rather conceive, however, that these cases are to be included in that category of dropsical effusions which I spoke of formerly, as resulting from the detention in the blood, or from the absorption into the blood, in the first place, of an undue quantity of watery fluid; and its subsequent discharge, by a kind of secretion, either into shut cavities, or through some one of the natural vents of the body. The balance of the circulation between the skin and the internal surfaces appears to be destroyed on these occasions, by the operation of external cold upon the tegumentary membranes.

But by far the greater number of cases of ascites are cases of passive dropsy which arise slowly from a mechanical obstacle to the free return of the venous blood towards the heart.

Ascites occurs, as you know, in general dropsy, with anasarca of the universal areolar tissue; and this general effusion of fluid depends, in almost every case, either upon renal disorder, or upon organic disease of the viscera of the thorax; of the lungs or of the heart, or of both; and, above all, upon such disorder as is attended with dilatation of the right chambers of the heart.

But I exclude this form of ascites, wherein the dropsy of the belly is only a portion of more general disease of the same kind, and limit myself at present to that kind of passive ascites which is unattended with dropsy elsewhere; or which at any rate precedes the occurrence of serous accumulation in other parts.

The symptom which first leads us to suspect ascites, is the progressive enlargement of the abdomen. But the abdomen may grow gradually large and prominent when there is no disease whatever: in pregnancy, for example; or in mere obesity. It is necessary, therefore, to search for more definite signs of peritoneal dropsy.

In order to make an accurate diagnosis of ascites, we must know what are the morbid conditions with which it is most liable to be confounded. Solid tumours and simple corpulency are readily enough distinguished. But there are certain kinds of *encysted* dropsy of the abdomen, of which the recognition is not so easy and obvious. Of these, what is called *ovarian dropsy* is the chief. In some of its symptoms this complaint closely resembles ascites: in some it differs from it widely. So also the treatment of the two disorders is alike in some respects; dissimilar in others. For these reasons, and because I am more solicitous to be practical than to be methodical, I shall consider these two maladies together; turning first to the one, and then to the other, and marking, as I go on, the various points of similitude, and of contrast, which they mutually offer.

Recollect that ascites signifies the accumulation of serous liquid in the bag of the peritoneum; whereas ovarian dropsy consists in the collection of fluid in one or more cells within the ovary; or in a serous cyst connected with the uterine appendages.

One source of distinction between the two is furnished by the condition of the abdomen during their early stages.

In ascites the enlargement is uniform and symmetrical, in respect of the two sides of the body. When the patient lies on her back the flanks bulge outwards, or swag over, from the weight and lateral pressure of the augmenting fluid. This increased *breadth* of the trunk is not observable in the case of an ovarian tumour; nor, I may add, in pregnancy.

When we are able to trace the early history of ovarian dropsy, we find, in most instances, that the abdominal tumour was first perceived on one side; in one or the other of the iliac fossæ, or somewhere between the ribs and the ilium. But when the enlargement of the abdomen is great, the distinction between ascites and encysted dropsy, drawn from the shape of the swelling, fails. The ovarian tumour distends the abdomen, if not uniformly, yet nearly or quite as much on one side as on the other.

The next thing that we do, when the *visible* bulk and shape of the abdomen have suggested a suspicion of ascites, is to employ the sense of *touch*.

Examination by *pressure* will sometimes suffice to assure us that there is fluid in the peritoneum. If you press suddenly with the tips of your fingers, in a direction perpendicular to the surface, you will often become aware of a sensation which it is difficult to describe in words, yet which is quite decisive, and not to be mistaken; a sensation of the displacement of liquid, and of the impinging of your fingers upon some solid substance below. So that, by this manœuvre, you frequently detect, not merely the presence of the liquid, but an enlarged liver, or spleen, or (it may be) an ovarian or other tumour; even when simple palpation, or handling in the ordinary way, would not lead you to the discovery of these enlargements.

Again, *percussion* of the abdomen is fertile of information in these cases. First, by the sense of fluctuation which it causes when liquid is collected within. The left hand being laid flat against one side of the tumid abdomen, if a slight blow be struck with the fingers of the right upon the opposite side, the impulse is conveyed by a wave of the liquid to the open flat hand, which feels a little shock that is perfectly distinctive. The larger the amount of the accumulated liquid, and the thinner and tighter the walls within which it is confined, the more sensible and decided is this fluctuation. Even when the quantity is small, not exceeding a few ounces, a little practice and management will enable you to detect it. Percuss with one finger the most depending part of the cavity, and apply at the same time a finger of the other hand, very near the part struck; and if liquid be there, you will perceive a limited, yet distinct fluctuation. In the same way the presence of liquid in a small cyst may sometimes be ascertained. Much more when the cyst is large. And the cyst, in ovarian dropsy, is often very large: and the liquid it contains is often thin and aqueous; and then the fluctuation may be quite as perfect and perceptible as ever it is in ascites.

Hence mere fluctuation is not a discriminating symptom between ascites and ovarian dropsy.

But, secondly, percussion is full of instruction in the *sounds* it elicits. The sense of *hearing* will generally supply what the sense of touch may leave wanting.

In true ascites the relative place of the liquid and of the intestines is determined by the posture of the patient. The bowels, which always contain some gas, float to the upper part of the liquid, and there give out (when the finger, as a pleximeter, is applied to the corresponding surface, and struck) their peculiar resonance. Mediate percussion will thus follow the gravitating fluid, and discover always a dull sound in the lowermost and a hollow sound in the uppermost part of the abdomen.

But it is not so in ovarian dropsy. The cyst, in a diseased and enlarging ovary, rises in front of the intestines, which, being tied down by the mesentery, cannot embrace the tumour so as to reach its anterior aspect, but are in fact pressed back by it towards the spine. Hence, if there be any resonance produced by percussion, it is in one, or the other, or in both of the flanks; and the umbilical region yields a dull sound whatever the position of the patient may be. The same is true of the enlarging womb in pregnancy.

This simple expedient, then, is quite decisive. In ascites, the patient being supine, the epigastric and umbilical regions are tympanitic on percussion; in ovarian dropsy the latter, at least, is dull. To be quite sure it is well to make the patient assume different postures in succession. If the person affected with ascites turn upon her side, the uppermost flank will become resonant; the umbilical region dull: whereas in ovarian dropsy, the sounds under every change of position remain severally where they were. In ascites, with a little care, you may ascertain the exact level at which the contained liquid stands; and measure its rise or fall from day to day.

Further—as Mr. Spencer Wells has pointed out to me—in all forms of encysted dropsy, the lines and limits of dulness and fluctuation correspond, whereas the fluctuation of fluid free in the peritoneal cavity may be felt far beyond the line of dulness, even around tympanitic intestine.

This mode of diagnosis is scarcely open, under ordinary circumstances, to fallacy, or exception. Yet there are two or three possible conditions in which it may fail; and these it is right that I should briefly mention.

1. The distension, in true ascites, may be *so* great, that the mesentery shall not be broad enough to allow the buoyant intestines to reach the surface, when the patient is supine. This impediment to the efficacy of the proposed test I have met with in practice. A woman came under my charge in the hospital with ascites. Fluctuation of the belly was unequivocal. While she lay on her back, the umbilical and epigastric regions were resonant when percussed; the flanks were dull. When she turned upon either side, the other side, previously dull, gave the hollow sound; the umbilical and epigastric regions, previously resonant, gave the dull flat sound. Under the treatment employed, the accumulated liquid was removed, and she left the hospital.

Some time afterwards, as I was going round the wards, I recognised the same woman among the patients recently admitted by my colleague, Dr. Hawkins. The ascites had returned. The abdomen, enormously distended, projected upwards, as she lay on her back, to an excessive height. I found that fluctuation was very distinct, as before: but every part of the belly yielded a dull sound when struck by the fingers. At length this patient died: and it was seen, after death, that there was nothing to prevent the rising of the intestines. They had floated, at the utmost tether of the mesentery, as high as they could, without reaching the surface of the prominent belly.

2. Another occasional source of fallacy I have just now hinted at. The case which I quoted from Mr. Spencer Wells furnished an example. The intestines may be tied down, and so prevented from ascending, by their specific lightness, to the upper part of the surrounding liquid. And this may happen, either in consequence of the adhesion of the various coils of the intestines to each other, and to the parts behind them; which is not an uncommon occurrence:—or the intestines, though unadherent, may be swathed, as it were, and bandaged down, by a thickened and diseased omentum. This also I have myself seen. A man died in the hospital, who had manifest ascites. Yet his whole abdomen, though not so much distended as to hinder the intestines, had they been free to rise, from reaching its walls, sounded dull on percussion. Inspection of the body explained this circumstance. When the peritoneum was opened by an incision carried through the fore-part of the abdomen, a quantity of serous liquid gushed out. The floor of the cavity which it had occupied was smooth and level; and was found, on further examination, to be formed by a thick cake of omentum, strapped tightly over the subjacent intestines. Of course, the same diseased condition may occur in the female.

3. On the other hand, I have once known an ovarian cyst to exist, when the umbilical region was tympanitic under percussion. The case furnished just that kind of exception which serves to prove a rule. This also was a hospital patient. Her *history* was the history of ovarian dropsy. Some time previously she had discovered a small tumour in one of the iliac regions. It increased without much disturbance of her general health, until it became very inconvenient from its bulk. She was then tapped in one of the Borough hospitals: and she stated distinctly that it was not a clear watery fluid that was let out; but a glutinous, mixed, and grumous matter: such as belongs to ovarian disease. No doubt could be entertained that the enlargement of the abdomen resulted from disease of

that kind. Yet the umbilical region, when percussed, always rendered a hollow sound. Upon the death of the patient the mystery was solved. Air hissed forth from the opening made by the scalpel through the abdominal parietes: and the source of it being traced, an ovarian cyst, of considerable magnitude, was found adhering to the peritoneum in front of the belly, and containing no liquid, but some yellowish shreds only; the remains, apparently, of some smaller included cysts. This ovarian bag had been filled with air, and had given occasion to the equivocal sounds.

Another way in which air may sometimes get into an ovarian cyst, and perplex the diagnosis, is through an opening of communication between the cyst and a portion of adherent bowel.

These sources of possible mistake or obscurity very seldom occur; and the physical diagnosis, as I have now pointed it out, is very sure and valuable. So completely physical, indeed, are these tests, that we recognise ascites by them as readily and certainly in the dead, as in the living body.

Other points of distinction may frequently be derived from the history and progress of the two disorders.

The equable enlargement of the abdomen, on both sides, in ascites, and its unequal prominence on one side in the early stages of ovarian disease, I have already mentioned.

Again, it is observable that in true ascites, there are almost always manifest indications of constitutional suffering and disturbance: a sallow complexion; debility; emaciation. The morbid accumulation results (as we shall see) from disease in some organ, of which the functions cannot be impaired without injury to the whole system.

Ovarian dropsy, on the other hand, may last long, and be extreme in degree, while the general health is scarcely affected. The very bulk and weight of the swelling produce, indeed, much inconvenience and discomfort; but, in other respects, the patient often remains in fair, and even in good health. This appears to be owing to the circumstance that the ovary is not directly necessary to the life or well-being of the individual, but is merely subservient, for a limited time, to the purpose of reproduction.

Among the symptoms that are common to ascites and ovarian dropsy in their advanced stages, are all those which are occasioned by weight and pressure: viz. shortness of breath, from the resistance opposed to the descent of the diaphragm;

anasarca of the legs and thighs, from pressure upon the inferior cava and its branches; and a peculiarity of carriage and gait, like those of a woman big with child, and depending upon the same cause, the necessity of throwing the head and shoulders backwards, to balance the weight of the distended abdomen in front.

It is necessary to caution you against mistaking a distended bladder for dropsy of the abdomen. An old Frenchman was brought into the Middlesex Hospital, afflicted (so his friends said) with dropsy. He had been treated for that complaint. The abdomen was large, and dull under percussion from the pubes to above the umbilicus. In the hypogastric region I could detect an obscure sense of fluctuation. I noticed a strong smell of urine about this patient. Being interrogated, he said that he had formerly had some 'stoppage,' but that he now passed plenty of water; that it even ran from him. It was obvious that his bladder was enormously distended, unable to contract upon its contents, and overflowing. With some difficulty a catheter was introduced, and some quarts, I forget the exact quantity, of turbid and stinking urine were drawn off. The patient sank at length, and the bladder was found to be much diseased. I have known several similar mistakes to occur in private practice. You will not think the caution I am now giving you superfluous, when I tell you, on the authority of Sir Everard Home, that no less a surgeon than John Hunter once actually tapped a distended bladder, in the belief that the disorder was ascites.

Encysted dropsy, in the abdomen, is not always *ovarian* dropsy. *Omental* dropsy is described: the omental cavity alone being unfolded, and full of liquid. This I have never seen. Cysts containing a considerable quantity of a clear thin liquid, and connected with the *liver*, are common. Probably these are in all cases (they certainly are in many) the effects of the growth of hydatids. Dropsy of the *fallopian tubes*: dropsy of the *uterus*: large serous cysts in the *kidney*: constitute other forms of abdominal encysted dropsy. Such states must be discovered by their own particular circumstances. None of them are very common.

LECTURE LXI.

Pathology of Chronic Ascites; of Ovarian Dropsy. Treatment of these two disorders. Internal remedies: Extirpation of the ovarian sac: Paracentesis Abdominis.

In my last lecture I pointed out the means we possess of distinguishing ascites from ovarian dropsy. Continuing the parallel between these two disorders, I have still to consider their pathology; and to prescribe their treatment.

I mentioned that chronic ascites is sometimes the sequel of acute inflammation of the peritoneum. In such cases, the abdomen is usually uneasy, and tender under pressure; or, at any rate, more than commonly sensitive: and I believe more than commonly *hot* also. Whereas when ascites is passive as well as chronic, you may make the requisite examination without causing any distress to your patient. There is no pain produced by palpation, by percussion, or by pressure. Even when the dropsy has resulted from bygone inflammation, it does occasionally happen, though rarely, that no other trace of such inflammation is discoverable in the *living* patient. The absorbing function of the membrane having, however, been spoiled, the collected liquid remains. Such a condition, I believe, I have witnessed. The history of sudden and sharp pain, and tenderness of the abdomen, with fever, immediately before the dropsical swelling took place, made it probable that it was the consequence of inflammatory effusion. But the fever had entirely subsided; no tenderness was left; no large veins were visible on the surface of the belly, denoting internal obstruction; and the general health was good. The patient had no other dropsy.

The main exciting cause, however, of true and uncombined ascites, when no inflammation is, or has been, at work, is some impediment to the venous circulation in the abdomen. Whereabouts, and of what kind, is this impediment? That is the question, which, in each particular instance, we ask ourselves.

The old doctrine respecting the causes of ascites, vaguely referred the collection of liquid to *obstruction*; and to *organic diseases* of the abdominal viscera; and, above all, to *hepatic* disease. But as we are now better instructed, and know that organic diseases produce the dropsy, ultimately, by retarding the flow of blood through the system of the vena portæ, we see that the truth was only half perceived by the ancient pathologists. We can now understand why some organic diseases of the abdomen lead to dropsy of the peritoneum, and others (even of the same viscus) do not. And we have no difficulty in comprehending why, of all the abdominal viscera, the *liver* is the one of which the diseases are the most frequently connected with ascites; that gland being traversed by the converging branches of the venous trunk, through which passes by far the greatest part of the serosity absorbed from the surface of the vast membrane that inwraps most of the abdominal organs, and lines the cavity containing them. It is plain that an accumulation of serum in the peritoneal sac may arise from a mechanical obstruction in the trunk of the vena portæ, or in some of the principal branches that unite to form that vein; or from certain diseases of the liver itself. But we know that disease of the liver is of very common occurrence, and oftentimes very obvious, while there is no ascites. And a further question arises—With what kinds of disease of the liver is hepatic ascites most apt to be associated?

In truth, there is one special form of liver-disease which, though not the sole, is the grand cause, of passive and simple ascites. It has long been noticed that mere enlargement is not the most common condition of the liver met with in hepatic dropsy; but rather the small, hard, contracted viscus. Mere increase in the size of the organ may interfere but little with the portal circulation; whereas a shrinking and diminution of its bulk must needs do so. In point of fact, that particular state of the liver which the French have termed *cirrhose*, and which is familiar to morbid anatomists in this country as the *hobnail* liver, is the great source of passive ascites.

The true character of this remarkable condition of the liver has been much misunderstood. The change from the healthy state is commonly attributed to chronic inflammation, and thickening, and subsequent shrinking, of the connective tissue which accompanies and involves the portal vein, the hepatic artery, and the biliary ducts, in their course throughout the liver, and which is called the capsule of Glisson; the hepatic vein and its branches being lodged in the substance of the gland without any such involving tissue.

Granting the inflammation of Glisson's capsule, the following explanation, which I have been in the habit of giving in former courses of these lectures, seems simple and plausible enough.

A general thickening of this tissue must produce a general pressure upon the portal veins, large and small, and hinder the return of the venous blood from the intestines. Hence, as in analogous cases, congestion of the capillaries, arrested absorption, mechanical transudation of serous liquid. The pressure affects also the nutrient vessel, the artery of the liver; so that, in most instances, there is atrophy and shrinking of the organ; and occasionally, though rarely, jaundice also ensues, from pressure upon the biliary vessels. By degrees, the areolar tissue itself begins to shrink; and the spaces in which it ramifies on the surface of the liver are pulled inwards; the lobules appear to be prominent; and the surface becomes irregular and knobby, and studded with little roundish eminences like the heads of nails. The constricted lobules are likewise very conspicuous in the cut surface of the liver: appearing like a congeries of peas, of a pale yellowish colour, like that of the surface.

But Dr. Beale has shown reason for believing that this explanation—which is still, I fancy, very generally current—is erroneous. There is no evidence of any inflammatory condition of Glisson's capsule. Careful examination of the cirrhose liver under the microscope demonstrates that the change commences in the hepatic cells, which, from some vice in the blood, undergo degeneration, shrink and shrivel up, and are gradually destroyed from the circumference of the lobules towards their centres. The lobules themselves are seen to be wasted, and the spaces between them occupied by a firm white material, looking like fibrous tissue, but being granular rather than fibrous. Here and there contiguous lobules appear as if they had been fused together, all the vessels that lay between them gone. The portal vein having been injected, Dr. Beale found that the white material which separated many of the wasted lobules was penetrated and traversed in every part and in all directions by pervious vessels of considerable size; so that the passage of the blood could not have been mechanically precluded *in the way supposed by the prevalent hypothesis.* Many blood-vessels, and ducts, must indeed have been wasted and abolished; and to the remains of these, rather than to the presence of any adventitious tissue, Dr. Beale imputes the fibrous appearance. The capillaries of the lobules in these cases have almost entirely disappeared. The whole of the liver, composed mainly of shrunken lobules, is proportionally diminished in bulk; and the portal circulation, through its damaged texture, must accordingly be impeded, scanty, partial, or altogether prevented.

Hereafter I shall have to show you a similar morbid condition of the kidney, constituting one variety of Bright's disease—the small, red, *gouty* kidney. It would seem that the least diluted forms of alcohol, what are called ardent spirits, especially when swallowed 'neat,' lead to cirrhosis of the liver; while wine and beer foster gout, and the analogous condition of the kidneys. Certainly, among the causes of cirrhosis of the liver, the habitual intemperate use of spirits holds the chief place. The diagnosis will be assisted therefore by our knowing that the patient has been a spirit-drinker. The liver in question is the true gin-drinker's liver. The same disease must arise from other causes also, for we sometimes meet with it in the bodies of children, and of adults who had always lived temperately. Nay, Sir Robert Carswell (whose delineations of the appearances presented by cirrhosis are before you) found precisely the same state of the liver in a cow dead with ascites; and Dr. Budd has seen it in a pig: but in ninety-nine cases out of a hundred it is traceable to the repeated operation of the poison of alcohol. In dogs that had been destroyed by that poison, Dr. Percy recovered alcohol from the blood, the brain, and other parts, but most of all from the liver; and we may conclude with Dr. Budd, that the strong alcohol, rapidly absorbed from the stomach into the portal vein, is carried at once to the liver, and exercises a directly mischievous influence upon its tissues.

In the living body the presence of this hepatic disease is, for the most part, a matter of inference only. It is rendered probable by its ascertained frequency in connection with ascites, and by the absence of any other obvious cause for the dropsy. But sometimes the irregular surface can be felt through the walls of the abdomen.

The nature of this morbid change affords a reason for the intractable and unpromising character of ascites in general. The obstructed blood seeks indeed new channels; but the compensation they furnish is rarely sufficient. The portal blood is diverted towards the vena cava and its tributaries. The superficial veins become obvious, numerous, large; and wander with many inosculations over the surface of the belly. Large veins, significant of the same compensating effort, have been met with also in the adhesions which previous inflammation had left between the liver and the diaphragm.

But cirrhosis of the liver, though it is the principal, and by far the most frequent, is not the only cause of obstruction to the current of the blood in the portal vessels, and of consequent ascites. In those specific forms of liver disease in which separate tumours are scattered through its substance, one of these tumours may be so placed as to press upon the trunk of the vein. So, obviously, may abdominal tumours of any kind; enlarged mesenteric glands; cancer of the pylorus; cancer of the head of the pancreas; and the like.

Ascites is found to be not unfrequently associated with disease and enlargement of the *spleen* also; but in most instances of this kind, the enlargement of the spleen and the peritoneal dropsy are not connected as cause and effect; but are both consequences of portal obstruction.

When, after death preceded by ascites, the cavity of the abdomen is laid open, its contents present a bleached and sodden appearance. It has been made a question whether this be the result of the long-continued immersion of the living tissues in the accumulated water; or of their short maceration after death. The question has no practical importance.

Dropsy of the ovary in its ordinary form consists (I believe) in disease and enlargement of one, or more, of the Graafian vesicles; or of the ova which they inclose.

The actual condition of the dropsical ovary is subject to much variety. Sometimes there is but one cyst; and this may be no bigger than a pea; or it may be large enough to contain many gallons. Its walls may be as thin and flexible as those of the healthy urinary bladder; or they may be firm, and half an inch or more in thickness. It may spring from a small pedicle, and lie free and otherwise unattached in the cavity of the peritoneum; or it may adhere, partially or at all points, to the contiguous surfaces; or it may be tied and tethered by bands of coagulable lymph. Its inner surface may be smooth and even, or knobby and irregular. Lastly, the fluid contained in the cyst may be thin, or consistent; limpid, or glutinous; opaque, or transparent; and of various tints; so that, in different cases, it may be colourless, green, purple, red; and more or less resemble in appearance pure water, white of egg, jelly, glue, birdlime, or treacle. Most commonly, however, when the cyst is single, its contents are thin and aqueous.

Again, the dropsical ovary may be multilocular, composed of many cysts, distinct, and separate from each other; or sometimes, through mutual pressure, communicating one with another: and these cysts, in the same ovary, while they vary much in size, may differ also from each other in any or in all the particulars just enumerated as being incidental to ovarian cysts. In this variety we speak of *multiple* cysts.

But there is another variety of multilocular ovarian dropsy, and much the commoner of the two, in which one or more large cysts have their walls embossed, as it were, by the projecting outlines of groups of small nodules that lie within their parietes, and may be strictly spoken of as their offspring; as yet of stunted growth, but capable, like the parent cysts, of indefinite enlargement, and of giving birth to cysts of a still later generation. These are distinguished by the name of *compound* or *proliferous* cysts.

The external surface also of the multilocular ovary is generally lobulated; and its inequalities may often be discovered by a careful examination of the abdomen in the living subject.

Sometimes the tumour is solid throughout; in which case the term dropsy is altogether misapplied.

These differences are not without importance, in reference to some points in the treatment of the disease.

The *progress* of ovarian dropsy is no less wanting in uniformity. Sometimes it is very rapid; occasionally it is very slow. It may destroy life in a few months; more rarely it may continue, a mere burden, with scarcely any fatal tendency, for many years. Not unfrequently, after a period of active increase in the tumour, the morbid process, without any obvious cause, suddenly stops: and the pause may be final; or, after an uncertain interval, the disease may resume its former activity.

Under all circumstances the malady is a serious one; for its possible grievances are many; and its issue is precarious and unpromising. Although, in some cases, the general health for some time is but slightly or not at all impaired, in others the disease runs a short course; the tumour, if not meddled with, increasing rapidly, and proving ultimately fatal by its bulk and pressure; or embittering and abridging the unhappy patient's existence by some accident of growth or of position. Even when of no vast magnitude, it may be so situated as to impede or prevent the expulsion of the fæces from the bowel, of the urine from the bladder, or of the fœtus from the gravid uterus.

The single cysts, having thin parietes, and containing a serous liquid, are not always produced by disease and distension of a Graafian vesicle; for they sometimes have no connection with the ovary, but spring from some other part of the uterine appendages.

Neither, perhaps, can it be *demonstrated* that the complaint originated within the Graafian vesicles, when it exists in its more complicated form; when the cysts are many, and their contents various. But the shape of the cysts, which is more or less spherical, their number, their isolation in many cases, and the diversity in the matters by which they are filled, render this view of their origin at least a probable one.

This form of the disorder has been considered as belonging to the category of *malignant* diseases; but, in my judgment, without sufficient reason. It is true that the tumour does sometimes involve one or more of those morbid conditions, which have been denominated scirrhus, fungus hæmatodes, cerebriform disease, colloid cancer, or melanosis, and which all, or nearly all, appear to be varying results of the same morbid process, and to be referable to the genus *carcinoma*; but whenever this is observed to be the case, other structures also are found to be infested with analogous changes. The so-called malignant disease occupies the ovary in common with other parts; and this is one of its most constant characters, namely, that proceeding from some vice in the constitution, or disseminated from some local germ, it pervades different organs of the body at the same time, or in succession; whereas in by far the majority of instances of ovarian dropsy, these peculiar products are met with neither in the diseased gland, nor in any other place. It has already been remarked that some women, labouring under ovarian dropsy, enjoy nevertheless in all other respects very good health, even for many years. The victims of malignant disease are not so fortunate. *They* either are soon cut off, or, if they linger, they seldom fail to exhibit, in their complexion and general condition, notable indications of the mischief which is in progress, and gradually undermining the powers of life.

If it be admitted, as a reasonable conjecture, that the Graafian vesicles, or the ova they contain, are the seat of the primary changes, we may push our speculations a little further. These ova are destined, under the peculiar stimulus of impregnation, to build up the fabric of the body in all its parts and qualities. And we may suppose that, in consequence of some unnatural and morbid stimulus, perverted and erring action may be set up, and strange products result. It is not uncommon to find fat, hair, cholesterine, teeth, in the diseased ovaria, even of virgins. These substances being all appendages of the skin, the cysts in which they are met with have been called *dermoid* cysts.

This view of the matter is strengthened by the fact, that dropsy of the *ovary*, of the ordinary kind, seldom *commences* before the age of puberty; nor often after the capability of child-bearing has ceased; but chiefly during that period in which the organ, if healthy, is susceptible of its proper and temporary function. Virgins, and barren and fruitful wives, are alike subject to the disease; but in what relative proportions, statistical enquiry has not yet (so far as I know) determined. Where it accompanies, it may also account for, sterility.

The catamenia during the progress of the malady sometimes appear with more or less of punctuality and quantity; sometimes are entirely suspended. This function is so often interrupted under other circumstances, that its derangements shed but little light upon cases that are otherwise obscure. When the discharge continues to recur, we may presume that one, at least, of the ovaries is in a tolerably healthy state: when both are sensibly diseased, the catamenia may be expected to be wanting.

The *treatment* of these two forms of abdominal dropsy must, up to a certain point, at which the intervention of surgery becomes expedient, be considered separately. Of both it may be said that their cure by medicine is seldom accomplished. Of both, the natural progress tends steadily towards the destruction of life: yet one of them is more within the reach and rescue of art.

In passive ascites, when the distension of the peritoneum has crept on without pain, fever, or other marks of inflammatory action, our first and best hope of removing the collected fluid will rest upon diuretics. Hepatic ascites and renal disease may be sometimes found in conjunction, but according to my experience, they seldom are so: and except that both may probably owe their occasional origin to habits of intemperance, there appears no reason why they should be. Diuretics may be prescribed, therefore, without scruple. The hydragogue purgatives are to be employed, also, when diuretics fail to act, or to reduce the swelling; and when the disease is not already complicated with diarrhœa. In cirrhosis of the liver no conceivable benefit can be expected from mercury except in so far as it may promote the action of diuretics. In Germany, the muriate of ammonia is in much repute as a therapeutic agent. This diuretic salt, though seldom administered internally in this country, is believed by some practical men who have employed it, to exercise the same beneficial influence upon the functions of the liver, as is commonly attributed to preparations of mercury; while it is less productive of distress or inconvenience. My own experience upon this point is too limited to justify me in expressing any confident opinion about it; but in some recent instances I certainly have noticed a remarkable improvement in the character of the biliary excretion, after the daily exhibition of sal-ammoniac combined with the extract of taraxacum.

You will generally be obliged to try, in their turn, all the diuretics within your reach, and frequently to no purpose. Our efforts to remove by medicine the accumulated liquid, or to cure the morbid condition on which the accumulation depends, are too often made in vain. The distension of the peritoneum continues to augment; the distress arising therefrom becomes urgent and extreme; and at length, to afford temporary ease to the patient, and in the faint hope also of giving him permanent relief, we resort to the mechanical expedient of *paracentesis*.

When we have the opportunity of treating *ovarian dropsy* from its commencement, we sometimes find that the enlarging ovary is painful, or tender. This is an indication for *antiphlogistic* measures. But from such remedies, or from any remedies, little more than temporary relief is to be expected. My position as physician to a hospital, has brought under my notice many cases of ovarian swelling, at a very early period of its development; when all that could be detected by careful examination of the abdomen was a small tumour, not larger, perhaps, than an egg, and occupying the situation of the ovary; to which tumour the attention of the patient had been drawn by some pain or uneasy feeling in that part. I have treated such cases assiduously, with the remedies of chronic inflammation, frequent topical bleedings, and the use of mercury till the gums were affected: with the remedies of ordinary dropsy, diuretics and drastic purgatives: and with remedies accounted specific; the liquor potassæ, the various preparations of iodine: and I must honestly confess to you that I am unable to reckon one single instance of success. Yet these are the measures, if any, that we are bound to try: never further than they can be carried without serious or permanent damage to the general health of the patient. They have succeeded—as we are assured by competent and credible witnesses: they may therefore succeed again. The amount of my own experience, however, has led me to the belief that medicine has, in general, very small influence over the progress of this disorder. The cases that do well, do well we scarcely know how or why; the cases that prove fatal run their course in spite of us.

Sometimes, as has been stated, these ovarian tumours reach a certain magnitude, and then (wherefore we cannot tell,) enlarge no more; but remain, a mere inconvenience and deformity, for many years. Occasionally, either spontaneously, or in consequence of some accidental violence, they burst into the cavity of the peritoneum, whence the effused fluid may be absorbed; but more commonly it causes fatal inflammation. Or the bursting tumour may empty itself more harmlessly (adhesion having previously taken place) through some channel of communication with the bowels, with the vagina, or with the bladder; or externally through the parietes of the abdomen.

Tumours, supposed to be ovarian, do sometimes disappear entirely. It may, however, be doubted whether all, or even many, of the enlargements which have had this fortunate issue, were really connected with the ovary. One source of mistake I have myself more than once encountered, and I believe it to be not uncommon. A brief statement of the circumstances under which I first observed the fallacious symptom, will show you at once what I mean. Some years ago I was sent for by a lady, who for many days had been labouring under an ordinary attack of continued fever. While examining the abdomen by pressure, I discovered, on the right side, between the ilium and the umbilicus, a round, hard, painless tumour, as big as a swan's egg. The patient was aware of it; and thought it had existed for some time. At my next visit it was gone. In the interim, very abundant discharges from the bowels had followed the administration of purgative medicine. The tumour had obviously been formed by the accumulation of fæcal matters in the cæcum.

Similar collections take place, less frequently, on the left side, just above the sigmoid flexure.

The parts concerned in this disorder are not essential to life, or to the enjoyment of health. On some of the lower animals (pigs, for example), the operation of spaying is as customary in the one sex, and is performed with as little risk, as that of gelding (of horses, for instance) in the other. Healthy ovaries have in several instances been extracted from the living human body without any ill consequences. These facts, and the intractable character of the disease, naturally suggested the thought of extirpating the tumour in cases of ovarian dropsy.

But although the ovary, when healthy, or when not greatly enlarged, might be removed without much difficulty or hazard, the operation becomes always perilous, and sometimes impracticable, when the altered gland has attained any considerable magnitude. Yet these are the very cases for which the remedy is needed. A large ovarian tumour is usually multilocular, with firm parietes, and thick internal septa; and is therefore incapable of collapsing much when punctured. In order to extirpate such a tumour, it was thought that the abdomen must necessarily be (and it often has been) laid open from the sternum to the pubes. Very commonly, also, a large ovarian swelling is adherent to the contiguous parts; a circumstance which might either make the proposed removal of the tumour impossible, or, if the adhesions admitted of being broken down, would augment in a fearful manner, it was thought, the jeopardy of the patient. At the time to which I am referring, the diagnosis of these tumours was far less secure than it now is. Three times at least (one of the cases is recorded by Mr. Lizars, another by Mr. King, the third fell under the cognizance of Dr. Richard Bright) the abdominal muscles and the peritoneum had been slit open for the purpose of extracting a diseased ovary, when no disease existed. I may mention another case in which a similar mistake was very near being made. My colleague, Mr. Arnott, had a patient brought to him from the north of England, with a view to the removal of an enlarged ovary. Tapping had been meditated, and the trocar was even in hand, and pointed at the abdomen, when the house-surgeon of the charity where she then was dissuaded this step. A surgeon accompanied the patient to London, and dwelt upon the disappointment she would experience if she could not get the operation performed by a first-rate London surgeon. Mr. Arnott examined the patient; there was no enlarged ovary, no abdominal tumour, to be discovered. The girl was a wretched hysterical sufferer, perhaps a malingerer; the enlargement had resulted from flatulence; it was what has been well called a *phantom* tumour.

It is not surprising, therefore, that the operation at its first proposal was received with much discouragement. I remember attending a discussion on this subject at a meeting of the Medico-Chirurgical Society in 1850, when no less eminent a surgeon than the late Sir William Lawrence denounced the operation, as perilous both to the patient and to the character of the profession. You may read in the 46th volume of the same society a succinct but interesting history of Ovariotomy, by Mr. Spencer Wells. The first case of it on record, a perfectly successful one, was performed in America, in 1809, by Dr. M'Dowell, a Kentuckian surgeon, who had been a student in Edinburgh under John Bell in 1794. Various attempts were subsequently made in this country to remove diseased ovaries by means of a long incision in the abdominal walls. But for the most part they were disastrous attempts. Then, in 1836, a modification of the process of excision was adopted, apparently in pursuance of an old suggestion of Dr. William Hunter's, whose words sufficiently explain its nature. 'If it be proposed indeed (he writes) to make such a wound in the belly as will admit only two fingers or so, and then to tap the bag and draw it out, so as to bring its root or peduncle close to the wound of the belly, that the surgeon may cut it out without introducing his hand, surely in a case otherwise so desperate it might be advisable to do it, could we beforehand know that the circumstances would admit of that treatment.'

Mr. Jeaffreson, a surgeon of Framlingham in Suffolk, was the first person in Great Britain who performed ovariotomy by the small incision. He removed a bilocular cyst through an opening only one inch and a half in length. The patient recovered, and bore several children afterwards.

This operation has since been done, with most encouraging success, by various persons in this country; by Dr. Clay, of Manchester, by Dr. Thomas Keith, of Edinburgh, by accoucheur physicians and surgeons in London, and most especially by Mr. Spencer Wells, who informs me that he has now (February 1871) performed the operation 408 times. In his first hundred cases there were 66 recoveries, in the second 72, in the third 77, in the fourth 78; and eight patients who have since undergone the operation are all doing well. Dr. Keith has been, on a smaller scale, even more fortunate than this, counting 19 deaths only in 100 cases.

Surely this measure of success—vying with that which may be claimed for amputations of the thigh—sufficiently vindicates ovariotomy as a legitimate operation of surgery; indeed, it seems to me one of its greatest triumphs. You may estimate its value by contrasting the good prospect it holds out of perfect and permanent recovery on the one hand, and the almost certain destruction of life within a very few years at the longest, with miserable suffering towards the end, on the other.

In the progressive decrease of mortality in Mr. Spencer Wells' four centuries of cases, we see the result and the reward of well-used experience directed upon minute yet important niceties of detail--improvements in the method of operating; safe-guards against ascertained perils; cautions as to the preparation and after-care of the patient. It would be absurd for me to affect to give you instruction in these matters; but there are some other points which require a short comment.

On former occasions I have explained to you a plan of treatment, the main part of which consisted in steady and continued pressure made upon the tumour by a tight flannel bandage; at the same time I expressed very serious doubts whether this plan, which was said to have had some prosperous results, would, if tried on a more extensive scale, be found generally successful, or expedient, or even endurable. I believe I may now say that the practice has been entirely abandoned.

Neither has lapse of time and larger experience confirmed the merit of another method, proposed by Mr. Bambrigge, which also I have been in the habit of mentioning—that of keeping open the aperture in the cyst—whether made by the trocar, or the result of accident, or the spontaneous work of nature—without further meddling by instruments or injections. The hope was—a hope in some few cases realised—that the discharge might gradually become puriform, diminish in amount, and finally cease; the orifice closing from the contraction and ultimate obliteration of the cyst, or a simple fistulous passage alone remaining. The drain, however, of a long-continued discharge must in itself be a great evil; and there is always the risk that destructive inflammation may be set up within the cyst, and compromise the immediate safety of the patient. It is interesting to know that even under that extreme danger ovariotomy may sometimes come to the rescue. Take one of several examples. From a woman whose life was despaired of, who was suffering under pyæmic fever, with a pulse of 120 and a temperature of 103, Mr. Spencer Wells removed a suppurating cyst full of putrid and most offensive matter. The next day the pulse had fallen to 100, the heat to 99, and all went well thereafter.

Ovariotomy may also be, and has been, performed during pregnancy, without leading to premature delivery.

I told you that the presence of adhesions had been thought likely to render the operation both difficult and dangerous. Sometimes, no doubt, they may make it impossible; but the verdict of Mr. Spencer Wells' experience is that adhesions may, in most instances, be carefully broken down without any ill consequence.

Anxious questions have arisen respecting the minor operation of *tapping*. Should that be done once, or oftener, before ovariotomy is practised? Mr. Spencer Wells has brought forward a sufficient number of facts to warrant the following conclusions:—'1. That one or many tappings do not considerably increase the mortality of ovariotomy. 2. That tapping may often be a useful prelude to ovariotomy, either by giving time for the general health to improve, or by lessening shock when the fluid is removed a few days before removing the more solid part of an ovarian tumour. And 3. That when a syphon-trocar is used in such a manner as to prevent the escape of ovarian fluid into the peritoneal cavity, and the entrance of air into the cyst, the danger of tapping is very small.'

Once more : 'In seven persons he has tried injection of iodine into the emptied cyst, with the hope of obtaining a radical cure. Of these only one is alive who has not since undergone ovariotomy. The injection of iodine should be restricted to cases where, for some reason, ovariotomy cannot be performed, but where a cure may be hoped for after suppuration and drainage.'

The danger of paracentesis, whether in ascites or in ovarian dropsy, is, as has already been hinted, the danger of exciting fatal peritonitis.

Formerly, the rapid evacuation of a large quantity of liquid from the belly was often attended by terrifying effects; fainting, convulsions, almost instant death. This made the ancient physicians afraid of the operation: and when they could no longer avoid it, they let the accumulated fluid out by little and little, and at short intervals.

The cause of these alarming symptoms is now well understood, and easily averted. They were owing, doubtless, to the sudden removal of the pressure to which the viscera and large blood-vessels had been for some time submitted and accustomed. For this explanation of the fact we are indebted to the sagacity of our celebrated countryman, Dr. Mead, who was the first to suggest that external compression should be substituted, in lieu of the tension taken off by the operation. The complete success of that expedient fully justified his ingenious opinion. We now drain the cavity of its liquid contents without scruple or delay. Round the body of the patient, who sits on the edge of a chair or of the bed, a sheet, or broad roller is thrown, and tightened as the fluid escapes, so as to maintain an equable pressure, which is continued for a while, and at length gradually withdrawn. The risk, however, of exhaustion or syncope, and therefore the necessity for this artificial compression, may in most cases be avoided or diminished by keeping the patient, during the performance of the operation, in the horizontal posture, upon his or her side. We owe this practical improvement, I believe, to Sir James Simpson.

Other casualties occasionally happen; the trocar has sometimes pierced the intestine. In one instance which I myself witnessed, clear serum issued for some time through the canula, but at length pure blood; not less than a pint. The patient sank; and no opportunity was given of investigating the cause of the bleeding. In another strange but well-authenticated case, the almost incredible quantity, twenty-six pints, of blood flowed out at the orifice made by the trocar, and afterwards separated into clot and serum. To the wonder of those who saw the incident, this patient recovered from the tapping; and the source of the hæmorrhage is still a matter of conjecture.

And apart from these mischances—which, after all, are not of frequent occurrence—you must bear in mind that paracentesis can seldom be contemplated as a mode of cure, but simply of temporary relief from distress. A few instances have happened where the liquid has been drawn off, and has not again collected: but such cases are very rare. Sometimes the kidneys resume their activity upon the removal of the dropsical fluid. This results, I believe, from the liberation of the large veins within the abdomen from the pressure to which they had been subjected. Ordinarily, the liquid re-accumulates, often with more rapidity than before; and again, and again, the pain and the hazard of the operation must be repeated: wherefore, in my judgment, paracentesis in abdominal dropsy ought seldom to be performed, unless the quantity of liquid is so great as to occasion painful distension; or causes great distress of breathing by its upward pressure against the diaphragm; or gives rise to some positive suffering or urgent inconvenience, which the removal of the water may be expected to remedy.

Acupuncture of the dropsical belly has of late been recommended; the passage of a grooved *needle*, instead of a *trocar*, through the abdominal parietes. I believe this to be sometimes an eligible and a useful piece of practice. By ascertaining the character of the inclosed liquid, it may settle the diagnosis of a case otherwise ambiguous; but it may do much more. Dr. Robert Lee informs me that he has done this minor operation many times; never with any bad result, generally with relief and benefit to the patient. In one case, ten gallons of liquid escaped from the little puncture. In another, where ordinary tapping was thought unsafe, acupuncture was performed, and fluid oozed freely away for two days and two nights. Great comfort was obtained from this process, and the woman's life was probably prolonged for two years by several repetitions of it. In a third instance, four ounces only of liquid followed the puncture, but a larger portion, which was left behind, gradually disappeared.

It is seldom that tapping is many times performed upon the same person, when the complaint is mere passive ascites. The dropsy returns indeed, and again the operation is required; meanwhile, in most cases, the health and strength rapidly deteriorate, and the patient sinks.

The same speedy declension and early death too frequently occur in ovarian dropsy also, when ovariotomy is deemed impossible; yet simple tapping commonly bears to be repeated more often than in ascites, without serious detriment to the general health. Sometimes the liquid re-accumulates in the cyst very quickly; sometimes slowly; in a very few instances not at all. I have had under my own care a patient who had been tapped for this disease thirty-eight or thirty-nine times. Extraordinary examples of a similar kind are on record; one or two I may mention as specimens.

Dr. Mead narrates the case of a lady, who, 'for the information of posterity, ordered by her will that the following English inscription should be engraved on her monument in Bunhill Fields:—

> "Here lies Dame Mary Page,
> Relict of Sir Gregory Page, Bart.
> She departed this life, March 4, 1728,
> In the 56th year of her age.
> In 67 months she was tapped 66 times:
> Had taken away 240 gallons of water,
> without ever repining at her case,
> or ever fearing the operation." '

Among authenticated instances, the most remarkable that I have met with is detailed in the 'Philosophical Transactions' for 1784, by Mr. Martineau, who was at that time surgeon to the Norfolk and Norwich Hospital. An abstract of the case is given in the printed catalogue of the Hunterian Museum, where the cyst is preserved: it belonged to the left ovary of Sarah Kippus, a widow, fifty-five years old. 'The complaint began after a miscarriage, at the age of twenty-seven. From the year 1757, to August, 1783, when she died, she had been tapped eighty times, and had, in all, had taken from her 6631 pints of fluid, or upwards of thirteen hogsheads. 108 pints was the largest quantity ever taken away at any one time. But after death, Mr. Martineau could not make the sac contain more than fifty pints.' Mr. Spencer Wells has removed 115 pints of fluid at one tapping.

Upon the whole, it may be stated of this operation, as applied to ovarian dropsy, the extraction of the ovary being impracticable—

1. That when it is essential to the comfort and continued existence of the patient, it brings sensible relief to her distress, and often materially prolongs her life:

But, 2. That when it is performed under less pressing circumstances, it tends to shorten the patient's days. Dr. Bright is of opinion that the number is small of those who survive the first tapping more than four years. I question whether even that brief limit might not justly be abridged by one half. A respectable woman having very large ovarian dropsy, entered the Middlesex Hospital, under my care, for the express purpose of being tapped. The tumour incommoded her by its bulk and weight, but in no other way; and she had carried it for thirteen years. I felt that I should not be justified in sanctioning the operation in such a case. The patient was made to understand that the performance of it would not be altogether free from immediate danger; and that if she went through it safely, the swelling would return, and the same kind of remedy again become equally necessary. She was instructed how to suspend the heavy overhanging abdomen by a sling passing over her shoulders. There appeared no reason why she should not continue in good health for another period of thirteen years.

I am aware of another instance, in which a woman, similarly burdened, but otherwise in comfortable health, has lived, not without enjoying life, between twenty and thirty years. Had she been tapped when the mere enlargement might have seemed to warrant the operation, she would probably have been for twenty years in her grave.

Of the cases of hysteria in which ovarian tumours are mimicked, we have an easy diagnostic test in chloroform. Put your patient under the subduing influence of that vapour, and the 'phantom' vanishes.

LECTURE LXII.

Acute Gastritis: symptoms; anatomical characters; treatment. Chronic Inflammation of the Stomach; thickening of the Mucous Membrane; Ulceration; symptoms and treatment of the disorder; Softening and perforation by the Gastric Juice. Cancer of the Stomach.

Acute inflammation, when it affects the peritoneum, usually spreads with rapidity over the whole surface of the membrane. This is characteristic of inflammation of the serous membranes generally. But it is not so with the other tissues that compose the alimentary canal. Inflammation of the mucous membrane may be, and often is, very limited in extent: and the different portions of the intestinal tube, as they differ in function, so also they differ somewhat in their diseases, and still more in the symptoms by which those diseases are revealed. Not being fettered by any artificial system of arrangement, I shall take the course which promises to be practically most useful, and consider separately the maladies of the several parts of the alimentary canal in the abdomen, extending my remarks occasionally to the whole of the tube, when speaking of disorders that are common to all portions of it.

Let me, then, in the first place, draw your attention to the organic diseases, and the morbid conditions, of the *stomach*.

It is remarkable, all things considered, how seldom the stomach is affected with *acute inflammation*. Scarcely ever do we find either the organ as a whole, or any one of its tissues separately, the subject of *spontaneous* acute inflammation. Indeed, the mucous coat of the stomach is even more patient of rude and rough treatment than the healthy peritoneum is sometimes found to be. Of this there is evidence in plenty, none perhaps more striking and conclusive than that which is supplied by a most strange and most interesting case narrated by Dr. Murchison in the forty-first volume of the 'Medico-Chirurgical Transactions.' A perverse hysterical girl, contrived, among other extraordinary pranks, to establish an oval opening into her stomach, three inches by four in dimensions, by secretly but continuously pressing an old-fashioned copper penny piece upon her epigastrium. Through this wide window, as it were, the interior of the stomach could be seen and felt, and was not seldom everted and extruded, turned inside out and handled, without pain, or any worse consequence than a very transitory feeling of sickness and faintness.

What is described in books as *gastritis*, means inflammation of the *mucous membrane* of the stomach: and almost all that we know, for certain, of this disease, we derive from observation of the effects of strongly irritant substances upon that membrane. Idiopathic gastritis, in an acute form, I never saw. Acute gastritis, from the contact of corrosive or acrid poisons, I have frequently seen: and a highly interesting affection it then becomes. This is a subject that cannot be thoroughly discussed in this course of lectures: neither may it be altogether omitted.

When an irritant poison has been received into the stomach and excites inflammation there—or when acute inflammation arises from any cause—the symptoms which mark that inflammation are pain, usually of a burning character, in the epigastrium; with frequent vomiting, especially upon the entrance of anything into the stomach; and often with hiccup, and with tenderness and tension of the upper part of the abdomen. To these local symptoms are added fever of a low type; and a small weak pulse. At first, indeed, the pulse, although small, is generally sharp and hard; but it soon becomes thready and feeble. The muscular power undergoes a corresponding depression; the patient is pale and faint, with collapsed features, cold extremities, and a damp skin.

In all this we see a strong tendency to death by *asthenia*. Upon this remarkable sympathy between the heart and the stomach I have had frequent occasion to insist. You are aware that a smart blow upon the epigastrium may put a sudden stop to the movements of the heart, and induce mortal syncope; without leaving any local trace of its operation. On the other hand, a person in a state of extreme exhaustion and faintness, will sometimes revive at once, upon swallowing into the stomach an ounce or two of brandy, and recover his pulse and colour much too speedily to allow of our ascribing these effects to the absorption of the alcohol into the blood. Dr. Alison suggests that the depression of the circulation may be attributable to the peculiar sickening pain which accompanies inflammation or sudden injury of the stomach. It appears, however, more probable that the remarkable sympathy in question is governed by the nerves of organic life. The great solar plexus of the ganglionic system lies upon the spinal column immediately behind the stomach. The heart is largely supplied with nervous filaments from the same system. Hence we might almost expect that any sudden stimulus applied to this important plexus would excite, and that any sudden depressing influence would subdue, the natural action of the heart. Upon the same principle may be explained the facts that deadly faintness and nausea are apt to result from injury to the testes, which are also abundantly endowed with influence from the nerves of organic life. Be this as it may, it is important for you to know that the mode of dying in these cases is precisely what Bichat describes as *death beginning at the heart.*

The pain that accompanies gastritis is augmented by pressure upon the epigastrium. It is increased also by the full descent of the diaphragm, and the *breathing* is consequently short and constrained. In the most exquisite cases of gastritis, produced by chemical or mechanical irritants applied to the interior of the stomach, the inflammation probably reaches and involves, more or less, the peritoneum. The patients speak of the pain as a pricking and burning sensation; it is attended with great anxiety and restlessness. The sufferer is tormented with extreme thirst, while all that he drinks, even cold water, is almost instantly rejected by vomiting.

Hiccup does not always accompany acute gastritis. It sometimes occurs early; but more generally it comes on late in the disease, when the patient is sunk and much debilitated.

The bowels, in this complaint, are sometimes bound: sometimes, on the contrary—especially when the inflammation has been caused by corrosive poison—dysenteric diarrhœa ensues, with much griping and tenesmus.

Such, then, are the symptoms that indicate the existence of acute gastritis; but you ought to be aware that they occur in varying combinations, and with different degrees of severity; and consequently that the course of the disease is not uniformly the same in all cases. When the symptoms are the most violent, and the progress of the complaint is the most rapid, the peritoneal coat of the stomach is usually, I believe, more or less implicated.

Intense inflammation of the stomach may be expected to be rapid in its progress. It may destroy life within twenty-four, or even twelve hours. When it is fatal, it generally is so within a few days; and death takes place by fainting; with a remission of the pain, sometimes very sudden, and sometimes occurring only just before dissolution. But as idiopathic gastritis is rare, *fatal* idiopathic gastritis is, of course, still more rare. Louis states, that during six years' experience at La Charité, in which period he noted the details of 6,000 cases of disease, and of 500 dissections, he did not meet with a single instance of fatal idiopathic gastritis. The subject derives almost all its importance, therefore, from its connexion with poisoning; and the many interesting points of enquiry which arise out of that connexion will be brought before you by the Professor of Forensic Medicine. This consideration is a great satisfaction and relief to me; because I find that the limits of my own course will not permit me to go into any detail in this matter.

The morbid appearances to be looked for after death by acute gastritis, are redness of the mucous membrane, softening, sloughing, and even (after the action of strongly corrosive poisons), perforation of all the coats of the stomach.

I wish particularly to caution you against being misled by mere *redness* of the interior of the stomach; or of the inner surface of the alimentary canal in general; or of *any* mucous membrane; and, indeed, I may add, of any *serous* membrane also. Redness and inflammation have been made, too often, convertible terms. Persons finding the inner surface of the stomach red, have thence too hastily concluded that suspicions of poisoning which had arisen were well founded. We are indebted to Dr. Yelloly, in the first instance, and to M. Billard and some other Frenchmen, in the second, for correcting this error—an error which not only was of importance in questions of imputed poisoning, but heretofore ran through and vitiated almost the whole of pathology. Mistaking mere redness for evidence of inflammation, Cullen divided gastritis into two species—one of which he called gastritis *erythematica*; and he inferred from the observation of cases in which the redness of the membrane had been met with after death, that this peculiar kind of inflammation of the mucous coat of the stomach might take place, without fever, pain, vomiting, or any other symptom indicative of gastritis: whereas it is almost certain that, in the cases to which he refers, there really was no inflammation at all. So also Morgagni, puzzled by intestinal vascularity, was disposed to attribute the absence of pain, in what he believed to have been inflammation of the bowels, to a paralytic affection which blunted the sensibility of the parts; and Haller conceived, from so constantly meeting with this vascularity in *his* inspections of the body, that inflammation of the bowels was almost always present in fevers of all kinds; and was frequent in every other complaint. And the same doctrine has been strenuously inculcated of late years, as I dare say you know, by Broussais, in France, and adopted by a vast host of his disciples. Finding the lining membrane of the stomach and intestines red and vascular in most of the bodies of patients who had died of fever, Broussais concluded that fever depends, in all cases, upon inflammation of the gastro-enteric mucous membrane. You will perceive that this doctrine is likely to exercise a bad influence upon the *practice* of those who entertain it. If inflammation constitute an essential part of any disorder, it follows that the *remedies* of inflammation will be adapted to that disorder; and thus, even so slight a mistake as that may appear to be, against which I am now cautioning you, of regarding every surface which is red as being inflamed also, may lead to very mischievous views in respect of treatment.

The redness that is *in*dependent of inflammation may be of various kinds; but the principal cause of it is venous congestion. 'The appearances of vascular fulness (says Dr. Yelloly) in the villous coat of the stomach, whether florid or dark-coloured, in distinct vessels or in extravasations of different sizes, are not to be regarded as unequivocal marks of disease; inasmuch as they occur in every variety of degree and character, under every circumstance of previous indisposition, and in situations where the most healthy aspect of the organ may be expected.' To the truth of this statement I can bear witness, having at one time of my life carefully examined, with a view to this matter, a great number of stomachs in succession, in the dead-house of a large hospital. 'The vascularity (according to Dr. Yelloly) is entirely *venous*, and depends on a power capable of being exercised on the artery itself at the close of life, which carries on the blood to the veins, after the further supply of fresh blood from the heart is stopped. The branched or stellated form of vessels, under which the vascularity usually appears, is capable of being *imitated*, either by injecting the veins with fine injection, or by forcing back with the finger, or the back of a scalpel, the blood from the larger branches of veins into the smaller.' 'And this vascularity soon becomes diffused redness, by transudation of the blood through the coats of the containing vessels, just as happens with the bile in the gall-bladder.'

Redness, from mere repletion of the smaller veins, is usually extensive and undefined; except that, being influenced by the force of gravity, it settles into the most depending parts of the organ, which are either its exclusive seat, or at any rate are of a deeper colour than the parts more elevated. It is attended with an empty state of the arteries, and with a full state of the larger veins. Hence the condition of the venous and arterial trunks, and especially of the vena portæ, should, in doubtful cases, be ascertained before the main blood-vessels are laid open, and drained of their contents.

The redness that belongs to inflammation is generally circumscribed, and of limited extent; it occupies indiscriminately the upper or the lower side of the tube (for these remarks apply alike to the stomach and to the intestines); it is attended with some fulness of the corresponding arterial trunks; and it may or may not be coincident with comparative emptiness of the venous system within the abdomen. Much will depend, in this respect, upon the *mode of dying*, as I have fully explained to you on a former occasion.

You will please to remember, then, in all your future investigations into morbid anatomy, that it is generally difficult, and often impossible, to determine, from the aspect of the vessels of a dead part, from its redness, that inflammation had been present in that part during life, unless the unequivocal products or effects of inflammatory action are present also.

A much more certain evidence of inflammation of the mucous membrane of the stomach and intestines, is its *softening*. This can seldom be attributed to anything else, unless it be to decomposition, or to the solvent action of the gastric juice. Neither of these last causes can come into operation until life is extinct. It is well known that the membrane is slow and late in passing into the state of putrefaction after death. To the effect of the gastric juice in softening and dissolving the coats of the stomach, and to certain important questions respecting their perforation, I shall by and by return.

I say that gastritis is most commonly the effect of poisons applied to the mucous surface of the stomach; but I must include under that head certain substances, which, to most people, are not poisonous or injurious at all, and which only become so to some persons under particular circumstances. Thus, large draughts of cold drink, taken when the body is hot, and rapidly parting with its heat, and especially large draughts of cold *sour* liquors, as cider or stale beer, are apt to give rise to acute gastritis. Another occasional cause of gastritis is the ingestion of very large quantities of food at one time, especially during convalescence from any serious disorder. It is an exceedingly curious fact, too, but one which I merely mention without dwelling upon it, that certain poisons introduced into the body through some other channel, will cause inflammation of the mucous membrane of the stomach, with which they have *not* been in contact. Corrosive sublimate, and arsenic, excite inflammation, with ulceration or sloughing of the gastric mucous membrane, even when they are merely rubbed, in a certain quantity, upon the skin; or when they are applied to the surface of a wound, or inserted into the rectum.

The treatment of acute gastritis is simple. Early in the disease, and especially if the pain be severe, apply leeches to the epigastrium, and cover the bleeding bites with a soft, light, and warm poultice. If the small feeble pulse grow fuller and stronger, you may conclude that the bleeding has done good. Keep the patient as strictly as you can at rest, in the horizontal posture: in other words, see that the depressing influence of the disease upon the action of the heart is not aided and augmented by the position of the body. If cold water be retained, that is the best medicine which you can give by the mouth; purgatives so administered would be almost sure to be rejected; and if not rejected, they would be likely to increase the existing inflammation of the organ. Enemata are, however, extremely useful: of warm water, if the bowels are not much confined; of purgative materials if they are. After the intestines have been thus cleared—or when they are loose and irritable—opiate injections (thirty or forty drops of laudanum, with three or four ounces of starch or gruel) do much good. They often have a very tranquillizing effect upon the irritable stomach, and check the vomiting. These measures are to be pursued until the inflammation has subsided. If the stomach be capable of retaining any nutriment at all, it must be given in small quantities, at distant intervals, in a liquid form, and of the blandest kind: barley-water, milk diluted with water, arrow-root, smooth gruel, and the like.

When any *corrosive* substance has been swallowed, I scarcely need say that pains should be taken to remove it as speedily as possible from the stomach: or to administer such remedies as are known to be capable of decomposing the poison, or of affording a specific antidote to it. Not that the stomach-pump should be employed in such cases, as too often it is. These, however, are points that must be fully treated of in the lectures on Forensic Medicine, and therefore I shall dwell upon them no longer here.

Chronic inflammation of the stomach is probably a very common disorder. Except when it results in ulceration, it does not put life in imminent jeopardy: and it is often recovered from. Deranging, however, the functions, and perverting the feelings of the stomach, it gives rise to the manifold and multiform symptoms of *dyspepsia*. But dyspepsia, with its manifold and multiform symptoms may be, and often is, entirely independent of inflammation. You see, then, why the effects of chronic gastritis are various; and why the symptoms that are supposed to denote its presence are apt to be obscure, uncertain, and equivocal. I intend, before I quit the subject of the stomach, to investigate the principal circumstances that mark its functional disorders, and to describe the means which we sometimes find conducive to their relief. I shall therefore restrict myself at present to a few points which seem to have been fairly ascertained respecting chronic gastritis.

We know that chronic inflammation had been going on in the stomach when, after death, we see that its coats are thickened; or when we perceive that a portion of one or more of them has been removed by ulceration.

It is not at all uncommon to find the mucous membrane of the stomach, over a larger or smaller space, thick, granular, uneven, and of an unnatural colour. Gray, or slate-coloured, it often is. The slate colour is much dwelt upon by the French writers, as being a sure and unequivocal impress of chronic inflammation. The colour proceeds, I believe, from the operation of the gastric acids upon the blood, which, under habitual congestion or slow inflammation, is detained in the vessels of the altered part. The ulcers that result from chronic inflammatory action are usually small, varying from the size of a split pea to that of a shilling; sometimes with no surrounding vascularity or thickening at all, but looking exactly as though a piece of the mucous membrane had been struck out by a stamp; sometimes with rounded and elevated *edges* only; and sometimes they occupy *patches* of thickening and induration of the parietes of the stomach. Generally there is but one solitary ulcer. Its most usual situation is the posterior part of the stomach, in or near its smaller curvature, and nearer the pyloric opening than the cardiac. More rarely it occupies the anterior part. Now and then an ulcer is found on both the back and front surfaces, at exactly opposite spots. Sometimes two, or three, or more ulcers, are met with in the same stomach. It is very seldom, however, that they are numerous.

Ulcerative disease of the stomach may prove fatal in various ways. The ulcer may penetrate as far as the peritoneum, and excite inflammation of that membrane, whereby the stomach becomes adherent to the neighbouring parts. In these cases, prior or subsequently to adhesion, death may at length ensue, from gradual exhaustion and protracted suffering.

If an ulcer happen to lie over the track of a large blood-vessel in the stomach, it may eat its way into that vessel, and give rise to fatal hæmorrhage.

Or the ulcer may perforate the walls of the stomach, without any previous adhesion, and suffer the food, or the secretions of the stomach, to pass into the peritoneal cavity, where intense inflammation is lighted up, and the patient soon perishes.

Or the ulcers may at length *heal*. Of this we are certain, because we often find cicatrices denoting the spots which the ulcers had occupied.

Our stock of knowledge respecting this dangerous disease, ulcer of the stomach, has been much extended, and rendered more exact, by the researches of living physicians. There are three English volumes which, if you read them carefully, will furnish you with all that has hitherto been learned on this interesting subject: Dr. George Budd's on 'Diseases of the Stomach;' Dr. Thomas Chambers' on 'Digestion and its Derangements;' and Dr. Brinton's comprehensive monograph on 'Ulcer of the Stomach.' In amplifying somewhat the sketch which I have just given you, I borrow chiefly from these writers.*

Ulcer of the stomach is not an unfrequent disease. Dr. Brinton met with it in about one per cent. of his out-patients at the Free Hospital. It is much more common in women than in men; and it is mainly, though by no means solely, a disease of middle and of advancing life. That it is capable of cure is manifest, as I have said, from the puckered scars which mark the sites of former ulcers. Reckoning from a large number of recorded cases, Dr. Brinton concludes that, in dead bodies, the cicatrix is not less often seen than the open ulcer. There are obvious reasons why a spontaneous cure is not more frequent even than this. The healing of such ulcers must be hindered by the alternate stretchings and contractions to which they are subject in the sudden and repeated changes of volume of the stomach, now full and distended with food or with gas, now empty and flaccid: it must be hindered by the vermicular movements of the stomach during the work of digestion; by the contact of food and drink of various kind and quality; and probably, as Dr. Budd suggests, by the action of the gastric juice upon the soft and recent lymph which must needs form the material of repair in the healing process.

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* Since the delivery of this lecture the profession has sustained a great loss in the premature death of Dr. Brinton.

Of the open ulcers of the stomach a certain proportion only—about one in four—go through; become perforating ulcers. And if it be admitted that of the whole number of ulcers there are as many healed as open, then the ratio of the perforating ulcer to the whole number becomes one in seven or eight. This accident of the ulcer is more than twice as common in females as in males: and it is a curious fact, which I am not able to explain, that it occurs more often in maid-servants, between the ages of fifteen and twenty-five, than in any other class of persons. As life goes on, after the thirtieth year, the liability to the formation of a gastric ulcer increases, while the risk of its perforating the walls of the stomach decreases. According to Dr. Brinton, perforating ulcers of the pyloric extremity of the stomach are more common in men than in women.

This risk of going through has some relation to the position of the ulcer; and it is a relation which is quite intelligible. Perforation is much more frequent in the anterior than in the posterior wall of the stomach. The posterior wall is at once the most subject to ulcers, and the least subject to perforation: it is more closely and more constantly applied to the solid abdominal viscera; its movements over them are fewer and less extensive; and it therefore more readily contracts adhesions with them, which adhesions prevent its perforation, in both senses of the word prevent. The threatened aperture is more often stopped by an adherent pancreas than by any other viscus; but adhesion may take place with the liver, with the colon, or with any part that happens to lie in contact with the stomach. When the colon is the attacked part the ulcer may indeed penetrate into that intestine, but I use the word perforation to express the formation of an opening that communicates with the general cavity of the peritoneum. Without any such communication the ulcer may eat its way beyond the stomach, and produce limited abscess in adherent organs or tissues.

Perforation, when it does occur, may result from sloughing or rupture of the peritoneal coat of the stomach, in the sometimes slow, sometimes rapid progress of the deepening ulcer: but it is more often caused, at last, by pressure of some sort, which suddenly breaks the thinned and fragile membrane. The instant of the rupture is marked by definite and terrible symptoms. It has frequently happened just after a hearty meal; and during the acts of vomiting, and of straining at stool. It has been known to take place in the effort of sneezing; under the sudden compression of the waist by a tight belt; from a rough jolt in a dog-cart. These facts suggest a caution to ourselves—how we handle in such cases the epigastric region, or explore the abdomen by pressure.

A certain number of the ulcers (from 4 to 5 per cent. it is calculated), prove fatal by erosion of a large blood-vessel, and consequent hæmorrhage. As the ulcers most commonly occupy the lesser curvature or the posterior wall of the stomach, it follows that the coronary artery which runs along its lesser curvature, and the splenic which crosses its posterior surface, taking its course along the upper border of the pancreas, are the arteries most obnoxious to this erosion. Here is a drawing after Sir Robert Carswell, representing an ulcer which laid open the coronary artery of the stomach and caused fatal hæmorrhage: you may observe that it shows also three scars of healed ulcers. The hæmorrhage is usually abundant, and the vomiting of the blood is preceded by faintness, or actual syncope. It is not however always followed by immediate death. Indeed Dr. Budd states that it was fatal *at the time* in one instance only, among 'a considerable number of cases of the kind' that had fallen under his own observation. The bleeding is capable of being somehow staunched, and the injury repaired; and the danger, if not permanently averted, yet postponed. An example of death from this cause is detailed in the 'Journal Hebdomadaire,' for May 1830. The patient had vomited considerable quantities of blood for eight days in succession, five years previously to the attack which terminated his life. So that hæmatemesis from a ruptured blood-vessel in the stomach is not absolutely hopeless.

As this accident of the ulcer is, after all, somewhat rare, I will briefly relate an instance of it, which occurred in the year 1831, at St. Bartholomew's Hospital. Dr. Latham, who had charge of the case, was good enough, some time ago, to give me the following history of it. The subject of the disorder was a man thirty-eight years old. He was admitted on the 19th January. His countenance was dusky, but exsanguine; his pulse 100, and weak; his tongue pale, and slightly furred. He made no complaint of pain anywhere.

He had been ailing for two years; had suffered much pain across the epigastric region; and had frequently vomited his meals. Two days before, he had been suddenly attacked with faintness and giddiness, and then vomited about two quarts of blood. He was an habitual spirit-drinker.

In the afternoon of the day on which he entered the hospital, he was again seized with giddiness; and fell into a state of syncope, in which he remained for several minutes. Upon recovering, he vomited a large quantity of blood, not less than three pints. The next morning, early, he brought up a like quantity, under similar circumstances; and he passed three evacuations from the bowels, all of them *black*. He was gradually sinking during the whole of that day, the 20th. Towards the evening, he vomited about half a pint more blood. He died quietly the next morning.

When the abdomen was laid open, the stomach was seen to be distended. The intestines had, in several places, a black appearance; from the colour of their contents. The stomach contained about two pints of coagula, and of a dirty red liquid. At the upper part of its lesser arch was a small excavated ulcer, with hardened edges. In the centre of this ulcer there were visible the orifices of three or four arteries, filled with minute clots of blood.

Blood to a less amount, and by a slower drain, and less arterial in character, darker and more tar-like, may be poured out from many ruptured capillary vessels, in the stomach itself, or in the adherent and eroded pancreas, liver, or spleen.

The symptoms that indicate the existence of ulcer of the stomach are, in kind, the symptoms that accompany chronic gastritis: pain or uneasiness in the epigastrium increased by pressure, increased also on the introduction of food, or perhaps felt only while digestion is in progress; flatulence and eructation; vomiting of mucus, and of the meals; loss of sleep; languor and debility.

By closely observing the course and succession of symptoms of this sort we may often arrive at a tolerably sure diagnosis of the presence of an ulcer.

Some of the most fearful cases of perforation of the stomach, those I mean which are apt to occur in young unmarried women, run apparently a brief course, and are attended with few or but slightly marked symptoms. These patients are, however, mostly anæmic; and when questioned, generally confess to previous dyspeptic feelings. It has been fancied that some derangement of the uterine functions may be influential in causing this mysterious and terrible form of ulceration: but it has happened prior to the period of puberty, and when the menstrual flux has been complete and regular, as well as when it has been scanty and suspended. The ulcer has always the punched-out character.

In slower cases (and they sometimes go on for years), the symptoms, equivocal at first, become more and more significant as the disease proceeds. One leading symptom is *pain*—felt in a circumscribed space in the epigastrium, and often at the same time, or alternately, in the back, just below the shoulders. The pain begins immediately upon, or very soon after, the entrance of food into the stomach; especially of food or drink which is hot or stimulating. It usually continues until the digested aliment has passed the pylorus; or until vomiting puts an end to it. The pain is produced or aggravated by pressure, by exercise, curiously too, by mental anxiety,—mitigated by recumbency—and accompanied frequently by sour eructations.

Vomiting is another of the principal symptoms—later commonly in its arrival than the pain; occasional at first; afterwards very frequent. Supposing an ulcer present, this is a very dangerous symptom. It tends to starve and weaken the patient, and so to promote the progress of the ulcer: it augments also the hazard of its suddenly breaking through.

The persistence of these symptoms—which are symptoms of mere dyspepsia also—may justly engender the suspicion of a gastric ulcer: and if, after they have existed for some time, copious hæmatemesis should supervene, the suspicion passes into something like certainty. Dr. Budd holds that if profuse vomiting of blood occurs in a person between the ages of eighteen and thirty, after a long continuance of pain in the stomach, extending into the back, with tenderness of the epigastrium, the pain and soreness being always brought on or increased by meals, with occasional sour eructations and occasional vomiting, with no great wasting or constitutional disturbance, no evidence that the orifices of the stomach are obstructed, and no tumour to be felt—hardly a doubt can remain that the disease is simple ulcer of the stomach.

Some of the last qualifications, and the limitation as to age, are introduced to exclude the only possible alternative of *cancerous* ulceration—of which I shall by and by have to speak.

Having achieved this point in the diagnosis, the ambition of still greater precision is natural and laudable. Dr. Budd thinks that when there is much tenderness of the epigastrium, and no pain in the back, the ulcer is most probably on the anterior face of the stomach. According to Dr. Brinton, under the same *data* the decubitus may sometimes be a guide: the ulcer is most likely to occupy that part of the stomach which is uppermost when the patient is lying in his habitually easiest position.

The practical management of this perilous condition is delicate, but simple and obvious. When the symptoms are urgent, the patient should remain at rest; and even keep himself in the recumbent posture. All food which is likely to create pain by its quality or by its temperature, or which has been found upon trial to give pain, should of course be forbidden. Tepid milk, alone or thickened with biscuit-powder—containing as it does all the elements of nutrition—is probably the very best *kind* of food. The stomach must never be *distended* by a meal; yet the strength and nourishment of the body require to be cherished and sustained. The food must therefore be taken in small quantities, and often; a table-spoonful say, or two table-spoonfuls, every two hours. If the pain be severe, it may sometimes be appeased by counter-irritation; the mustard poultice for example, or a stimulating liniment containing opium, applied to the epigastrium, or to the back. Bismuth, in doses of eight or ten grains, is often found serviceable; it may sometimes be advantageously combined with five grains of the compound kino-powder: or from three to five grains of the compound soap-pill may be given from time to time. When there is hæmorrhage, ice swallowed in small quantities is both beneficial and grateful to the patient: and nutritive enemata may spare the stomach some of its work, and help to maintain his strength. If the bowels be sluggish, their action may be regulated by an aloetic or a colocynth pill.

These are the points to be kept in view. The detail must be left to the common-sense of the practitioner. And when I say this, I am forcibly reminded of a most striking and instructive case, beautifully told by the celebrated Dr. William Hunter, in the sixth volume of the 'Medical Observations and Inquiries.' The perusal of that history has afforded me hints upon which I have often acted with great advantage to my patients, and with some credit to myself, in treating chronic disease of the stomach. As I doubt whether many of you would find immediate opportunity or leisure for referring to the narrative, and as I should spoil it by attempting to give you an abstract of it, I am tempted to read it here in Dr. Hunter's own words.

'Many years ago (he says) a gentleman came to me from the eastern part of the city, with his son, about eight or nine years old, to ask my advice for him. The complaint was great pain in the stomach, frequent and violent vomitings, great weakness, and wasting of flesh. I think I hardly ever saw a human creature more emaciated, or with a look more expressive of being near the end of all the miseries of life. The disorder was of some months' standing, and from the beginning to that time had been daily growing more desperate. He was at school when first taken ill, and concealed his disorder for some time: but growing much worse he was compelled to complain, and was brought home to be more carefully attended. From his sickly look, his total loss of appetite, besides what he said of the pain which he suffered, but especially from his vomiting up almost everything which he swallowed, it was evident that his disorder was very serious.

'Three of the most eminent physicians of that time attended him in succession: and tried a variety of medicines without the least good effect. They had all, as the father told me, after sufficient trial, given the patient up, having nothing further to propose. The last prescription was a pill of solid opium; for in the fluid state, though at first the opiate had staid some time upon his stomach, and brought a temporary relief, it failed at length, and like food, drink, and every medicine which had been given, was presently brought up again by vomiting. The opiate *pill* was therefore given in hopes that it would elude the expulsive efforts of the stomach. It did so for a time; but after a little use, *that* likewise brought on vomiting. Then it was that his physician was consulted for the last time, who said that he had nothing further to propose.

'Though at first the boy professed that he could assign no cause for his complaint, being strictly interrogated by his father, if he had ever swallowed anything that could hurt his stomach, or received any injury by a blow, or otherwise, he confessed that the usher in the school had grasped him by the waistcoat at the pit of the stomach, in a peevish fit, and shaken him rudely, for not having come up to the usher's expectation in a school-exercise. That though it was not very painful at the time, the disorder came on soon after. This account disposed the father to suspect that the rude grasp and shake had hurt the stomach. With that idea he brought him to me, as an anatomist, that an accurate examination might if possible discover the cause or nature of the disorder.

'He was stripped before the fire, and examined with attention in various situations and postures; but no fulness, hardness, or tumour whatever could be discovered; on the contrary, he appeared everywhere like a skeleton covered with a mere skin; and the abdomen was as flat, or rather as much drawn inwards, as if it had not contained half the usual quantity of bowels.

'Having received all the information that I could expect, and reflected some little time upon the case, I wished to speak with the father alone, in another room; and to give my patient some employment as well as refreshment, asked him to take a little milk in the mean time. But his father begged that taking anything into his stomach might be put off till he got home, because he was certain that it would make him sick; "just before we set out (said he) I gave him a little milk; but he was sick, and brought it all up in the coach, before we had got many paces from the house."

'In the adjacent room I said to the father, This case, sir, appears to me so desperate, that I could not tell you my thoughts before your son. I think it most probable, no doubt, that he will sink under it; I believe that no human sagacity or experience could pretend to ascertain the cause of his complaint: and without supposing a particular or specific cause, there is hardly anything to be *aimed at* in the way of a cure. Yet, dreadful as this language must be to your ear, I think you are not to be without hope. As we do not know the cause, it may happen to be of a temporary nature, and may of itself take a favourable turn; we see such wonderful changes every day, in cases that appear the most desperate, and especially in young people. In them the resources of nature are astonishing.

'Then he asked me if I could communicate any rules or directions, for giving him a better chance of getting that cure from nature, which he saw he must despair of from art.

'I told him that there were two things which I would recommend. The first was not so important indeed, yet I thought it might be useful, and certainly could do no harm. It was to have his son well rubbed, for half an hour together, with warm oil and a warm hand, before a fire, over and all around his stomach, every morning and evening. The oil, perhaps, would do little more than make the friction harmless, as well as easy; and the friction would both soothe pain, and be a healthful exercise to a weak body.

'The second thing that I had to propose, I imagined to be of the utmost consequence. It was something which I had particularly attended to in the disorders of the stomach, especially vomitings. It was, carefully to avoid offending a very weak stomach, either with the quantity, or quality, of what is taken down; and yet to get enough retained for supporting life. I need not tell you, sir, said I, that your son cannot live long, without taking *some* nourishment; he must be supported to allow of any chance in his favour. You think that for some time he has kept nothing of what he swallowed; but a small part must have remained, else he could not have lived till now. Do you not think, then, that it would have been better for him if he had only taken the very small quantity which remained with him, and was converted to nourishment? It would have answered the end of supporting life as well, and perhaps have saved him such constant distress of being sick, and of vomiting. The nourishment which he takes should not only be in very small quantity at a time, but in quality the most inoffensive to a weak stomach that can be found. Milk is that kind of nourishment. It is what Providence has contrived for supporting animals in the most tender stage of life. Take your son home, and as soon as he has rested a little, give him *one* spoonful of milk. If he keeps it some time without sickness or vomiting, repeat the meal, and so on. If he vomits it, after a little rest, try him with a smaller quantity, viz., with a dessert, or even a teaspoonful. If he can but bear the smallest quantity, you will be sure of being able to give him nourishment. Let it be the sole business of one person to feed him. If you succeed in the beginning, persevere with great caution, and proceed very gradually to a greater quantity, and to *other* fluid food, especially to what his own fancy may invite him: such as smooth gruel, or panada, milk boiled with a little flour of wheat or rice; thin chocolate and milk; any broth without fat, or with a little jelly or rice or barley in it, &c., &c.

'We then went in to our patient again; and that he might be encouraged with hope, and act his part with resolution, I repeated the directions with an air of being confident of success. The plan was simple, and perfectly understood. They left me.

'I heard nothing of the case till, I believe, between two and three months after. His father came to me with a most joyful countenance, and with kind expressions of gratitude told me, that the plan had been pursued with scrupulous exactness, and with astonishing success; that his son had never vomited since

I had seen him; that he was daily gaining flesh, and strength, and colour, and spirits, and now grown very importunate to have more substantial food. I recommended a change to be made by degrees. He recovered completely; and many years ago he was a healthy and a very strong young man.'

In fulfilment of my promise I revert to the subject of *perforation* of the stomach.

There are three ways, exclusive of mechanical violence, in which such perforation may be effected. In each of the three the perforation proceeds from within outwards. All the coats of the stomach, as we have seen, may be penetrated in succession by a chronic ulcer. The direct contact of corrosive poisons may rapidly eat them through. They may be partially digested and destroyed by their own proper secretion, the gastric juice. Questions of much nicety and of grave importance present themselves, from time to time, respecting holes that are discovered in the stomach after death: medical questions, bearing upon pathology and therapeutics; legal questions, involving life and character in their solution. Perforation by disease, perforation by the gastric juice, are both liable to be mistaken (and often have been mistaken), for evidence of murder or of suicide by poisoning. It is fit that you should have considered these points.

Now of ulcers of the stomach I have already told you all that I know: and it would be impossible for me, as well as out of place, to enter at large upon the topic of corrosive poisoning: that you will hear fully discussed by the Professor of Forensic Medicine. Perforation by the gastric juice demands a somewhat closer attention.

John Hunter, as you probably know, was the first to recognise and announce the remarkable fact that the stomach is capable, through the peculiar fluid furnished by itself, of digesting its own tissues. The dissolving power of the gastric juice survives for a while the vitality of the body, and acts as readily after death upon its parent flesh, as upon the food submitted to its influence during life. This discovery naturally excited great curiosity and interest. Hunter's observations were verified by several of his contemporaries or immediate successors; and hypotheses were soon framed to account for the supposed infrequency of the phenomenon. Dr. Adams thought that the stomach was soluble by the gastric juice, only when the death was general and complete as well as sudden; only, i.e. when the stomach itself became instantly dead, and no organic vitality lingered in its tissues: and he took the continued fluidity of the blood, and the absence of the *rigor mortis*, as tests of such universal sudden death. He correctly supposed also that temperature was concerned in the matter. Mr. Allan Burns remarked that softening and perforation sometimes occurred in persons dead of chronic diseases; and he conceived that the gastric juice could exercise its solvent power, not only after being poured forth into the stomach, but also while still contained in its proper vessels. To this opinion he was led by finding perforation on the anterior face of the stomach. By degrees the simplicity of Hunter's doctrine was obscured and frittered away by hypothetical notions, propounded chiefly by continental writers, respecting the accessory operation of disease in producing these softenings. It was held that either the gastric juice, through some vice of the nervous system, was secreted of an unusually acrid and corrosive quality;—or that the mucous membrane of the stomach was rendered, by some previous morbid condition, more than commonly soluble in its own secretion after death. At length, the effect of the gastric juice was lost sight of altogether, and softening of the mucous membrane was ascribed to the sole agency of a kind of inflammation, or to faulty nutrition.

More than fifty years after the publication of Mr. Hunter's first paper on the subject in the 'Philosophical Transactions,' Sir Robert Carswell endeavoured to bring pathologists back to the truth, in a French essay, read before the Royal Academy of Medicine in Paris, of which a version is to be found in the thirty-fourth volume of the 'Edinbugh Medical and Surgical Journal.' He therein shows that the action of the healthy gastric juice is sufficient to account for changes which—by Chaussier, Broussais, Louis, and others, abroad, and by Dr. John Gairdner in particular in this country, since the time of Hunter, as well as by Morgagni, and still earlier pathologists before that time,—had been attributed to the operation of disease. The whole subject has finally been surveyed and simplified by Dr. Budd, who, in the Croonian Lectures, delivered before the College of Physicians in 1847, has cleared up several of the difficulties that surrounded it, and explained some apparent anomalies. The substance of these lectures has since been published in his excellent book, already referred to, on diseases of the stomach.

For perforation of the stomach, or for softening of its tissues by the gastric juice, three conditions must concur. In the first place the stomach must (of course) contain gastric juice; which appears to be secreted directly into its cavity, and never to be retained, as Allan Burns supposed, within its coats. Secondly, that fluid must possess its natural quality of acidity. Thirdly, a certain degree of heat is requisite for its solvent operation.

Now it has been proved by Spallanzani, and more clearly and fully by the interesting observations of Dr. Beaumont, to which I shall hereafter more particularly refer, that during the state of health, no gastric juice is secreted into the stomach except under the stimulus of food, or of some mechanical irritation, applied to its interior.

Hence we perceive why it is that perforations of the stomach, of the kind in question, are most of all to be expected when a healthy person is suddenly killed by violence, soon after a meal, and while the process of digestion is in progress.

But instances do occasionally happen (Dr. Budd relates a very remarkable one) in which the same kind of perforation is met with, although no food had for some time before death been received into the stomach.

Dr. Budd believes the secretion of the gastric juice to be a reflex process; which he assimilates to, and illustrates by, the secretion of tears. Tears may be presently made to flow by direct mechanical irritation of the conjunctiva, or, indirectly, by pungent vapours acting upon the nostrils, or by certain feelings of the mind. In like manner the secretion of the gastric juice may, he conceives, be excited, not only by some stimulus applied immediately to the mucous surface of the stomach, but also under certain diseased conditions or injuries of distant organs (as the brain, or lungs), and even by mental emotion. In this way he would explain the occurrence of perforation, or of softening, after death by blows on the head, when no food had been recently introduced into the stomach; and after death by pulmonary consumption.

That more or less digestion of the tissues of the stomach after death is exceedingly *common*, is a fact which was well known to John Hunter, but which has been lost sight of by the majority of more recent observers. 'There are few dead bodies' (he writes) 'in which the stomach is not at its great end in some degree digested; and one who is acquainted with dissections can easily trace the gradations from the smallest to the greatest.'

Dr. Budd points out circumstances which frequently interfere to prevent this effect of the gastric juice, by annulling one or both of the other conditions, just now mentioned.

The solvent property of the fluid is arrested whenever its acidity is neutralized by the admixture of an *alkali*. This has been fully proved by Spallanzani and others. The same is true of *alcohol*. But in the last moments of slowly ebbing life, medicines containing ammonia, and alcohol in some form or other, are very commonly indeed poured into the stomach. Moreover, if the gastric acid happen to be present in small quantity, 'it may be neutralised, and thus rendered inert after death, by transudation of the alkaline serum of the blood.'

Cruveilhier found softening of the fore part only of the stomach, in a person who had died of fever, with marked disorder of the brain. Allan Burns also records a case of perforation of the anterior of the stomach; the patient was anasarcous. In both instances the stomach was empty; i.e. 'its surface was merely moistened by the gastric juice.' Dr. Budd supposes that in the first of these cases, the blood, remaining fluid after death, gravitated to the lowest part of the organ, and there gave out its alkaline serum, whereby the small quantity of gastric acid collected in its fundus was rendered neutral and inert; and that, in the second case, the alkaline dropsical fluid, oozing through the coats of the stomach at its lowermost part, had the same effect.

Again, the gastric juice is solvent of those things whereof it is the natural menstruum, at a certain temperature only. Probably it is most active at or about the standard temperature of the body. Below 60° its digestive action is found to be feeble, or extinct. Softening therefore—and *à fortiori* perforation—is more likely to take place in summer than in winter; in warm weather than in cold; in a heated room than in the cool open air; and after some modes of dying, which imply a long retention of the vital warmth, than after others.

I have mentioned, for the sake of explaining them, certain exceptional cases, in which the front of the stomach was digested; but the rule is that the softening, which usually comprehends a considerable space, happens almost always at its largest end, and in its lowest part, where whatever fluid it may contain collects under the influence of gravity. If the surface be wrinkled into folds or ridges, the summits of those ridges may alone be dissolved. Sometimes the stomach, lying across the vertebral column, is partially supported by it, and two little pools, and two spots of softening, are formed, one of them to the right of the spine, towards the pylorus. Now and then the gastric juice passes out of the stomach into the œsophagus, or into the duodenum, and these parts exhibit traces of its action. And when actual perforation occurs, the chemical solution of the animal tissues sometimes extends further; and the organs immediately opposite to the aperture undergo the digestive process:—the spleen, the intestines, the liver, the diaphragm, nay, after penetration of the diaphragm, even a portion of the lung.

These apertures produced by the gastric juice have soft and ragged edges, and are irregular in their size and outline. When the softening has stopped short of perforation, the mucous membrane looks and feels pulpy, like paste, or is completely dissolved and gone. The pulp varies in colour from brown to gray, according to the quantity of blood contained in the part. The blood-vessels that ramify over the softened portion are rendered black, or brown, and therefore conspicuous, by the effect of the acid on their contained blood; or, if they are empty, the surface is pale, and presents that uniform, semi-transparent, jelly-like aspect which, under the name of 'gelatinous softening,' has been erroneously spoken of as the result of disease.

The stomachs in which this agency of the gastric juice is discernible show no marks of putrefaction: there is no extrication of gas, nothing of the fœtor of gangrene; but their interior always exhales a peculiar acid odour, and litmus applied to the softened spots turns red.

Chronic *ulcers*, on the contrary, affect chiefly the lesser curvature of the stomach, and are commonly situate nearer the pyloric orifice than the cardiac: their margins are thickened by inflammation; or, if not thickened, the edge of the hole in the mucous membrane is smooth and regular. They are not necessarily associated with a sour smell, nor with acid reaction upon litmus paper.

Perforations of the stomach by corrosive poisons are discriminated from other perforations by the specific chemical tests of the presence of those poisons; by the amount of disorganisation which they have produced; by traces of their corrosive action upon other parts—in the mouth, in the fauces, in the œsophagus; by the violent symptoms which precede the fatal result of their operation; and (often) by the history and moral features of the case.

If you bear in mind the particulars that I have thus hastily brought together, they will, I trust, enable you to avoid wrong inferences, which you might otherwise be led to form concerning the morbid appearances and real conditions of the tissues of the stomach, laid open to your inspection after death.

The stomach is very frequently the seat of specific malignant disease; of cancer in its various forms and denominations. The fatal nature of this complaint; the obscurity in which it is sometimes wrapped; the possibility of overlooking it altogether, or of confounding it with disease of a more innocent character, combine to invest it with peculiar interest.

Carcinoma of the stomach has sometimes no symptoms at all, or none which the most sagacious practitioner would refer to the organ affected. Not long since I saw, in consultation, an elderly clergyman, who complained of pains in his back, which were brought on or aggravated by certain movements of the body. His bowels were costive: and purgatives always relieved his pains. He was passing lithic acid gravel. The pains were felt in or near the renal region. Several years before he had suffered in a similar manner; and had then been cured by being cupped on the loins. What was the matter here? Was it lumbago? Was there a calculus in one of his kidneys? These were the best guesses that I could make. The eminent physician whom I met, and a surgeon of no less eminence, who had seen the patient previously, had not been able to attain any more exact diagnosis. Upon this gentleman's death, which occurred not long afterwards, his disorder was discovered to have been cancer of the stomach. Excepting slight sickness a day or two before he died, there had been no symptom to direct attention to that part.

A young woman came into the Middlesex Hospital, under one of my colleagues, with a pulsating tumour in her epigastrium. It was thought, at first, to be an aneurism, and the case attracted, on that account, a good deal of notice. But the tumour subsided very much after free purgation. This led some to suppose that it was formed by accumulated fæces in the transverse colon. There was no sickness; nor indeed any one symptom referable to the stomach. She died. The tumour was cancerous; and in the stomach. Lying in front of the abdominal aorta, it had been lifted by its pulsations.

I was summoned to one of the hotels in Albemarle Street, to see a gentleman between forty and fifty years of age, who was on his way home from the Scottish Highlands, where he had been deer-stalking, and shooting grouse. He had been seized in the night with deadly faintness, very rapid breathing, and severe pain, which he referred to the sternum. I could detect no faulty sounds in his heart, or in his lungs. His epigastrium felt full and pulpy. The next night he had a similar paroxysm, in which he died. His body was examined by Mr. Paget. The lungs and heart were sound in structure. The large curvature of the stomach presented, throughout its whole extent, a mass of scirrhus lying beneath and among thick ridges of mucous membrane, with two or three deep patches of ulceration. Both the cardiac and the pyloric orifice were free from change.

This case, which I saw in consultation with Dr. Turner, of Keith, who had accompanied the patient to London, was imperfectly reported by me on a former occasion. In fact I had stumbled in the diagnosis, and hearing that the patient had for some weeks suffered dyspnœa and palpitation of progressive severity on slight bodily exertion, I thought that he probably was the subject of fatty degeneration of the heart, and consequent portal congestion. Dr. Turner had construed the symptoms more correctly, and it is due to his better sagacity that I should here acknowledge my own shortcomings. From the first he thought the stomach, and the stomach only, to be the seat of the disease. The patient had gradually lost flesh and strength. He had pain after eating, referred always to one circumscribed spot in the epigastrium, and sour eructations. At a later period pitchy alvine dejections, consisting chiefly of altered blood, had occurred; at first once or twice daily, and then recurring at irregular intervals of from four to ten days. The dyspnœa and palpitation were rightly attributed by Dr. Turner to the anæmic condition to which the patient had been reduced.

What is especially to be noticed in this case is the entire absence of vomiting, notwithstanding the great amount of organic mischief.

Dr. Turner told me that the epigastric pain was always, and *immediately*, alleviated upon the patient's swallowing a little undiluted brandy; and that his experience had led him to attribute considerable value to this effect of alcohol, as an aid towards the diagnosis of similar cases.

Instances to the same effect are related by Dr. Seymour, in the 'Medico-Chirurgical Transactions;' and by M. Andral, in his 'Clinique Médicale.'

But even when the stomach is the organ pointed out, by the symptoms, as the probable seat of the malady, those symptoms fail, often, to indicate with any certainty its nature. The effects of the carcinomatous disease exhibit no uniformity. The ingestion of food is apt to produce great distress; but differently in different cases: sometimes as soon as the food is swallowed; sometimes not for an hour or two afterwards. Some cases are attended with much pain; some with none at all. One patient vomits continually; another has little or no vomiting from first to last.

Can these differences be in any way accounted for? Partly they may. By analysing case after case, we approximate to a knowledge of their causes. But this knowledge is yet far from being complete.

One circumstance that has a considerable influence upon the symptoms, is the *situation* of the disease. With respect to this point there are certain general rules which are for the most part true. Still we can speak of them only as applicable *on the average*; they are not absolute or infallible.

The rules I mean are these:—

1st. That there is more suffering, *cæteris paribus*, when the cancerous disease is situate at, or very near, either extremity or orifice of the stomach, than when it occupies the intermediate parts; whether in the greater, or in the lesser curvature.

2nd. That when the cardia, and its immediate neighbourhood, is the part solely or principally diseased, the food and drink find a hindrance in passing into the stomach; but being once there, the distress is over. The symptoms are very like those of stricture of the œsophagus; and in fact the œsophagus is often involved in the disease. The morsel reaches the bottom of that tube, and there causes uneasiness, till at length it is brought up again through the mouth, or passes gradually in the natural direction.

3rd. That when, on the other hand, the disease is limited to the pyloric end of the stomach, the food enters that bag readily enough, and remains there for a certain time; then uneasy sensations arise, and the imperfectly digested meal is apt to be rejected by vomiting.

It is the difficulty of passing the *doorways* in these cases, that gives rise to the principal suffering: the difficulty of getting into, or the difficulty of getting out of, the stomach. But when the disease is confined to the intermediate space, no such difficulty occurs: and therefore little or no pain.

You must expect, I say, to meet with individual variations from these rules. A remarkable example of such variation was presented by one of my hospital patients, in the year 1837.

I have the notes of that case before me, which I will read *short.*

Simon Ailes, aged thirty-six, admitted March 14. His main complaint was of pain in the epigastrium, always present, but augmented, in frequent paroxysms, to an extreme degree of severity. At first, pressure gave him some relief. The pain was most violent an hour or two after he ate. He was troubled also with flatulence, and with sour eructations. Occasionally a clear tasteless fluid, looking like water, rose into his mouth. His bowels were costive.

At this time his countenance was natural and placid; but it gradually assumed that pinched and anxious expression, and that peculiar yellowish hue, which are so significant of organic visceral disease. He wasted fast. At length the epigastrium became tender as well as painful; but no tumour, except the left edge of the liver, could be felt there. He died on May 11, about eight months from the commencement of the pain. A week before his death he vomited some dark, grumous, offensive fluid, evidently containing blood. With this exception he had no vomiting.

Many remedies were tried, which I do not specify, for none of them gave him any sensible or continued relief.

In the smaller curvature of the stomach we found a ragged, sloughy surface, as big as the palm of one's hand, and extending to about half an inch from the pylorus. A section of this diseased portion exhibited the characters of true scirrhus: a white and hard mass, nearly half an inch across at its thickest part. The mucous membrane of the duodenum was congested, and dark coloured. The rest of the intestines were healthy except the rectum, which was surrounded, towards the anus, by scirrhus and thickened areolar tissue, intermixed in laminæ. The gut itself was not affected.

The diseased stomach was removed, and examined by Mr. Kiernan, who found, upon careful dissection, that the trunk of the gastric branch of the par vagum ran directly into, and was lost in, the scirrhous mass. This sufficiently accounted for the dreadful sufferings of the patient.

And I am here reminded that, with regard to the structural alteration itself, there are some circumstances well worth attending to.

Andral places all these organic affections of the stomach in the class of chronic gastritis. But it is clear that he is wrong: and you will perceive at once that it is of immense importance to recognise the specific disease from the mere result of common inflammation.

But though cancer is not, in any case, a mere product of common inflammation; neither is all that is called cancer really such. Specimens of morbid texture, misnamed scirrhus of the pylorus, are not uncommon in anatomical museums. I show you some from our own; not so denominated, however. The correct labelling would be *hypertrophy*. You may perceive that the areolar and the muscular tissues near the pylorus are very much thickened. A section of the thickened parts presents an appearance somewhat like horn; and is crossed by whitish lines that run nearly parallel to each other. The morbid structure is quite definite and uniform; and very dissimilar, in that respect, to the irregular masses of scirrhus, and to the amorphous deposits of encephaloid cancer. Neither does it at all resemble that of the colloid variety of carcinoma. Changes of this kind are liable to occur in the muscular tissue of this, as of other organs, whenever a permanent obstacle is opposed to the onward progress of the contents of the hollow viscus. The impediment may have been originally produced by inflammatory thickening of the textures composing the pylorus; and then the hypertrophy may, in a certain sense, be accounted a consequence of inflammation. To that extent alone is M. Andral right. He has unquestionably pushed his theory on this subject too far. Whatever narrows the pyloric orifice leads to increased effort of the propelling muscle, and to augmentation of its bulk and power. Now cancer itself, situate at, or close upon, the pylorus, may impede the exit of the digested aliment; and then it causes a gradual hypertrophy of the muscular coat. In these cases there is a mixture of the two changes; of the cancerous growth, with the muscular hypertrophy: and this is one reason why they have been confounded together. Here are several preparations, exhibiting true cancer of the stomach. At the bottom of each bottle may be seen a sort of whitish powder or sediment, consisting of some of the matters peculiar to cancer. This fact has been pointed out to me by Mr. Kiernan in the numerous specimens contained in his private collection. But there is no such deposit when the hypertrophy is not combined with malignant disease.

To those who are conversant with its revelations, the microscope, in equivocal cases, becomes a valuable diagnostic test.

It has long been thought and asserted, that cancer of the stomach is not so apt to be attended, as cancer of other parts, with a disposition to present itself in various organs of the body at the same time, or in succession. Now I believe—and I am glad to add the weight of Mr. Kiernan's authority to my statement—that this is not really so. Cancer is a constitutional affection: or, if local and solitary in the outset, is prone to disseminate itself. The error has arisen out of that confounding of one morbid condition with another, against which I have been warning you. Instances are not at all uncommon of thickening of the areolar and mucous tissues about the pylorus, producing first a mechanical impediment to the passage of the food, then more energetic muscular efforts towards its expulsion from the stomach, and at last hypertrophy of the ring of muscular fibres. In these cases, you do not find cancer in other organs: because, in fact, there is no cancer in the stomach.

You may say that as both forms of disease are alike fatal, it signifies nothing whether there be really cancer or not. But it is always satisfactory to clear away an apparent anomaly, and to show that it has no real existence. Besides, you know with how much anxiety the relatives of the dead inquire concerning these matters. That cancer 'runs in families' is well understood even by the public. An example of this hereditary disposition has just occurred to me in practice (1853). A patient of mine, a barrister, forty-eight years old, has sunk under scirrhous disease involving the omentum, and extending into all the folds of the peritoneum. This gentleman's mother died of malignant disease of the leg, where it commenced as a small wart. Her brother died of cancer of the lungs which penetrated the sternum, and sprouted out upon the chest. And this brother's wife, who was his cousin also, fell a victim to some form of cancer. The first Napoleon died of cancer of the stomach; so did his father and his sister. The distinctions which I have been pointing out are surely worth learning, if they do no more than enable us to comfort the minds of survivors, and to relieve them from the apprehension that they also may be doomed, or likely, to become the subjects of this horrible disorder.

Notwithstanding what I have now been saying, it must be allowed that genuine cancer of the stomach is accompanied less frequently than some other modes of carcinomatous disease, by cancer elsewhere. In fact cancer of the stomach is most often of the scirrhous variety, which until it softens is not so readily disseminated as the others, and which is apt to prove fatal before it softens.

When primary cancer of the stomach is of the encephaloid kind,—or when, being scirrhus, it begins to grow soft,—its secondary manifestation occurs chiefly, as I told you on a former occasion, in the *liver*: the reason being that the veins of the stomach communicate with the general system not directly, but through the hepatic capillary system. Cancer of the colloid or gelatiniform species, with which also the stomach is liable to be affected, spreads mainly, when it spreads at all, by inoculation of parts of the body that happen to be in contact with it.

But to return to the *symptoms* of carcinoma of the stomach. In some cases, I say, the food is rejected by vomiting; in some cases it is not. Now it has been argued that this difference depends upon the condition of the pyloric outlet; whether it be free and open, or contracted and shut. The explanation is more plausible than sound. It is not strictly consistent with facts. Vomiting of the food has been an urgent symptom, when there was no mechanical bar to its passage into the duodenum. The pylorus is a sphincter muscle, of which the natural and habitual state is that of contraction. It yields, however, in health, to the pressure of the digested aliment, which is driven forwards by the muscular fibres that surround, and compress by their action, *the pyloric end of the stomach*. If there be a mechanical impediment, that affords a sufficient reason why the food should be thrown up again. But sometimes, I repeat, the orifice is wide open, and yet the food is rejected: and it is rejected because the disease so involves the pyloric end of the stomach, that the propelling force cannot be exercised.

When there is a mechanical obstacle, the disposition to hypertrophy of the muscular coat is conservative. But in feeble and delicate persons, the baffled muscles may never acquire strength enough to overcome the impediment; and then the very opposite condition is apt to take place: the coats all become very thin; meal after meal is retained; the stomach is enormously distended, and relieves itself now and then, at distant intervals, by copious vomiting; until at last it is unequal to that effort, and the patient dies.

Sometimes the sickness and vomiting are urgent even when the stomach contains no food: and the matters rejected are of various character and appearance. They often resemble coffee-grounds, and consist, no doubt, of altered blood. Vomiting of this kind is a very pregnant sign of *organic* mischief in the stomach.

Emaciation is another ugly circumstance in these cases; and forms a strong ground of presumption that the symptoms depend upon structural disease. Yet it is not a uniform consequence, even of malignant disorganisation of the stomach. Napoleon Bonaparte was very fat when he died. His omentum is described as having been 'remarkably fat:' and 'the fat was upwards of an inch thick upon his sternum, and one inch and a half upon his abdomen.'

The existence of a palpable tumour strengthens the unfavourable diagnosis. But this is far from being a constant phenomenon. It is not even pathognomonic when it does occur. The diseased head of the pancreas has been mistaken for a thickened pylorus. The stomach is liable also to be dragged much out of its place; and then a thickened pylorus may be mistaken for something else. Sometimes the form of the stomach may be distinctly traced. In the person of a medical practitioner who died lately in this neighbourhood, the shape of the organ, its occasional peristaltic motions, and the irregular and hardened pylorus, were plainly to be felt. They might indeed almost be *seen*, in the hollow and attenuated abdomen. When a tumour is ascertained to belong to the stomach, it indicates disease of the pylorus rather than of the cardia.

In equivocal cases the diagnosis may sometimes be aided by remembering the facts that cancer of the stomach rarely happens before the age of 35; that it is steadily progressive, and generally kills the patient within 12 or 15 months; and that it is attended with a peculiar sallowness of complexion, and gradual wasting of the flesh: whereas chronic ulceration of the stomach may arise at any period of life, and may continue to exist for 5, 10, or even 20 years together, without much constitutional disturbance, or marked diminution of the general bulk and strength.

It is a curious feature in these malignant diseases of the stomach, that the symptoms sometimes remit, in a remarkable manner; so as to excite a hope in the mind of the patient, and in that of his medical attendant, that the nature of the malady had been mistaken, and that recovery is about to take place. But the truce is not for long. Frightful disorganisation is at length produced, ragged ulceration, perforation of the coats of the stomach, adhesion to the parts adjacent, which thus are constituted adventitious walls;—and inevitable death at last.

The treatment of this dreadful complaint can only be palliative. If there be pain, we are driven, sooner or later, to opium.

Anodyne enemata have often as good an effect in relieving the pain as opium given by the mouth; and they have this advantage, that their constipating properties are more easily obviated than when that drug is put into the stomach. Or the remedy may be used hypodermically. Nutritive injections are proper when food taken through the natural channel is not retained.

Other palliative measures may be aimed at particular symptoms: of these I propose to speak when I come to the symptoms and remedies of *dyspepsia*.

LECTURE LXIII.

Hæmorrhage from the Stomach: sometimes from a large vessel, usually capillary. Idiopathic Hæmatemesis. Vicarious Hæmatemesis; Hæmatemesis from Gastric Disease or injury; from disease in other organs. Melæna. Hæmatemesis from a morbid state of the blood. General phenomena of Hæmatemesis. Diagnosis. Treatment.

HÆMORRHAGE from the stomach, to which I wish next to direct your attention, is of much more frequent occurrence than acute gastritis. It is a complaint, or a symptom, that presents several points of interest and importance. I use the phrase 'hæmorrhage from the stomach,' rather than the single term 'hæmatemesis,' because that term, signifying strictly a *vomiting* of blood, does not necessarily imply hæmorrhage *from the stomach*; nor, indeed, does it always accompany such hæmorrhage, although it is one of its most common and most striking symptoms.

What I have so frequently mentioned in respect of hæmorrhages from the mucous membranes generally, viz., that the efflux of the blood is seldom owing to the rupture of a large blood-vessel, holds true in this. It can rarely happen that any vein or artery belonging to the stomach is divided or laid open by accidental injury, so as to pour forth its blood. When hæmorrhage does proceed from one or more of the larger blood-vessels, the opening by which the blood escapes is commonly the result of chronic ulceration; such as I spoke of yesterday. Sometimes hæmatemesis is a consequence of the erosion of a blood-vessel by an ulcer in the *duodenum*; such as I mentioned before as being apt to follow severe and extensive burns. But hæmorrhage from the stomach, and from the alimentary canal generally, is far more commonly what we have agreed to call *capillary* hæmorrhage.

Now this kind of hæmorrhage happens under various circumstances; and is attended with different degrees of danger. 1. The bleeding may be idiopathic. 2. It may be vicarious of some other habitual hæmorrhage. 3. It may depend upon disease or injury of the stomach itself. 4. It may be the consequence of disease situate elsewhere, and producing, mechanically, a plethora of the veins of the stomach. 5. It may result from a morbid condition of the blood, and form one symptom of a more general disease; as in the passive hæmorrhages of purpura and sea-scurvy. Each of these varieties requires a short notice.

1. Hæmorrhage strictly *idiopathic*—i.e. independent of any apparent change of texture, whether in the surface itself, or in any part obviously capable of influencing its blood-vessels—is as rare, I believe, from the mucous membrane of the *stomach*, as from that of the *lungs*. I have never seen, nor do I recollect to have read of any instance of, hæmatemesis analogous to the *epistaxis* which is so common in children and young persons; and which affords the most familiar example of idiopathic hæmorrhage.

2. But hæmorrhage from the stomach, occurring in connexion with other constitutional hæmorrhages, or in their stead—and above all, occurring *vicariously* of menstruation—is *abundantly* common. It is the most common indeed of all the species of *hæmorrhage by deviation*. I told you, in a former lecture, that patients will sometimes menstruate for years together through the lungs; without any apparent injury to their general health. More commonly still do they menstruate through the stomach. I will mention one concise but singular example of this which I had from Dr. Latham, and which came within his own knowledge. A young woman became the subject of hæmatemesis, recurring at monthly periods, about the age of fourteen. She had never menstruated. This continued until she married and, in due time, fell with child. Thereupon, the hæmatemesis ceased. She brought forth and suckled her infant. During lactation the hæmorrhage did not recur. It came on again soon after she ceased to nurse the child; no regular menstruation by the uterus having ever happened. This was the woman's own account, and there appeared no reason to question its accuracy.

Gastric hæmorrhage of this kind, vicarious of regular menstruation, is not generally thought to have any tendency to shorten the existence of those who are afflicted with it. Cullen states broadly that this species of hæmatemesis is hardly ever a dangerous disorder: and this is true. Yet it is not so *entirely* free from peril as to preclude the necessity of *some* caution and qualification in stating the prognosis. The exhaustion from the mere loss of blood is sometimes so great as to create serious alarm for the patient's safety. And Mr. North has recorded (in the 'London Medical and Physical Journal') two instances in which suppressed menstruation was followed by repeated and at length *fatal* hæmatemesis. In neither of these women was the health seriously deranged; nor, previously to the hæmorrhage, did there exist debility, or any other symptom calculated to excite the apprehension of danger. In fact, in both of these cases, a strongly favourable prognosis was given by experienced physicians a very short time only before the fatal event.

3. Gastric capillary hæmorrhage is often a consequence of *disease or injury of the stomach itself.* It is sometimes one of the earliest declaratory symptoms of scirrhus or cancer of that organ—occurring long prior to ulceration. Hæmatemesis attends also, very commonly, the *ultimate* stages of that fatal disease: and then it may be owing to the erosion of some vessel of notable magnitude, in the course of the process of disorganization, as in the examples already spoken of: or (what I believe is far more common) it may result from a kind of general oozing from the ulcerating surface. Blood is often vomited soon after the reception of strongly irritant poisons into the stomach. I show you again Dr. Roupell's plate, representing the crimson surface of a portion of the stomach of a dog which had been killed shortly after the administration of a dose of alcohol. The intense congestion thus produced is doubtless *active* congestion; congestion, the result of an increased afflux of blood through the arteries. Pushed a degree further, such congestion passes into hæmorrhage.

4. On the other hand, intense *passive* congestion—congestion arising from the detention of blood in the *veins* by some mechanical obstacle to its progress—is a very common source of gastric hæmorrhage. Hæmatemesis is therefore an occasional symptom of obstructive disease of the heart. Much more frequently, however, it depends upon abdominal changes. The hæmorrhage is symptomatic of disease situate not in the stomach itself, but elsewhere. And the viscera, with the diseases or morbid conditions of which, bleeding from the stomach is most often connected, are the liver and the spleen.

All this is well known: and it is easy to see, from the peculiar construction of the venous apparatus in the abdomen, how disease of one or both of these viscera may produce mechanical congestion of the submucous capillary tissue; and how that congestion may be relieved, under certain circumstances, by the effusion of serous fluid on the one or the other surface, constituting ascites or diarrhœa, as the case may be; or under *other* circumstances, not perhaps easily discriminated or well understood, by the extravasation of the collected blood itself. It would be superfluous to describe the peculiar distribution and functions of the vessels which return the main portion of the venous blood from the stomach and intestines towards the heart. It seems to be highly probable that one at least of the offices of the *spleen* is to provide a receptacle or reservoir for this blood when its free passage through the portal vessels is temporarily obstructed. It then becomes a sort of safety valve (if such an illustration be allowable), which obviates the danger that might otherwise arise to more vital parts from any great or sudden disturbance of the venous circulation. The stress of the congestion is continually felt in the submucous capillary system; and the hæmorrhage which is apt in such cases to occur from the loaded membrane, receives a simple solution upon principles almost purely mechanical. Nay, the very circumstances which lead to the effusion of the blood from the *mucous* surface on the one side, rather than from the *serous* on the other, may perhaps be themselves susceptible of mechanical explanation.

Gastric hæmorrhage, symptomatic of hepatic disease, is chiefly to be looked for in those morbid conditions of the liver which imply obstruction of the portal vein and of its ramifications. We are not surprised, therefore, to find it coincident, often, with a contracted and shrunken state of that organ. The state of the spleen, on the contrary, for reasons that must be obvious to you, is uniformly, in the cases that we are now considering, a state of *enlargement*. And the augmentation of bulk is not so much to be ascribed to disease inherent in its proper texture, as to distention by the mere quantity of blood which it holds. The internal structure of the spleen furnishes a credible presumption in favour of that view of one of its uses to which I just now alluded; and this structure, and this presumed function, when considered together, throw a strong light upon some of the pathological relations of the spleen, which well deserve attention.

Numerous instances are on record of hæmatemesis going along with evident enlargement of the spleen; and in some of them that organ has been observed to diminish in bulk, in proportion as blood was poured out by the stomach. If I am not greatly mistaken, I have more than once seen this myself. In such cases the tumid condition of the spleen may be regarded as an evidence of venous obstruction *elsewhere*; and as depending, sometimes at least, upon disease of a less striking and prominent character in the liver, impeding the progress of the blood through the vena portæ. Of this kind would seem to have been a case related by Morgagni, wherein, after repeated attacks of hæmatemesis, under which the patient sank at last, the spleen was found to weigh four pounds, and to be gorged with dark blood: while the liver was pale and exsanguine. Frank gives the history of a patient, who had vomitings of blood, and whose spleen, taken from the body after death, weighed sixteen pounds: the ordinary weight of the spleen in a healthy adult being from eight to ten ounces. In Latour's work on 'Hæmorrhage,' which is remarkable for the number of examples it contains, collected from various sources, and amounting to nearly a thousand, several instances are detailed of this combination of splenic enlargement with hæmatemesis. One of these occurred in the person of a friend of his, who had been living in a malarious district, and who had laboured for nearly two years under obstinate intermittent fever. This was followed by an immense enlargement of the spleen—a huge *ague-cake*—which came to occupy almost the whole of the abdomen. Latour's experience enabled him to predict that hæmatemesis would probably supervene upon this condition of the spleen; and, accordingly, one night he was called in a hurry to his friend, and found that he had vomited an enormous quantity of clotted blood. A great deal passed away through the bowels also. The hæmorrhage recurred from time to time, till in the course of a month the spleen was so far reduced in bulk, that it could no longer be felt in the belly: and the patient lived and enjoyed good health for twenty-five years afterwards.

It is necessary, therefore, in marking the connexion which frequently subsists between hæmatemesis and enlargement of the spleen, to guard ourselves against concluding that these two circumstances hold always the relation of cause and effect. In many such cases, probably in most of them, they are simply concurrent effects of one common cause; and that cause is chiefly to be sought in such morbid conditions of the liver—or of other parts within the abdomen—as are competent to produce a considerable impediment to the free transmission of blood through the system of the vena portæ.

When gastric hæmorrhage results from hepatic obstruction, there is almost always *intestinal* hæmorrhage also. At any rate, there are almost always black alvine evacuations, like tar or dark paint. This form of disease has therefore been called *melæna*. The ancients supposed that the unnatural stools consisted of black *bile*.

Hæmorrhage from the stomach, independent of *disease* in that or in any other part, sometimes happens in the advanced periods of utero-gestation. Yet, though it does not result in these cases from disease, it is difficult to class it among idiopathic hæmorrhages. The want of periodical recurrence, and the absence of the hæmorrhage during the *earlier* months of pregnancy, are circumstances which sufficiently refute the old notion, that this form of hæmatemesis depends also upon the suspension of the catamenia. It is caused, no doubt, by the pressure of the gravid uterus, which impedes mechanically the venous circulation of the abdomen.

5. Gastric hæmorrhage, resulting from *changes in the blood* itself, occurs in sea-scurvy, in purpura hæmorrhagica, and in the yellow fever. Being merely a symptom in these cases, it requires no separate consideration here.

When a *large* quantity of blood is poured into the stomach, whatever may have been its source, it appears to have a nauseating and emetic effect. At least the blood ejected in hæmatemesis is almost always considerable in amount. The vomiting may, for aught I know, be dependent on the mere distention of the stomach, which appears to be tolerant of the presence of the blood up to a certain point, but no further. A small quantity may doubtless pass, all of it, onwards through the pylorus, after undergoing, more or less completely, the process of digestion in the stomach; and a *portion* of the blood pursues that course in most instances. But when it is vomited, it comes up in large quantities, usually of a dark colour, and more or less coagulated. Sometimes the coagula have evidently been moulded in the stomach; and sometimes clots are thrown up, partially deprived of the colouring matter of the blood, and resembling the fibrinous polypi so often met with in the cavities of the heart. Of course the degree of the coagulation of the blood, and of its separation into serum and crassamentum, will depend upon the time that it remains in the stomach; and this again would seem to bear a proportion to the rate of its effusion.

The blood that is vomited is almost always of a dark colour; while that which is coughed up is most frequently florid and bright. Why is this? We are told that the blood which comes from the lungs is rendered florid by the admixture of atmospheric air. But this is not the whole of the matter. Neither can we say that the dark hue of the blood ejected in hæmatemesis is always, or solely, due to some morbid alteration effected in that fluid while yet circulating in its proper vessels. There is another cause, which till of late years, was much overlooked, but which frequently changes the colour and appearance of the blood *after* it has been extravasated into the stomach; and that in so great a degree as sometimes to render doubtful, or to disguise altogether, the real nature of the fluid vomited. I mean the chemical agency of the gastric acid. The effect of acids in blackening the blood out of the body is well known; and it is somewhat singular that the ascertained existence of an acid secretion in the stomach, varying in quantity at different times and under different circumstances, was not sooner applied in explanation of the dark colour of the blood, and its occasional blackness, when vomited. The degree of blackness will be in proportion to the relative quantity of acid which it meets with in the stomach, and the intimacy of the admixture. Sometimes the blood is clotted and not very much altered in colour; sometimes it is grumous, brown, of a chocolate tint, or like coffee-grounds. This generally denotes the existence of *organic disease*; and the appearance of the blood is probably modified in some degree by the morbid process that leads to its effusion. There is good reason for believing that in the *black vomit* of the yellow fever, the colour of the blood undergoes alteration, even while it is yet circulating through the blood-vessels: but that the black appearance of the matter vomited is in great part owing to the chemical action of the gastric acid, may be inferred from the fact, that the fluid so discharged is always (so I am informed) intensely acid. Andral has described an effusion of black liquid into the stomach, as an example of *melanosis*. He states at the same time that an accurate analysis of the liquid showed its composition to be very nearly the same with that of the blood. May we not suspect that this inky fluid really consisted of blood that had been blackened, subsequently to its extravasation, by the acid with which it mixed in the stomach? Upon the same principle may be explained the dark brown, or almost black, colour of the spots which are sometimes seen (I presume when there has been a great superabundance of acid) in the substance of the mucous membrane of the stomach, or even beneath it; and which also have been set down as melanotic. They are so like, in all circumstances, except in the single particular of colour, to the crimson spots which are obviously formed by minute extravasations of blood in the same parts, that we can scarcely refer them to any other source. The slate-coloured patches, which I spoke of yesterday as being vestiges of chronic gastritis, depend likewise upon the blackening effect of the gastric acid upon the congested surface. We have the same dark colour of the effused blood, in many cases, when it is poured out in the intestines. Here, of course, its colour is not referable to the gastric juice; but it is blackened by some of the intestinal gases; probably by the sulphuretted hydrogen for example, or by the carbonic acid that enters into their composition.

There can be no doubt that this gastric acid, when intense in strength, or copious in quantity, is capable of changing the colour of the blood, after death, even while it is contained in the submucous blood-vessels. In these cases it must be conveyed to the blood by imbibition. And the very same thing takes place when strong acids are introduced into the stomach from without. When, for instance, the sulphuric acid, or what is perhaps more to our present purpose, the vegetable oxalic acid, has been taken as a poison, it has the effect of blackening, and, as it were charring the blood, with which the membrane becomes loaded in consequence of the irritation produced by the poison. It does this when no destruction of the mucous membrane has been produced.

It is but justice to observe, that the credit of having been the first to perceive, and to explain, this cause of the blackened state of the blood, while yet remaining in its proper vessels, is due to Sir Robert Carswell.

When blood is ejected through the œsophagus and mouth, we have demonstrative evidence of the existence of *hœmorrhage*; and the *diagnosis* of *hœmatemesis* may appear to be so simple as to admit of neither mistake nor doubt. The diagnosis of *hœmorrhage from the stomach*, however, is really oftentimes difficult and obscure, and to be established by presumptive evidence alone.

In the first place, bleeding may take place from the mucous membrane of the stomach, and no hæmatemesis ensue, especially when the blood is poured forth in small quantities, and slowly. In these cases the blood becomes visible only in the stools, where it may not be looked for, and where, if seen, it may not always be recognised, in consequence of the changes which it has undergone during its passage through the intestinal canal. And even supposing that its presence is detected in the alvine evacuations, it will remain uncertain in what part of that long canal it was effused. The hæmorrhage may even be profuse, and the patient may die, without *any* escape of the blood externally. There is a case related by Frank, in which death took place from hæmorrhage of the stomach without hæmatemesis; and both the stomach and the intestines were found distended by an enormous coagulum of blood which had assumed their form.

Even when the blood is ejected by the mouth, the exercise of some care and sagacity is occasionally, though not always, required, in order to determine the part from which it was originally poured out.

Thus blood may be swallowed, and afterwards vomited: and so we may have hæmatemesis without hæmorrhage from the stomach; just as we may have hæmorrhage from the stomach without hæmatemesis. There are cases of slow bleeding from the lungs, the fauces, the mouth, or the nasal cavities, where the blood, collecting in the pharynx, provokes, from time to time, an instinctive and involuntary act of deglutition; and thus is gradually accumulated in the stomach up to that point at which the organ becomes impatient of its contents, and ejects them by vomiting. This is very apt to happen during sleep, and especially to young children: and as the blood, *when vomited*, is coagulated, and in considerable quantity, it is scarcely possible to conclude, from its mere appearance, that it has proceeded from any other source than the stomach itself. If, however, we mistake such cases, our error is likely to produce much needless alarm, and to lead us to unnecessary activity in treating them. We are assisted towards forming a right judgment (when our attention happens to be directed to this source of fallacy), partly by the general history and symptoms, and partly by an examination of the mouth, fauces, and nostrils, to ascertain whether any coagula, or other marks of hæmorrhage, are visible on the mucous membrane belonging to those parts.

But blood may be swallowed knowingly and purposely, by impostors, and afterwards vomited. Hæmatemesis is one of the complaints which have frequently been feigned; either for the sake of avoiding some imminent punishment, or distasteful service; or with the view of exciting compassion, and of profit-ing by the contributions of the charitable and the credulous; or sometimes from a kind of wilful perversity, akin to insanity. In treatises on forensic medicine, you will generally find reference made to an instance of this kind recorded by Sauvages, in his 'Nosology.' A young girl, who was anxious at all hazards to escape the constraints of a convent, pretended that she was suffering from violent hæmatemesis. In fact she did, for several days in succession, vomit large quantities of blood in the presence of the physician who had been summoned to her assistance. It was afterwards discovered that on each of those days she had swallowed blood which had been secretly conveyed to her from the neighbouring shambles. A case of precisely the same kind occurred (as I was informed by a gentleman who witnessed it) in the Bristol Infirmary some years ago. A girl had long been a patient there, labouring (as was supposed) under hæmatemesis; but it was at length discovered that she was a malingerer. She was in the habit of assisting the nurses in their work; and this afforded her opportunities—of which she availed herself—of drinking the blood which had been drawn from the veins of other patients: and this blood she afterwards vomited.

And even where no fraud is attempted, nor any blood swallowed, it occasionally becomes a nice matter to determine the origin of the hæmorrhage, when blood is ejected in large quantities from the mouth: to decide, namely, whether the blood has come originally from the *lungs* or from the *stomach*. In copious hæmoptysis, the blood issues from the mouth in gushes, as it does in hæmatemesis; and the reflux of the blood into the pharynx, the tickling sensation it there produces, and the cough (which we know, even when the expectoration is not of blood, frequently excites retching); these causes, acting singly, or together, occasion sometimes a convulsive contraction of the muscles of the thorax, which *looks like* the effort of vomiting: and they often indeed give rise to actual vomiting. On the other hand in sudden and profuse hæmatemesis, the irritation caused by the blood as it passes over the upper part of the larynx, is apt to provoke a paroxysm of choking cough.

When I was speaking, some lectures back, of hæmoptysis, I promised that I would point out the means of distinguishing it from hæmatemesis, when I came to the consideration of the latter complaint. I have now therefore to redeem my promise.

However equivocal certain cases may be at first sight, we may generally guide ourselves to a correct decision by a careful investigation of the circumstances that *precede, accompany*, and *follow* the hæmorrhage. *Vomiting* of blood is commonly preceded by a sensation of weight and uneasiness in the epigastrium; and by nausea. Hæmatemesis is also, more frequently than hæmoptysis, ushered in by paleness of the face, dimness of vision, and an approach to syncope, or even actual fainting. These symptoms are not to be regarded (I apprehend) as premonitory of the *hæmorrhage*, although they have been so considered by some; they are rather a sign that it has already taken place; and yet they *are* preliminary of the *hæmatemesis.* Occurring before the blood comes up, they cannot be ascribed to alarm at the *sight* of it. On the other hand, *hæmoptysis* is wont to be announced by dyspnœa, cough, tickling in the throat, and a sensation as if of *bubbling* within the thorax. Most commonly too, before the expulsion of much blood from the lungs, some sputa are *coughed* up, composed more or less of that fluid. The symptoms that usually *succeed* the hæmorrhage in either case, afford equally valuable assistance to our judgment, in cases that might otherwise be doubtful. Generally copious hæmoptysis goes on, in a succession of mouthfuls, for some time; whereas there is, mostly, only one access of full vomiting. At any rate, at the close of abundant pulmonary hæmorrhage, the patient manifestly *coughs* up, and expectorates, smaller quantities of blood; while we usually may observe that, a few hours after hæmatemesis has occurred, slight griping pains come on in the abdomen, and a portion of blood is got rid of from the bowels. The blood in hæmoptysis shows an alkaline quality when tested by litmus paper: in hæmatemesis it is sometimes acid from admixture with the gastric juice.

Other questions, often of much importance in regard to the ultimate diagnosis, when the blood is traceable with certainty to the stomach, are, whether it be idiopathic, if, indeed, it *ever* be so: whether it be supplemental of some other discharge: whether it depend on disease of the stomach itself; of one, or more, of the contiguous viscera; or of the system at large. Certainly, in a very great majority of cases, gastric hæmorrhage is symptomatic; and the nature and seat of the disease of which the bleeding is a symptom may in many instances be determined without much difficulty. That which depends upon *incipient* cancer of the stomach, while it is by no means of rare occurrence, is also (I think) more frequently than other forms of hæmorrhage from that organ, obscure. It must be obvious to you, and therefore I need not dwell upon this part of the subject, that a little attention to the symptoms and past history of the patient will usually suffice to elucidate the nature of the case, where hæmatemesis supervenes immediately upon the introduction of corrosive poisons, or within a certain interval after they have been swallowed: where it depends upon the bursting of a large aneurism: where it breaks forth among other symptoms of scurvy or purpura: where it is the result of an *advanced* stage of cancer of the stomach: where it accompanies organic disease of the liver, spleen, or heart: where it occurs as a symptom of yellow fever: where it takes the place of suppressed or imperfect menstruation: or where it is occasioned by the pressure of the gravid uterus. In all these cases, there is, ordinarily, no room for mistaking the one disease for the other; or for regarding the hæmorrhage as idiopathic.

With respect to the *treatment* that should be adopted in cases of hæmorrhage from the stomach, it must be apparent, from what has just been said of the many different morbid conditions upon which it may depend, or with which it may be essentially connected, that remedies are, in most cases, rather to be directed against the disease of which the hæmatemesis is a symptom, than against that symptom itself. But sometimes we are obliged to treat the symptom: either because we are not certain of the exact nature of its cause; or because the condition out of which it springs is not within our reach.

Cases of *melæna* (I have told you what is meant by that term) require hard purging: and many patients recover thoroughly under that rough mode of treatment. You may prescribe five grains of calomel every night, and a black dose every morning, till the stools lose their pitchy colour. Do not be afraid of purging your patients in such cases. If they are curable at all, that is the way to cure them. I have pursued that plan with perfect success, even with patients whom the previous hæmorrhages had blanched, and whose pulse was feeble and irregular. You may sustain them, at the same time, by a full allowance of nourishing broths. The portal system is drained and unburdened by this active depletion. And if there be no irremediable change of texture in the liver, the recurrence of the hæmorrhage may often, by a proper regulation of the habits and diet, be averted. The ancients had learned by observation, the efficacy of treatment of this kind; but they used a different form of medicine, and purged away the *atra bilis* with hellebore. Remember that this active treatment befits those cases only of melæna which depend upon passive or mechanical engorgement of the portal system of blood-vessels, whether such engorgement results from some hepatic fault, or (as it may) from valvular disease of the heart. For such conditions styptic remedies would clearly be worse than useless.

On the other hand, hard purging would be simply perilous in those conditions for which styptics are really adapted; those cases, especially (could we but surely distinguish them), in which the hæmorrhage proceeds from a bleeding vessel. This is indeed the mode whereby we often succeed in stanching external hæmorrhages; namely, by applying astringents to the very part. Dr. Budd very properly lays great stress upon *prolonged fasting* in such cases. Similar means may be employed when hæmatemesis, of a purely passive character, depends upon some modification of the circulating blood. There is one remedy which is thought to have a sort of specific effect upon hæmorrhages of the gastro-intestinal canal: I mean the oil of turpentine, given in small doses; from twenty minims to half a drachm every four or six hours. I cannot say that I have had much experience of it. Of course the patient must be kept cool and quiet; whatever he drinks he should drink cold; even ice is often both grateful and effectual. If ordinary measures fail, recourse may be had to the acetate of lead; or even to the quack medicine, Ruspini's styptic. Not that I think you will often find the latter expedient successful, when more rational treatment has failed; but in obstinate and dangerous cases it ought to be tried. The tannic acid would however be more legitimate. The perchloride of iron is serviceable for its styptic qualities. If, with the hæmatemesis, there be any fever, or if, with or without much fever, there be tenderness at the epigastrium, leeches and fomentations should be applied. In cases where the catamenia desert their natural channel, and seek an outlet through the mucous membrane of the stomach, it will be well, while means are taken to discourage the hæmatemesis, as iced drinks and so forth, to endeavour to solicit the discharge towards its right direction. And we often succeed in this object, by placing leeches upon the groins of these patients immediately before the period when the vicarious menstruation is expected; and by putting their feet at the same time into hot water; or even by laying the patients in a warm hip bath.

LECTURE LXIV.

Dyspepsia. Physiology of digestion. Symptoms of Dyspepsia. Treatment and Prevention, Dietetic and Medicinal.

It is my intention to appropriate this evening's lecture to a cursory account of *dyspepsia*; by which I mean some evident derangement in the natural process of digesting and assimilating our food; and more especially, a faulty performance of the functions of the *stomach*. Indigestion is the prevailing malady of civilised life. We are more often consulted about the disorders that belong to eating and drinking, than perhaps about any others: and I know of no medical topic concerning which there is afloat, both within and beyond the profession, so much ignorant dogmatism and quackery.

Cullen, in his definition of dyspepsia, enumerates the various symptoms, by the occurrence of more or fewer of which, that complaint is most commonly manifested. 'Anorexia, nausea, vomitus, inflatio, ructus, ruminatio, cardialgia, gastrodynia:—pauciora saltem vel plura horum simul concurrentia, plerumque cum alvo adstrictâ, et sine alio vel ventriculi ipsius, vel aliarum partium, morbo.'

The variety in the actual presence and combination of these symptoms is very great: and any attempt to give a perfect or complete history of dyspepsia in these lectures is quite out of the question. But I will endeavour to draw such a general outline of the disorder as may assist and direct your observation of it hereafter.

I shall first take a brief view of the *pathology* of indigestion, so far as it is understood: and to make this intelligible, it will be necessary to interweave something of the *physiology* of the subject. To these preliminary considerations, I shall add a short comment upon the several symptoms of dyspepsia, enumerated in Cullen's definition: and lastly, I shall state what I know respecting the means of curing, and of preventing, this familiar disorder.

The conditions of healthy digestion are these: that the food should be masticated, mixed with saliva, and swallowed into the stomach: that in the stomach it should be reduced to a semi-fluid consistence and converted into a uniform pulp, called chyme: that the chyme should be transmitted through the pylorus into the duodenum, and there mixed with the bile, the pancreatic secretion, and the intestinal mucus; in consequence, as it would seem, of which admixture, the whole is separated into two parts, viz. the chyle or the nutritive portion of the food, now in a fit state to be taken up by the veins, or by the lacteals which open upon the mucous surface of the intestines, and to be carried by them into the blood; and the excrementitious portion, which at length is conveyed out of the body.

The food is dissolved and transformed, in the stomach, by the chemical agency of the *gastric juice.* This is a secretion peculiar to the stomach. All that need be stated of it here—almost all indeed that is known—is, first, that it oozes forth in minute drops from the mucous surface, but only when food (or some solid substance) is present in the stomach; secondly, that it is always *acid*; and thirdly, that under various disturbing influences and conditions, it is liable to be excessive, or else deficient, in quantity. To its acid, when combined with a peculiar ferment which is always present in the mucous membrane of the healthy stomach and which has been named *pepsin,* it appears to owe its solvent power.

The food, having arrived in the stomach, is moved about, by a sort of churning or revolving movement, and mixed with the gastric juice, and gradually changed into chyme: which also is acid. Finally, the chyme is propelled by degrees into the duodenum by the pressure of the transverse band of muscular fibres which embraces the pyloric extremity of the stomach. The time in which the whole operation is completed varies from two to four or five hours.

Liquids introduced into the stomach disappear much more speedily; either by direct absorption, or through the pylorus.

All this we know, not from mere speculation on the anatomy and usages of the organ, but from actual observation. An American physician had, for several months in succession, the singular privilege of looking, whenever he pleased, into a healthy human stomach, and of watching its condition, its movements, and its contents, during the process of digestion. A young Canadian had a portion of the skin, muscles, and ribs of the left side of his body blown away in a gun-shot wound, which laid open his stomach also. He recovered from this frightful injury with a permanent aperture in the side, communicating directly with the stomach. Through this loop-hole

Dr. Beaumont was allowed to introduce various articles of food: and to withdraw from time to time the gastric secretions; and the aliment, in the different stages of its digestion. He has published a very interesting account of these experiments, which have set at rest some points in the physiology of the stomach that were previously uncertain. I shall embody his deductions in what I have further to say on the subject.

In order that digestion may be perfect and easy, it is requisite that the food be in a state of minute division. This object is attained by *mastication.* In like manner the chemist first *triturates* a solid, when he desires to facilitate its solution in the proper menstruum. A weak dyspeptic stomach acts slowly, or not at all, on solid lumps and tough masses of food. The delayed morsels undergo spontaneous changes, promoted by the mere warmth and moisture of the stomach: gases are extricated: acids are formed: perhaps the half-digested mass is at length expelled by vomiting; or it passes undissolved into the duodenum, and becomes a source of irritation and disturbance during the whole of its journey through the intestines. Here, then, we have one common cause of dyspepsia; and an easy and obvious preventive. Dyspeptic persons should not eat in a hurry, as busy men, and studious and solitary men, are apt to eat. They are to be cautioned against *bolting* their food: it must be well ground in the mill that nature has provided for that purpose. I am not at all sure that the increased longevity of modern generations is not, in some degree, attributable to the capability of chewing their food which the skill of the dentist prolongs to persons far advanced in life.

There are certain things upon which the gastric juice has no power. The green colouring matter of certain vegetables; the husks of seeds; the rinds of many fruits. You may perhaps have observed that dried currants and the pips of apples, swallowed entire, reappear, unchanged, among the egesta. Whatever passes the stomach untouched by the gastric liquor, passes undissolved through the whole of the alimentary canal: provoking disorder sometimes in its transit; forming sometimes a nucleus for intestinal concretions. These substances are therefore unfit for a weak stomach. When the digestive powers are active, and the movements of the bowels slow, they may perhaps occasionally be even useful. Thus brown bread—i.e. the indigestible bran, or tegument of the kernel of wheat—stimulates the peristaltic motions of the intestines, and averts.

in certain persons, the necessity of more direct purgatives. Unbruised mustard-seed, once so much in vogue, owed much of whatever virtue it possessed to this principle. But if these intractable substances fail to excite the proper action of the bowels, they are apt to accumulate, and to lay the foundation of serious disease.

Indigestible matters, to which the pylorus refuses a passage, may remain in the stomach, and disturb its functions, for days, or even sometimes for weeks, together. If we could ascertain their presence, an emetic would be the remedy. And sooner or later vomiting is set up, and the offending substance is expelled. I lately saw a mass of hard curd—a small cream cheese in respect of consistence—which was thrown up after several days of severe gastric pain and disorder. The relief was immediate and complete. The patient had been taking large quantities of cream with his tea and coffee. In another person, a similar fit of indigestion terminated in the ejection of a mass of snuff. This is no unusual source of derangement of the stomach among those who use lavishly that nasty luxury.

The essential change which the chyme undergoes after leaving the stomach, appears to consist in its separation into two parts: namely, into chyle, which is taken up by the lacteals; and into excrement, which is discharged from the body. Any undissolved portions of the food become attached to this last part. The chief function or agency of the pancreatic liquor is believed to be that of facilitating the absorption of fatty matter into the lacteals by reducing it to the state of an emulsion; and of starch by converting it into sugar. With regard to the bile our knowledge is somewhat more definite. The acid developed in the stomach combines in the duodenum with the alkali of the bile, and is more or less neutralised. Dr. Prout conjectures that in a healthy state of the organs it is entirely neutralised. Bile has been found also to have an antiseptic power, even in the living body, and is therefore preventive of putrefactive fermentation and flatulence. It is, moreover, the natural stimulus of the intestines: when its secretion is stopped, or its passage into the duodenum prevented, digestion and assimilation may go on, but the bowels are usually sluggish. This hepatic secretion has doubtless other important uses; but with these we are not at present concerned. It is pretty evident that the state of the biliary functions can have no direct influence in the production of mere indigestion. When the constituents of the bile are imperfectly eliminated from the blood, various parts of the body may suffer detriment. And when the functions of the stomach and the functions of the liver are both disordered, it *may be* that the former organ sympathises indirectly with the morbid state of the latter: or it *may be* that one and the same cause operates in producing the derangement of both organs.

Let us now review the symptoms of dyspepsia which are mentioned in Cullen's definition. The first of these is *anorexia*: want of the natural appetite. Sometimes this is almost the only symptom observable. The patient is warned, by loss of appetite, not to take too much food; he refrains instinctively from certain kinds of food; or he feels perhaps absolute repugnance and disgust at the very thought of eating. Various have been the speculations respecting the immediate cause of hunger. It has been ascribed to the action of the gastric juice upon the surface of the empty stomach. But during health the gastric juice is never present in an otherwise empty stomach. Neither can the appetite depend upon contraction of the muscular fibres of the stomach; for the empty stomach, during health, is always contracted upon itself. No doubt the sensation of hunger, like all other sensations, arises from some particular condition of the *nerves* of the part. It returns periodically, acknowledging in this respect the influence of habit. It is sensibly affected by agencies which operate upon and through the nervous system. The receipt of a piece of bad news will destroy, in a moment, the keenest appetite.

Sometimes there is no anorexia. The appetite may even be morbidly craving and ravenous; or capricious and uncertain.

When defect of appetite is the only symptom, it may be remedied, often, by the employment of bitters or of the mineral acids, taken twice or thrice daily, for some time together. It would be out of place for me to speak in detail of particular medicines of this kind: it is enough if I indicate quina, columbo, gentian, quassia; the dilute sulphuric and nitric acids; or a mixture of the nitric and muriatic.

Nausea—vomitus. These are, in some instances, the most distressing results and signs of the dyspepsia. Sometimes nausea comes on soon after the food is swallowed. Sometimes there is no nausea; but after the lapse of a certain period, an hour or two generally, the food is rejected by vomiting. The matters thus thrown up are most frequently sour. Not seldom they are mixed also with bile, especially if the retching have been violent, or long continued; and then the patient is apt to ascribe the whole of his complaint to 'an overflow of bile,' although in fact the secretions of the liver have nothing whatever to do with it; the appearance of bile, in the fluids ejected from the stomach, proceeding from an inverted action of the duodenum. The effort of vomiting, however induced, will, if often repeated, be attended with the expulsion of yellow bile. I have more than once referred you, for an illustration of this fact, to the phenomena of sea-sickness. In the strange case which I lately quoted from Dr. Murchison, yellow bile was *seen* during the act of vomiting to be squirted from the contracting pyloric end of the girl's stomach, as if from a syringe. The fallacy I now point out has been one cause of the notion that is prevalent among patients, and the public—and not unfrequently perhaps among practitioners—that indigestion very commonly depends upon a disordered state of the biliary organs.

The vomiting which occurs in dyspepsia is often connected with a morbid irritability of the stomach; and it is sometimes a very troublesome symptom to treat. The carbonic acid gas has certainly a marked effect, in many cases, in allaying it. We give it, as you know, in the effervescing saline draught, made with the carbonate of potash, or of soda, and lemon-juice. Sometimes the mineral acids answer better. Sometimes, on the other hand, alkalies—the liquor potassæ for example, or lime-water—are more effectual. In these latter cases we may presume that there is a morbid acidity of the stomach. A few drops of chloroform swallowed in water sometimes answer well. Small doses of opium are occasionally successful when other means fail. Opiates thrown into the rectum—opium plasters to the epigastrium—blisters to the same part: these are measures which you will sometimes have to try one after another. Nux vomica, or its active principle, given in minute doses, is another of the useful remedies for dyspepsia and vomiting. There are two special remedies which have been greatly extolled for their virtue in abating sickness: the hydrocyanic acid is one of them; creasote is the other. The hydrocyanic acid I have found exceedingly useful in obstinate cases. It may be given alone—or mixed with the effervescing draught—or combined with a few grains of the sesquicarbonate of soda. The creasote has disappointed me oftener than it has answered my hopes from it. Yet it has a decided influence in checking some forms of nausea; and it is the more likely to succeed, in proportion as the condition of the stomach is remote from inflammation.

But after all, the grand principle on which to treat chronic vomiting—not dependent upon disease in other parts, as the head, the kidney, or the uterus—is that laid down by Dr. William Hunter; of reducing the *quantity* of food to that amount, whatever it may be, which the stomach is able and willing to retain, and making its *quality* as bland and nutritious as possible. The most satisfactory case which I have had to treat upon this principle occurred some years ago, in the person of one of my hospital patients. She was brought out of Kent by her father. She had been under the care of several medical men, one of whom had been a pupil at the hospital, and recommended her as a proper patient for admission there. Her age was sixteen. She and her father both agreed in the same story; viz. that she constantly vomited her meals; the food generally coming up again immediately after it was swallowed, and never remaining longer in her stomach than ten minutes. The vomiting was described as being easy; and was neither preceded nor accompanied by nausea.

She had been ill for four years: ever since a severe attack of scarlet fever. At first she vomited her meals now and then—three or four times a week—but the vomiting gradually became more and more frequent; and at the time of her admission she had vomited after *every* meal, for three months in succession. She had grown considerably in the four years; and was tolerably plump; and looked healthy; and the catamenia had begun to appear, though scantily, in the same period: but they had been altogether suspended for a year.

It was clear that a good deal of her food must have remained: and, bearing William Hunter's case in mind, I directed that she should have a very small quantity of roast meat for dinner, and a coffee cup of milk occasionally during the day; and no other food. I prescribed also some pills, consisting of aloes and soap, to act moderately on the bowels. I expected to have been obliged still further to limit her food: but she never vomited again, from that time. This distressful and protracted disorder, after long and fruitless treatment previously, yielded thus at once and easily to very simple means.

Inflatio—ructus. Flatulence, and belching. The gas that produces these symptoms is sometimes extricated from undigested food detained in the stomach, and in a state of fermentation, or of simple putrefactive change; sometimes secreted, it has been thought, by the stomach itself; for the flatulence comes on when the stomach is empty of food. It is apt to arise, in dyspeptic persons, if a meal happen to be delayed beyond the accustomed hour. Dr. Wilson Fox assures us, however, that there is an entire lack of experimental evidence in favour of the occurrence of any such secretion. Patients complain grievously of these symptoms, and accuse the 'wind in their stomachs' as being at once the essence and the cause of all their discomforts. They ask for medicines to get rid of the wind; and its escape may indeed be promoted by warm aromatics, and carminatives, as they are called; the relief thus afforded to the distended stomach being so sudden, and for the time so complete, that the sufferer ascribes to the medicine *vim carminis*, the power of a charm. One of the most effectual and popular of these carminatives is peppermint-water. A due regulation of the periods for taking food will often suffice to obviate the flatulence that belongs to emptiness. That which follows eating may, in many cases, be prevented, by swallowing, immediately before the meal, five or six grains of the extract of rhubarb, with or without a grain of cayenne pepper: or still more certainly, according to my experience, by the nitro-muriatic acid, taken in small doses, a quarter of an hour, or thereabouts, before the food. Pepsine is a favourite remedy with many when digestion appears to be slow and difficult. Of its virtue I am not able to say anything definite from my own experience. When the belching is attended with the odour and flavour of rotten eggs—in other words when the gas evolved is sulphuretted hydrogen—it results from decomposition of the contents of the stomach. Flatulence from this cause may sometimes be prevented or subdued by charcoal, or by creasote, which, like the gastric juice itself, are powerful antiseptics. But this condition is commonly an accidental and transitory condition, and its best cure is an emetic. If the ascending wind brings into the throat and mouth a portion of the solid contents of the stomach, the patient is said to *ruminate*. The regurgitated matters are often intensely acid; and then an alkali may remedy the existing flatulence; a teaspoonful of *sal volatile*, for example; or ten grains of the carbonate of potash.

Indigestion is, in many instances, attended with scarcely any *pain*; while in others the pain is very tormenting. Cullen speaks of it under the terms *cardialgia*: and *gastrodynia*. Cardialgia is that less violent and more permanent uneasiness which in popular language is called *heart-burn*: a sense of heat in the epigastrium or behind the sternum, attended sometimes with acid eructations. Gastrodynia is that more severe, and usually more transient pain, which is commonly denominated *spasm* or *cramp* of the stomach.

Dr. Abercrombie has some useful practical observations with respect to pain of the stomach. He speaks of it as occurring under four different forms; and I am able to bear witness to the reality of the distinctions that he has drawn. A still nicer discrimination of the varieties of stomach distress has been made by my friend Dr. Budd. In the first place, some persons suffer pain, occasionally, when the stomach is empty, even when there is no flatulence; and they are comforted and relieved by taking food. A clergyman of my acquaintance, who used to be much harassed by gastric pain of this kind recurring several times daily, and who had tried a round of drugs in vain, found by accident that it was appeased at once upon his eating a small biscuit. He therefore carries about with him always a supply of this easy remedy. It is reasonable to suppose that the pain in such cases depends upon some degree of acrimony of the fluids of the stomach itself. It often yields readily to alkalies, or to absorbent medicines. A tea-spoonful of the aromatic spirit of ammonia in a wine-glass of camphor julep, or half a drachm of magnesia, will still the whole uneasiness sometimes in a moment, as if by magic.

Dr. Budd notes these further characteristics of this sort of pain; that it is accompanied by slowness of the pulse, and coldness of the surface of the body, that the recumbent posture helps it away, and that the hydrocyanic acid is a very successful remedy for it.

A second form of pain in the stomach is when it occurs *immediately* after taking food, and continues during the whole process of digestion, or until vomiting ensues, which gives instant ease. In such cases we have reason to suspect the existence of chronic inflammation, or of ulceration, or of some undue sensibility of the mucous membrane of the stomach. The suitable remedies are such as I spoke of in the last lecture. I might have mentioned a form of medicine which Dr. James Johnson found especially serviceable against this morbid sensibility; I mean the nitrate of silver, in small doses.

When uneasiness rather than pain occurs presently after a meal, with a sensation of weight at the pit of the stomach, and indisposition to bodily or mental exertion, we may infer that the work of digestion is slow and difficult, in consequence of a too scanty secretion of the gastric juice. For this form of dyspepsia those remedies are the most proper which are believed to promote the secretions of the stomach; ipecacuanha in small doses, or rhubarb, before the meal; or condiments taken with it, such as salt, mustard, cayenne pepper.

In a third species of painful disorder of the stomach the pain does not begin till from two to four hours after a meal, but continues for several hours. This is a very common form of complaint. Dr. Abercrombie is of opinion that the pain is seated in the duodenum, and connected with inflammatory action, or with morbid sensitiveness of the mucous lining of that bowel. He says it is frequently accompanied by pain and tenderness of the right hypochondrium; and that the liver is often blamed when it really is not in fault. The last remark I can well believe; but I am not so easily persuaded that the pain is duodenal. It probably depends upon acidity in the primæ viæ. It has been ascertained, I believe, by several chemists that the essential acid of the gastric juice is the hydrochloric. Dr. Prout holds that the source of this hydrochloric acid is the common salt which exists in the blood, and that the decomposition of this salt is owing to the immediate agency of some modification of electricity; and he conceives that the principal digestive organs represent a kind of galvanic apparatus, of which the mucous membrane of the stomach and intestinal canal may be considered as the acid or positive pole, while the hepatic system is the alkaline or negative pole. However this may be, it is certain that the hydrochloric acid contained in the stomach is often in excess: other acids are also found there—the acetic, and more especially the lactic; and when the food, now converted into chyme, passes into the duodenum, the remaining superfluous acid teases the stomach. I think this explanation of the cause of the pain is a more probable one than Dr. Abercrombie's, because you may generally mitigate or remove the pain by introducing an alkali into the stomach, whereby the acid is neutralized: even the swallowing a cup of warm tea, by which the acid is diluted or washed away, often stops the pain. And I have, in numerous instances, succeeded in *preventing* the recurrence of this pain by directing the patient to take a small quantity of alkali, in some aromatic water, immediately after his dinner. According to Dr. Abercrombie's theory the pain ought not to be so immediately allayed by these remedies; and, since the food is gradually propelled into the duodenum as it is digested, the pain should begin, I think (supposing him right), earlier than it does. Dr. Abercrombie has found nothing of more general utility in these cases than the sulphate of iron, combined with one grain of aloes, and five grains of aromatic powder, taken three times a day. He praises lime-water also, and small opiates, and a combination of bismuth and rhubarb. Bismuth is believed to restrain undue secretion, and to exercise a sedative influence upon the stomach. Whatever may be its *modus operandi*, I am sure that it is a very effectual remedy for some kinds of gastric distress.

Cases now and then occur in which this pain, succeeding a meal, and the deposit of lithates in the urine, would seem to indicate the propriety of an alkaline treatment, but which really are more benefited by the mineral acids. In such cases the microscope detects oxalate of lime in octohedral crystals mixed with the lithate of ammonia, or of soda.

Pain in the stomach occurs in a fourth form, coming on at uncertain intervals in most violent paroxysms, and properly called *gastrodynia*. It is often accompanied by a sensation of distention, much anxiety, and extreme restlessness. In women it is frequently combined with hysterical symptoms. This form Dr. Abercrombie supposes to depend upon over distention of the stomach; and it may be so; certainly great quantities of air are sometimes extricated; and the pain is not confined to the stomach, but shoots through to the back and between the shoulders. I suspect that the pain is sometimes neuralgic. It is often very intractable: occasionally it yields to carminatives; to a few drops, for instance, of cajeput oil suspended by means of mucilage in some aromatic water. Dr. Abercrombie states that he has observed the most effectual relief in such cases to have been obtained from exciting a brisk action of the bowels by means of a strong purgative enema. He makes this practical remark, which is worth attending to. From the facility with which such affections often yield to the remedy just mentioned, it appears not improbable that the pain may be sometimes situated in the arch of the colon. Wherever its seat may be, I know that it is frequently removed by a mustard poultice laid upon the epigastrium. Opium also is of eminent use in many of these cases; and bismuth; and cordials; and perhaps chloral; but I have seen more rapid and decided relief afforded by the prussic acid than by anything else; and the cure so wrought is often permanent. It does not bring ease in all cases, nor is it a medicine that is any particular favourite of mine; yet its good effect is in some instances so striking, that if this were its only virtue I should esteem the hydrocyanic acid a most valuable remedial agent.

You will meet sometimes with what is called *spasm* of the stomach (and I suppose it is such) in gouty people; who are then said to have gout in the stomach. The pain comes on in sudden and severe paroxysms; and is removable in general by laudanum and stimulants, brandy for example; or by the mustard poultice. On these cases, however, we look with jealousy and apprehension. In some instances the attack is really inflammatory, and would then be aggravated by a stimulant treatment.

There is another modification of uneasiness and disorder of the stomach, of which the distinguishing characteristic is a burning sensation in the epigastrium, followed by the vomiting, or rather the eructation of a thin watery liquid, resembling saliva, sometimes sourish, but usually insipid and tasteless, and often described by the patients themselves as being cold. This is what Cullen calls *pyrosis*, the *water-brash*. It is a disorder much more frequent in the lower ranks of society than in the upper: and among women than among men. It is very common in Scotland, and is there ascribed to the large employment of farinaceous substances as food, and especially of oatmeal. But it is said to be still more prevalent in Lapland: and it is not at all uncommon in Wales, and in various parts of England, where the diet used is chiefly vegetable. Dr. Cullen, who saw a great deal of this disease, says that its paroxysms 'usually come on in the morning and forenoon, when the stomach is empty. The first symptom of it is a pain at the pit of the stomach, with a sense of constriction, as if the stomach were drawn towards the back. The pain is increased by raising the body into an erect posture, and therefore the body is bended forward. The pain is often very severe; and after continuing for some time, it brings on an eructation of a thin watery fluid in considerable quantity.' Such is Cullen's description of pyrosis. He states that the complaint often occurs without other evidence of dyspepsia: but this is not consistent with the experience of subsequent observers. It is a symptom sometimes of organic disease of the stomach. In one remarkable case of pyrosis which I saw, and in which not less than three pints of this thin tasteless liquid was brought up every day, the stomach, after death, was found to all appearance healthy; but it had been pressed upon by an enormous liver. I mention these facts that you may not imagine pyrosis to be always, as Cullen has described it, a substantive and idiopathic malady.

Dr. Budd supposes that the ejected fluid, when insipid and alkaline, comes, not from the stomach itself, but from the salivary and other glands in the mouth and pharynx, and that its secretion is provoked by the uneasy sensations of the stomach. When the fluid has an acid taste, we may be pretty sure that a part of it at least is furnished by the stomach.

When pyrosis is not caused by organic disease in the stomach or in the liver, it will yield in general to opium, and especially to opium in combination with astringents. The *pulvis kino compositus* of the Pharmacopœia is an admirable remedy for it. But we often have to contend with this difficulty, that the bowels, in cases of pyrosis, are apt to be confined, and that the opium tends to aggravate this unnatural condition; so that it becomes necessary to administer some aperient daily while the kino and opium are given: the watery extract of aloes, or the confection of senna, or the compound colocynth pill.

I scarcely need say that when the disorder has arisen under the use of innutritious or unwholesome food, the adoption of a more varied and generous diet, including a sufficient proportion of meat, is essential to the permanent success of any remedy.

One more form of stomach disorder I have still to bring before you. Like the last, it is attended with vomiting and characterized by the nature of the matters vomited. It is one of the acquisitions of modern diagnosis. In the year 1842, Mr. Goodsir, observing signs of fermentation in the fluids cast up from day to day by one of his patients, examined them in the expectation of finding some of the minute algæ which are known to accompany that process. To his surprise, however, a new form of vegetable life presented itself. He discovered multitudes of small flat bodies, having a rectangular outline, and a slightly oblong shape, divided into four similar portions by cross lines, and thus somewhat resembling little packets tied lengthwise and across by a string. These bodies he therefore named *sarcinæ*. Each of the four portions is similarly divided by fainter markings, in the manner of the diagram which I here show you.

Instances of the same kind of disorder were soon after noticed and recorded by Mr. Busk, Dr. Todd, Dr. Bence Jones, Sir Wm. Jenner, and Dr. Budd, who has devoted a lecture to this interesting subject.

The fluids vomited in these cases have a faint acid smell like that of fermenting wort; and it is obvious that they are themselves fermenting. After standing a few hours, they become covered with a thick, brownish, yeast-like froth, and they deposit a brown flaky sediment. In the frothy head, and in the flaky deposit are to be seen great numbers of sarcinæ, together with the torulæ proper to yeast. The fluid itself is always acid; or if not so, it contains no sarcinæ. Sarcinæ appear in the alvine excretions also.

Most generally the vomiting is copious, and sometimes it is enormous in amount, so that the stomach must have been vastly distended. It often takes place in the morning, after a night spent in distress from a sense of burning and distention in the epigastrium, and a feeling of bubbling or fermentation there. These painful sensations are relieved by the emptying of the stomach.

Vomiting of this kind is usually, if not always, indicative of some structural and incurable disease of the stomach; such disease as hinders the onward passage of its contents through the pyloric opening: any narrowing of that orifice, whether from cancer or from any other cause. Dr. Budd believes the disease to consist primarily and essentially in some organic change which prevents the stomach from completely or readily emptying itself, and which causes a secretion from the coats of the stomach capable, when mixed with the food, of undergoing or of exciting a fermentative process; and that the development of the sarcinæ bears to this fermentative process, or to some stage of it, the same relation as the development of torulæ bears to simple alcoholic fermentation.

The evolution of carbonic acid gas, and the formation of acetic acid, attending this process, produce heartburn, flatulence, and painful distentions; symptoms which alkalies may alleviate. But the most appropriate and useful remedies are those which tend to prevent the process of fermentation. Of these, common salt, creasote, and the sulphite of soda have been found the most serviceable. They are all antiseptic. Salt, and creasote in the form of a pill, may be taken with the meals. The sulphite of soda owes its efficacy to its ready decomposition by almost any vegetable acid, this decomposition setting free sulphurous acid, which has great power in preventing the acetous fermentation. The sulphite may be administered soon after the meal, or when the fermenting process is commencing, in doses varying from a quarter of a drachm to a drachm, dissolved in water. This remedy, the most effectual in relieving a disorder which is probably always incurable, was introduced into practice by Sir William Jenner.

Costiveness is a very frequent concomitant of dyspepsia, as Cullen's definition affirms (plerumque cum alvo adstrictâ). And this sluggish state of the bowels often aggravates, if it does not produce, the dyspepsia. At any rate the defective powers and uneasy sensations of the stomach are rectified, in many instances, by measures which promote the regular and healthy evacuation of the intestines. Without professing to go into detail in this matter, I may state that, in our remedial attempts, we should imitate, as nearly as we can, the processes of nature. The mixed contents of the small intestines furnish the natural stimulus of their peristaltic movements; and the excrement excites the larger bowels. When this natural stimulus is insufficient, the want may be supplied by some substance which is involved in the food, and accompanies it in its progress—as the bran of brown bread, already mentioned, or a few grains of rhubarb, or of aloes, swallowed immediately before dinner. In adjusting the proper quantity of the drug the patient must assist the physician. It should be just so much as suffices to effect what nature neglects to do, and no more. With some persons an aperient pill acts more comfortably and opportunely if taken at bed-time. Sometimes diarrhœa is associated with indigestion. This is usually connected with an excess of acid in the *primæ viæ.* The principle of treatment in such cases is obvious. Antacid and astringent remedies are proper; chalk, bismuth, catechu, kino, rhatany, logwood.

It is of no small importance to cultivate sedulously a daily habit, in respect of the *time* of the alvine dejections; and to avoid as much as possible any accidental breach in an already established habit. Indeed, for dyspeptic persons, the same observance of habitual hours should govern the entrance as well as the exit of their food.

There are innumerable sympathies of distant parts with a dyspeptic stomach, in respect of which I can do little more than barely enumerate a few. Thus indigestion is often accompanied by pain in the head, with some confusion of thought: or at all events, with a loss of mental energy and alertness. Together with violent headache there are frequently nausea and vomiting; and the complaint is popularly known by the name of the *sick-headache*; or, in the fashionable jargon of the day, as a *bilious* headache. I must refer you to a paper of Dr. Fothergill's in the sixth volume of the 'Medical Observations and

Inquiries' for a very good account of this troublesome complication.

I shall hereafter have a good deal to say respecting certain morbid conditions of the urine, which take their rise sometimes from faulty digestion in the stomach, sometimes from faulty assimilation of the digested aliment in the more advanced stages of the process of nutrition. These conditions of the urine, indicating grave derangements of the whole system, furnish the characters and the names of several distinct maladies.

I told you, in a former lecture, that certain affections of the viscera of the thorax are liable to be produced by mere indigestion. Palpitations of the heart, irregularities of the pulse, fits of asthma, are no uncommon accompaniments of a disordered stomach. This is partly to be ascribed to that reflex sympathy between the parts concerned, which I have so frequently mentioned; partly to the effect of flatulence, which, by resisting the descent of the diaphragm, impedes the free working of the lungs and of the heart. I told you, at the same time, that these symptoms torment many persons with the belief that they spring from organic disease. This notion is particularly apt to infest the minds of medical students. I suppose most teachers in our profession partake of that sort of experience of which Dr. James Gregory, of Edinburgh, used to speak in his lectures. He said that scarcely a winter passed over in which several of his pupils did not apply to him on account of palpitations supposed by them to depend upon structural disease of the heart: and in no single instance were their apprehensions well founded. They were all cases of mere dyspepsia and hypochondriasis.

Dyspepsia is often connected with phthisis, with leucorrhœa, with amenorrhœa and chlorosis: and some persons imagine that these diseases are *caused* by the dyspepsia. Indigestion may lead indirectly to the development of consumption, by producing debility; but the truer view of the matter seems to be that the dyspepsia is a consequence, rather than an exciting cause, of these complaints. When, for instance, leucorrhœa is cured by topical astringents, as it often may be, the indigestion frequently is cured too.

One of the worst occasional concomitants of dyspepsia is that peculiar state of the mind to which I just now alluded under the term *hypochondriasis*. This is, in truth, a species of insanity: but it is so often connected with disorder of the digestive organs, that Cullen, whose descriptions of disease are admirably clear and true, however faulty many of his theories may be, defines hypochondriasis to be '*Dyspepsia*—cum languore, mæstitiâ, et metu, ex causis non æquis.' In the following short paragraph he completes the picture: 'In certain persons there is a state of mind distinguished by the concurrence of the following circumstances. A languor, listlessness, or want of resolution and activity with respect to all undertakings: a disposition to seriousness, sadness, and timidity: as to all future events, an apprehension of the worst or most unhappy state of them: and therefore, often upon slight grounds, an apprehension of great evil. Such persons are particularly attentive to the state of their own health, to every the smallest change of feeling in their bodies: and from any unusual feeling, perhaps of the slightest kind, they apprehend great danger, and even death itself. In respect to all these feelings and apprehensions, there is commonly the most obstinate belief and persuasion.'

Now when the attention of the hypochondriac is thus morbidly fixed upon the states and sensations of his digestive organs (as it is very apt to be) the patient becomes a plague to his physicians as well as to himself.

There are a few simple rules which ought always to be kept in mind in our *treatment* of dyspepsia; although we can seldom enforce them, as they ought to be enforced, upon our patients. What patients want, in general, is some medicine that will relieve them from their discomfort and uneasy feelings, and allow them, at the same time, to go on in the indulgence of those habits which have generated the discomfort. And such remedies have not yet been discovered.

One great and indispensable principle in the treatment of indigestion, is that of restricting the *quantity* of food taken at any one time. The gastric juice is probably secreted in a tolerably uniform quantity. The muscular contractions of the stomach must needs be impaired or impeded by much distention of that organ. For both these reasons the amount of food introduced into the stomach should be kept within the limits of its capacity and powers; and these limits are transgressed if an uneasy sense of fulness is produced by the meal. The great good which the late Mr. Abernethy unquestionably did to a host of dyspeptic patients, was owing much more, I am persuaded, to the rules of diet, and the restrictions as to quantity, which he laid down, than to his eternal blue pill.

Dr. Beaumont's observations led him to the conclusion that, within certain definite limits, the supply of the gastric menstruum was exactly regulated by the demand for it. So much aliment evoked so much gastric juice. But that the amount of the latter was never greater than the measure of the requirements of the frame: and therefore that whenever the food exceeded that measure, a portion of it remained undissolved, and even disturbed the due digestion of the rest.

Again, as Dr. Abercrombie has well remarked, and as Dr. Beaumont actually saw, various articles of food are soluble in the stomach with various degrees of readiness. Therefore, when the digestion is liable to be easily impaired, it is of great importance, not only to refrain from those substances which are known to be soluble with difficulty, but also to avoid mixing together in the stomach different substances which are of different degrees of solubility. Hence there are two reasons why it is salutary to dine off one dish. 1st, Because we avoid the injurious admixture just adverted to; and 2ndly, because we escape that appetite and desire to eat too large a quantity, which is provoked by new and various flavours.

And another very important principle, greatly insisted on by Mr. Abernethy, is, that the stomach should have *time* to perform one task before another is imposed upon it. He always made his patients (at least he always strongly exhorted them) to interpose not less than six hours between one meal and another. Allowing from three to five hours for the digestion of a meal, and one hour over for the stomach to rest in, Mr. Abernethy's rule seems as much founded in reason as it is justified by experience. But we preach in vain on these topics. Mr. Abernethy was in the habit of saying that no person could be persuaded to pay due attention to his digestive organs, till death, or the dread of death, was staring him in the face. I have now in my mind a family consisting of a mother and three grown-up daughters, who are continually ailing and valetudinary. They profess to have great respect for my professional advice: yet I never can induce them to think that their plan of eating is a bad one. They are not early risers. They get to breakfast about half after ten or eleven. At two they think it absolutely necessary to eat luncheon, which consists of a mutton chop or some hashed meat, with vegetables. Next comes the afternoon *kettle-drum.* At six they dine: and at eight they drink tea: and then they eat no more till the next breakfast. And this is just a picture of the habits of scores of families. They huddle all their food into the stomach, at four or five periods, within seven or eight hours; and leave it idle for sixteen or seventeen.

Dyspeptic patients are very importunate to know *what* they may eat, and (more so still) what they may drink. It is of course impossible to lay down any general rules, which will suit every case. The stomach has its idiosyncrasies. I remember seeing a publication, some years ago, one section of which had this startling title, 'Cases of Poisoning by a Mutton Chop.' Dr. Prout knew a person who could not eat mutton in any form. He was thought to be whimsical, and mutton was frequently served up to him under some other guise, without his knowing it; but it invariably caused violent vomiting and diarrhœa. Yet, for the average of stomachs, mutton is probably the most digestible of all meats. And for the average of stomachs some useful general directions may doubtless be given.

In fevers and inflammatory disorders, experience has taught us to forbid or to limit the use of flesh meat on account of its stimulating qualities. And when the stomach itself is affected with chronic inflammation, or with morbid sensibility, a diet restricted to farinaceous substances, and milk, is sometimes attended with the happiest consequences. I suspect that a false analogy has led some into the mistake of supposing that animal food ought to be refrained from, or taken in a scanty proportion, in merely dyspeptic complaints. Animal food is easier of digestion, in the human stomach, than vegetable food. A much smaller quantity of it is needed. It is not so likely to generate acidity. It is nearer, in its composition, to the textures into which it is to be incorporated by assimilation: or rather it contains precisely the same organic and inorganic substances as the body consists of. There is less of 'conversion' requisite. Indeed we may look upon the appropriation of vegetable matter by granivorous and graminivorous animals, as one stage of the process by which such vegetable matter is prepared for the sustentation of carnivorous animals: even as one great end of vegetable life seems to be that of generating or concocting matter for the nutrition of the former class, out of inorganic materials, not fitted for that purpose. A more elaborate digesting apparatus is provided for the vegetable eaters. Man, indeed, is omnivorous. But his organs of digestion are more like those of the carnivorous than of the granivorous races. And it is notorious that vegetable food, when the stomach is weak, is followed by more flatulence, that is, is digested with more tardiness and difficulty, than animal food. Nevertheless, a mixture of the two, of well-roasted or boiled flesh or fowl, with a moderate portion of thoroughly-cooked vegetables, is better suited, in my opinion, for a feeble stomach, than a rigid adhesion to either kind of aliment singly. Water; earthy and saline substances; organic substances free from nitrogen, as starch, sugar, or oil; organic substances containing nitrogen, as albumen, fibrin, or casein; these all should be present in the daily food in such proportions as will suffice to repair the daily waste of the tissues, and the loss incidental to the process of respiration. After quantity, quality must be regarded. The more soluble in water, and the less irritating the food is, the more suitable is it for the functions of the stomach. All meats that have been hardened by culinary art, or by condiments, should be avoided by him, who, as the vulgar express it, has 'a bad digester;' all cured meats I mean—ham, tongue, sausages, and so forth. Mutton is thought to be more readily digestible than beef. Pork, its lean part at least, is much less so than either. All raw vegetables also must be eschewed; salads, cucumbers, pickles. But if we press our prohibitions much more strictly than this, we incur the risk of fixing the patient's attention too curiously upon his diet, and upon the sensations of his stomach; and of rendering him hypochondriacally alive to the miserable subject of his feeding.

Again, you will be continually asked whether you recommend malt liquor or wine, wine or brandy and water, white wine or port, sherry or madeira. Now it would be very easy to propound some positive rules in this matter, but it would not be so easy to vindicate them. Some allowance must be made, no doubt, for custom. I believe, however, that most dyspeptic persons would be better without any of these drinks. But it is very difficult to persuade them that the habitual use of strong liquors in small quantities can have any injurious effects. 'It is not easy (says Cullen) to engage men to break in upon established habits, or to renounce the pursuits of pleasure; and particularly to persuade men that those practices are truly hurtful, which they have often practised with seeming impunity.' They are too ready to believe that it is unsafe to abandon their accustomed indulgence. A friend of mine, who visited, some years since, many of the American prisons, told me that the health of even the most inveterate spirit-drinker improves, instead of suffering, upon the sudden and total abstinence from spirits, which the regulations of those prisons enforce. There certainly are cases in which the digestion seems to be helped by a moderate quantity of wine, or beer, or spirits; yet no one can say beforehand—at least I cannot—which of them is to be preferred. Upon these points patients should interrogate their own sensations and experience, instead of seeking the oracular counsel of a physician. Drinks which are followed by evident disturbance and discomfort are manifestly unfit. And even when a favourable effect, for the time, appears to be produced, there is always a risk of ultimate detriment to the powers of the stomach from this habitual excitement.

There are states of mind, and habits of life, which, having no direct relation to the organs of digestion, yet exercise a material influence over their functions. Mental distress; mental solicitude; mental toil; over-much study; want of exercise: these are all prolific sources of dyspepsia. Sedentary habits, when their injurious effects are known, may be altered: excessive intellectual labour may be abandoned, or abridged: but it is seldom that we can minister to a wounded or an anxious spirit. Our task is hardest of all when the patient's anxiety relates to his own complaints; when he is morbidly engrossed by his bodily feelings, and despondent about his recovery. 'No man,' said Mr. Gladstone most truly in one of his speeches in Parliament, 'no man can be constantly watching the state of his own health, without injuring it.' The management of the mind of a hypochondriac is peculiarly nice and difficult. It will not do to treat him as if his ailments were imaginary. He disbelieves you, contemns your judgment, and deserts you: to be fleeced perhaps by some unscrupulous quack. You must hear what he has to say; show an interest in his case; and prescribe for him: assuring him that you understand his malady, that it is curable, and that he *will be cured* provided he follows your directions. If you can succeed in gaining his confidence, and in persuading him of this, the battle is half won. To tell such a person, however, not to think of his grievances would be worse than useless. The very effort to drive a subject from our thoughts fixes it there the more surely. But you must endeavour to turn his attention to other things; and to awaken in him some new interest. Prescribe *change*: change of air; change of place and scenery; change of society. Get him to *travel* in search of health; and the chances are in favour of his finding it. A tour, in fine summer weather, and through a pleasant country, combines almost all the ingredients which are, separately even, desirable: the withdrawal of the mind from its ordinary pursuits and cares; the diversion of the attention from one's self, by new and varied objects; exercise, carried on in the open air; a holiday from intellectual toil. Six weeks among the mountains of Switzerland, or upon the rivers of Germany, will often do more towards restoring a dyspeptic hypochondriac, than a twelvemonth's regimen and physicking at home.

With these disjointed hints, Gentlemen, I must request you to be satisfied in regard to the principles upon which dyspepsia—and the hypochondriasis, which is in general so closely linked with dyspepsia—are to be managed. A full discussion of these subjects in detail would furnish matter for several lectures.

LECTURE LXV.

Enteritis: its symptoms; causes; treatment. Mechanical occlusion of the Intestinal Tube. Colic. Colica Pictonum: its symptoms, complications, treatment, and prevention.

SOME of the diseased states of the intestinal canal, while they differ much in their essential nature, have yet many characters in common. Colic; ileus; enteritis; mechanical obstruction of the tube. It will be convenient, therefore, to consider these disorders in succession, and in some degree of connection with each other.

In *colic* we have pain of the abdomen; pain of a twisting or wringing kind, occupying generally the umbilical region; vomiting; and costive bowels. Similar pains are apt to attend upon diarrhœa; but they are transitory, and are then termed *gripings*, or more learnedly *tormina*. When they are violent and more permanent, and, above all, when attended with constipation, they constitute colic.

You have not forgotten the symptoms of *peritonitis*. They are, briefly, pain in the abdomen, increased on pressure; and fever.

Now if to the symptoms of colic, you add the symptoms of peritonitis, you have the symptoms of *enteritis*: by which word I desire to express the disease that is commonly called *inflammation of the bowels*. The term has lately been extended so as to signify any and every form of inflammation which any portion or tissue of the intestinal canal within the belly may suffer; but I use it in its old-fashioned sense. Cullen makes two species of enteritis. One of these consists in inflammation of the mucous membrane of the intestinal tube: he calls it enteritis *erythematica*. That is not the disorder I am about to speak of; but the other of his species, the enteritis *phlegmonodea*. I say that in colic, we have abdominal pain, constipation, and vomiting. In peritonitis, the functions of the stomach and intestines are not, necessarily, affected: in enteritis they are. There is inflammation, not merely of the peritoneal coat, but of the areolar tissue uniting the several coats, probably of the muscular tissue also, and often of the whole substance of the bowel at the inflamed part.

When the intestinal channel is any how closed up, and a bar placed to the passage of its contents, the symptoms of colic are very apt to ensue; and at length, the obstacle continuing, fatal inflammation is set up.

The term *ileus* is applied, I believe, to those cases, whether inflammatory or not, in which the contents of the intestines are carried in a retrograde course into the stomach, and thence out of the body by vomiting.

Having made these explanations, I shall now address myself more especially to the consideration of enteritis.

It is of much practical importance to discriminate between enteritis and the disorders that resemble it; and particularly to distinguish it from colic. When it commences, as it sometimes does, with distinct rigors, and is attended by thirst, a hot skin, and a hard and frequent pulse, there is no room for doubt. But it often begins insidiously, with mere colicky symptoms; the pain, at first, is not much augmented, it may even be somewhat eased, by steady pressure. If we mistake colic for enteritis, the error is of no great moment; but the opposite mistake, which is more common, may be fatal. The abstraction of blood, and the other remedies of enteritis, will not aggravate the mere colic; they may even, though unnecessary, relieve the patient. Some of the remedies of mere colic are, however, highly dangerous when there is inflammation of the bowels. Physicians may fall into this error: patients who choose to prescribe for themselves, commonly do so. They take stimulants, cordials, carminatives—the pleasantest and nearest at hand is a glass of brandy or of gin. And in true colic these means are frequently of much service: but they exasperate the symptoms and increase the mischief when the disease is enteritis. Indeed, treatment of this kind may sometimes urge colic into enteritis. If the case be ambiguous, you must act upon the most unfavourable supposition, and treat the complaint as if you were sure that inflammation was present.

The pain of enteritis is increased by pressure. The pain of colic is not only not made worse, but is actually mitigated often, by pressure; and it usually intermits entirely. I know that when there is simply flatulent distention of the intestines, pressure does sometimes increase the patient's uneasiness; but the uneasy sensation is very different from that acute suffering which shrinks from the touch, in inflammation of the peritoneal covering. In enteritis also there are *paroxysms* of severe pain, determined, probably, by the peristaltic movements, or by the temporary distention, of the inflamed parts of the bowel; and the pain has frequently a twisting character: but there is not any thorough intermission. There is a duller abiding pain between the sharper fits. It is to be observed too, as a diagnostic circumstance, that the patient lies on his back, with his knees drawn up, as in peritonitis, and is fixed in that position, and for the very same reasons. If, in his agony, he toss about his arms, the trunk is kept motionless, and the respiration is thoracic: whereas in mere colic the mode of breathing is not altered, and the patient is apt to be turning and writhing in all postures, and out of one posture into another.

The nausea and vomiting are often most distressing. The patient not only rejects immediately whatever food, drink, or medicine he swallows, but he has fits of retching when the stomach is empty. In some instances, matters are cast up having the appearance, and something of the odour, of liquid fæces: or resembling, at any rate, the offensive fluids that are found in the small intestines after death in these cases. I may say that I have seen genuine excrement ejected, unequivocal ordure: and this may well happen if it be true that clysters, introduced into the rectum, have been voided through the mouth. Such a phenomenon would show that the whole tube was pervious; that there was no mechanical obstruction.

Although the fever, in the outset, may be high, and the pulse sharp and hard, it soon becomes (as in all acute abdominal inflammations) small and wiry; or weak, and like a thread. In bad cases, as the disease proceeds, the abdomen begins to swell, becomes tympanitic; hiccup sometimes comes on; the pulse intermits or beats irregularly; the extremities grow cold; the features are sharpened and ghastly; cold sweats break out; the pain ceases perhaps; and the sphincters relax. The head is generally unaffected. Now and then delirium occurs late in the disease; but much more frequently the intellect remains clear to the very last. Death begins at the heart, and takes place in the way of asthenia.

You may probably have observed that the symptoms which I have been describing are just the symptoms which the surgeon so frequently encounters in cases of strangulated hernia. The symptoms of that surgical complaint are, in truth, most commonly at least, the symptoms of enteritis, caused by the forcible closure of the bowel. Obstruction to the passage of the contents of the gut gives rise to its inflammation. And we often find, after death preceded by the signs of enteritis, an internal mechanical obstruction—an internal strangulated hernia. In some cases, bands, or strings of coagulable lymph, the products of bygone inflammation, have formed snares (so to speak) for the gut, which at length they catch and constrict. They do no harm till some coil of intestine gets beneath or beyond them; and then they strangle it, as the phrase is. I have twice seen (as I think I formerly stated) the appendix vermiformis prove the immediate cause of fatal internal hernia. In one of these cases, the person had been a private patient of Dr. Macmichael's; and I went with him to examine the body. The free end of the appendix had become adherent to the mesocolon; and so a loop was made, through which a portion of the gut had passed. In the other instance, which I saw at Edinburgh, the appendix was literally tied round a piece of the intestine. Sometimes, again, there is intus-susception: the upper part of the tube slips into the lower, fills it up, obstructs it; and inflammation or exudation tends to fix it there. Or it may be that a chronic thickening of the coats of the intestine has narrowed its channel; or a tumour presses upon the intestine from without; or some foreign substance, or morbid accumulation, plugs it up within; in all which cases a chronic disorder passes at length into acute inflammation. A very small hernia at one of the usual orifices—not large enough to manifest itself externally—is sometimes the cause of the obstruction; even though only a portion of one side of the gut may be nipped in the aperture. Or, finally, the bowel may get casually twisted on its axis, or lapped over upon itself.

Sometimes, but according to my experience less frequently, there is no mechanical impediment to account for the constipation. In all cases, whether there be mechanical obstruction or not, the inflamed portion of the gut is of a red or dark colour; distended by its gaseous or liquid contents; covered often, on its peritoneal surface, with coagulable lymph; or adherent to the contiguous organs. When the gut is mechanically closed, the part which lies on this side the obstacle, as we follow the natural course of the channel, is inflamed; the part which lies beyond it is pale, contracted, and, to all appearance, healthy. The line of demarcation is abrupt and strong; and it is determined by the obstacle. And the distinction between the healthy and the inflamed portions is usually as sudden and decided, when there is no apparent obstacle. I mean that up to a certain spot the intestine is red like a cherry, or dark like a grape, large, and smeared (perhaps) with lymph or with pus; while immediately beyond that point, and throughout the remainder of its course, it is white, empty, and shrunk up into the semblance of a cord. The pathology of these last forms of the disease is full of difficulty. Some have held that the contracted part was the original seat of disease, namely, of *spasm*; and that the upper adjoining portion of intestine became distended and inflamed in consequence of such spasm. Without attempting to explain these phenomena, which are comparatively unfrequent, I content myself with observing that it is the distended part which is the really inflamed part, and that its muscular coat appears to have lost its natural contractile power—not (I fancy) from overstretching, as now and then happens to the urinary bladder—but from the effect of the inflammation.

The inflamed portion of bowel is often of a very dark colour, and even almost black, from the great congestion of blood in its tissues. Now this black colour has sometimes been erroneously set down as evidence of gangrene. You must not trust to the mere colour, however. If the coats of the bowel are firm, and if the peculiar odour of gangrene be wanting, you are not to conclude that mortification has taken place, simply from the dark appearance of the intestine.

Enteritis, not dependent upon any mechanical occlusion of the bowel, may arise under the operation of the ordinary causes of internal inflammation: cold and wet, for instance, applied externally, and especially to the feet and legs. Cold is thought to be particularly injurious in this way when the exposure happens soon after a meal: probably because at that time the digestive organs, being in activity, receive a more copious supply of blood.

There is, however, good reason to believe that pure idiopathic enteritis very rarely befalls a healthy person. Of course it may be produced by mechanical injury. But if you inquire closely into the condition of patients who are suffering what seems idiopathic inflammation of the bowels, you will almost always find that they were already unsound; affected with renal disease, or with some other distemper implying a morbid state of the blood.

The mechanical impediments that occur *within* the bowel are still more various in kind than those which constrict it from *without*. Hardened fæcal matters: intestinal concretions. Some of these are curious, and I shall hereafter have a word or two to say respecting their composition. Persons who are in the habit of taking a good deal of magnesia, or of chalk, to relieve acidity and heartburn, are liable to have these substances accumulate, and become indurated in the intestines. They generally begin to collect, I believe, around some accidental nucleus: and such a nucleus may very readily be supplied. A cherry-stone, a fish-bone, a gall-stone that has found its way into the bowel. Even a pill, prescribed to cure, may thus come to kill one's patient. Dr. Prout was asked to analyse certain odd-shaped, triangular bodies which had been voided at stool, and were supposed to be gall-stones, but he found that they were specimens of Plummer's pill, which had defied the solvent action of the gastric juice, and had passed into, and lingered in, the bowels. Unbruised mustard-seed, and carbonate of iron, are other remedial substances which, injudiciously administered, have collected in hard masses, and caused intestinal disease. It is but seldom that we can discover, during life, what is the exact nature of the mechanical obstacle. Yet there are some symptoms which are very significant. For instance, in most cases of obstruction from *intus-susception*, I have found that after severe and sudden symptoms of obstruction and inflammation, *blood* has been passed by stool. Sometimes the included portion of bowel itself sloughs away, and is expelled *per anum.*

Inflammation of the bowels requires very much the same kind of treatment as peritonitis: indeed, the disease is in most cases peritonitis, and something more. The patient must abstain from every sort of stimulus, and confine himself, as strictly as may be, to the horizontal position. As in simple peritonitis, the abdomen must as early as possible be covered with leeches, and afterwards with fomentation cloths. Should one spot or region be more painful and tender than another, on that part the leeches are to be accumulated. It may be requisite to repeat the leeching once, or twice, or oftener; the propriety of such repetition depending upon the urgency and obstinacy of the symptoms, and upon the age and state of the patient. I am speaking of idiopathic enteritis, and of its earlier stages. In the advanced periods, when feebleness of the pulse is associated with tympany of the belly and coldness of the surface, it betokens weakness and sinking, and the tendency to death by asthenia; and, consequently, it then indicates support rather than depletion. So also when the enteritis is not idiopathic, but consecutive—when we have reason to believe that it arises out of a firm mechanical impediment—our employment of bleeding must needs be modified by that circumstance.

One point in the management of enteritis, requiring great caution and judgment, relates to the exhibition of purgative medicines. The costive state of the bowels is apt to be looked upon as the main evil, and their evacuation as the chief indication of treatment; but great mischief, I apprehend, is likely to arise from the exclusive pursuit of that indication. I am still speaking of the idiopathic disease, where it is presumable that no mechanical obstacle exists to render the passage of the fæces impossible. Purgatives given by the mouth are often rejected by the stomach, with great distress to the patient. If they are retained, and fail to operate, they must do more harm than good. Certainly they should not precede the leeching. I well remember, though it is now many years ago, being myself badly treated for enteritis. Being ill, in a strange place, I sent for the nearest practitioner, who happened to be a very ignorant man. Finding that I was sick, and that my bowels did not act, he gave me, for two or three days in succession, strong drastic purges, with no other effect than that of increasing my sickness and adding to the abdominal pain I suffered. I was then seen by a most intelligent physician (this was before I had paid any attention to physic myself), and the first thing he did was to have me copiously bled; and the immediate effect of that bleeding was to send me to the night-chair. And I am persuaded that when evacuations follow the administration of purgatives in such cases, they are often owing to the bleeding and other antiphlogistic measures that are employed at the same time. These are the opinions of Dr. Abercrombie also, who says, 'I confess my own impression distinctly to be, that the use of purgatives makes no part of the treatment of the early stages of enteritis; on the contrary, that they are rather likely to be hurtful until the inflammation has been subdued.' It is an instructive fact, that when purgative medicines *do* operate during the height of the inflammation, the stools they produce are merely watery; and it is only after the inflammation has been reduced, that *fæces* are discharged, and then in such quantity sometimes as to show that they must have been shut up in the bowels during the period of active inflammation.

You should wait, then, for the effect of other measures, leeching, fomentation, before you think of active purgatives by the mouth; wait patiently; the bowels will generally act of their own accord when the inflammation is fairly at an end.

If not, let the laxatives given be of the mildest kind, such as are least likely to excite irritation of either the stomach or the bowels—castor oil, for instance.

The same objections do not apply to enemata, which soothe while they empty the lower parts of the canal. You will read or hear of great service done by an infusion of tobacco, or by the smoke of tobacco, thrown into the bowels through the rectum. Dr. Abercrombie speaks of a weak infusion of the leaves as being a remedy of very general utility. It must, I apprehend, be a *weak* infusion to be at all safe. This is a measure which, for my own part, I should not dare to adopt. The injection of a large quantity of mere warm water, however, or of warm oil, is often of much use: from two to six pints, for example, thrown up gradually and gently. Indurated fæces are softened and brought away in this manner, and the warmth acts as an internal fomentation, and is comforting. Nay, such injections may sometimes have a happy effect even mechanically; untwisting, for example, a twisted gut, or setting free the half-ensnared intestine.

Of internal medicines opium is, again, by far the most important. It allays pain, it moderates or prevents dangerous peristaltic movements of the inflamed bowel, it settles often the irritable stomach, favours diaphoresis, and improves the pulse.

There is one very important point in the treatment of enteritis that I have yet to mention. I have told you that in the advanced stage of the disease symptoms of sinking often come on: a total cessation of pain, failure of the vital powers, and coldness of the body. These symptoms, this collapse and approach to syncope, are generally considered to denote gangrene, and, therefore, a hopeless state of disease. Yet in many cases which have proved fatal after similar symptoms, not a trace of gangrene has been discoverable. So that this unpromising change in the symptoms does not always indicate a morbid condition which is necessarily mortal. And if the patients are to be saved at all, they are to be saved by wine and support. We must combat the obvious tendency to death by asthenia.

‘A man, aged forty, was affected with enteritis in the usual form, for which he was treated in the most judicious manner by a respectable practitioner. On the fifth day the pain ceased; the pulse was 140, and extremely feeble and irregular: his face was pale, the features were collapsed, and his whole body was covered with cold perspiration; his bowels had been moved.

In this condition (says Dr. Abercrombie, from whom I quote the case) I saw him for the first time. Wine was then given him, at first in large quantities, and upon the whole, to the extent of from two to three bottles during the next twenty-four hours. On the following day his appearance was improved; his pulse 120, and regular; the wine was continued in diminished quantity. On the third day his pulse was 112, and of good strength, and in a few days more he was well.'

Dr. Abercrombie relates other cases to the same effect; and most physicians, I suppose, have occasionally seen such. They teach us that we must not abandon our patients in despair, even under the most adverse circumstances. If diarrhœa should supervene with this state of collapse, opiates must be joined with the wine. External warmth is also a powerful auxiliary.

I know of no cases of disease more painful to witness or to treat than those which result from invincible obstruction and closure of the intestinal tube. They are usually attended, sooner or later, with enteritis; but they differ much in some respects from what I may call simple enteritis. The inflammation is an accident or consequence of the obstruction; or of the means used to overcome it. What happens is often of this kind. A person thinks it expedient to take some aperient medicine. It has no effect. He repeats the dose. It causes pain and gripings, and probably sickness also; but still the bowels are not moved. Enemata are employed. They bring away, perhaps, some hardened fæces, but there is no further relief. Meanwhile the patient may have a clean tongue, a quiet pulse, a cool and soft skin, and a supple and painless abdomen. Purgatives of a more drastic kind are tried, but tried in vain; and the physician is summoned.

The symptoms of intestinal obstruction may wear, however, in the outset a very different aspect; setting in suddenly, with violent and sickening pain in the abdomen, a weak, slow, faltering pulse and a cold skin, and after no long time, the ordinary signs of enteritis. Such symptoms, when they arise from obstruction, denote some internal state analogous to external strangulated hernia.

Now the first thing that you have to do, when called to a stubborn case of 'obstruction of the bowels,' is to search narrowly whether there be not some unsuspected external hernia. All delicacy must be waived; and every part of the body, where hernia may possibly show itself, must be submitted to inspection. If none be found, the rectum, and in women the vagina, must be severally explored. Stricture, or a quarry of impacted fæces, may possibly be detected in the one: a uterine or other tumour, pressing upon the bowel, may perchance be felt through the other. I shall never forget the shock I once experienced on being sent for to see a woman, of middle age, who was *in articulo mortis*, and who, as I was told, had for some days been labouring under sickness, pain in the abdomen, and constipation. In her left groin there was a large palpable strangulated hernia, which had not been detected by the practitioner in attendance, simply because it had not been looked for; and which was discovered only when it was too late for remedy. Once, since, I had the better fortune to retrieve a similar oversight in time. Much pain in the abdomen there had been, and frequent sickness, and no alvine evacuation for eleven or twelve days. During this time active purgatives had been given, and had been worse than useless. The matter vomited became unequivocally stercoral. Within half an hour after the hernia was detected, the strangled gut was released by a surgical operation; and a life was saved which I firmly believe another day's delay would have forfeited.

Remember, then, that in every case of obstinate costiveness, with signs of inflammation within the abdomen, it is absolutely necessary, for your own credit and subsequent comfort, as well as for your patient's safety, to make diligent and thorough inquiry after such herniæ as may be recognised externally.

But often nothing of the sort can be found, and then the physician is apt to think himself at liberty to prosecute with more energy and decision the purgative plan of treatment. He prescribes strong doses of jalap and calomel; black draughts. The stomach being irritable, he gives *pills* of cathartic extract, and repeats them at short intervals; or large doses of calomel, ten grains or a scruple, three or four times in succession. He injects stimulating clysters. Then he is driven to croton oil: and at last, in some vague hope of relaxing spasm, to opiates. If symptoms of inflammation spring up, he puts fairly in force the remedies of inflammation; and especially the abstraction of blood. But all is in vain. The medicines are vomited; or, if retained, they serve but to augment the patient's distress, producing or renewing the pain and the nausea. It is extraordinary how comfortable he sometimes becomes upon the intermission of these active attempts. Now and then he suffers tormina, or has fits of retching: but in the intervening periods his sensations and outward condition may be those of perfect health; only, there is no alvine discharge.

It will be profitable to consider somewhat more particularly the various forms and evidences of an obstructed bowel.

If, then, the obstruction be announced by sudden and severe pain, it is of sudden origin: and is probably owing to intussusception; to a rapid plugging of the tube by some hard substance, such as a large gall-stone; to a twist, or some other kind of abrupt displacement of the bowel, whereby its channel is at once blocked up, and its texture hurt. When the obstruction has disclosed itself more slowly, without pain, in a person habitually costive, and who may perhaps have previously suffered attacks of obstinate constipation, then the impediment is more likely to be due to a collection of hardened fæces within, or to pressure from without the bowel, or to some kind of stricture.

Now it is a step towards exactness of diagnosis, to know that of these morbid conditions some—as those which result from constricting bands, loops, slits in the mesentery, and other peritoneal entanglements, and also the impaction of gall-stones—are most commonly met with in the small intestines: some—as strictures, compressing tumours, and twistings or overlappings of the bowel upon itself—affect chiefly the large intestines: and some—as invaginations—are common to the two, and most often involve them both.

Again, fatal *stricture*, which is much more common in the large intestines, is found to have its seat most frequently towards the termination of the bowel, in the rectum or in the sigmoid flexure, and with diminished frequency in the backward direction towards the cæcum.

The whole of this most interesting subject has been carefully and successfully studied by Dr. Brinton. You will find a vast amount of statistical information and original research, and sound pathology, in his 'Croonian Lectures,' delivered before the College of Physicians in the year 1859.

It is very desirable that you should have a clear conception of the consequences which necessarily result from a bar put to the onward passage of the contents of the intestines, wherever that bar may be placed.

The first and obvious consequence must be the accumulation of those contents above the obstacle: and this accumulation stretches and distends the gut, and at the same time provokes its peristaltic action. At length, if the distention become extreme, and still more surely if the distended portion become inflamed also, its contractile power is lost: the portion of bowel immediately above the obstacle is paralysed.

But before—as well as after—this happens, another effect of the peristaltic efforts to overcome the obstruction is apt to take place, which requires attentive consideration. If the patient continue to swallow food, or drink, the bowel becomes more and more full and bulging, and may even be made to *bag* round the constricted spot. The peristalsis churns the accumulating matters into one homogeneous mass, which is more or less liquid. As those portions of this liquid matter which are in contact with the contracting walls of the intestine are forced onwards, a portion of the matter ascends by a central upward or returning current, and is ultimately brought into the stomach, and vomited. This, as Dr. Brinton was the first to teach, appears to be the true pathology of fæcal vomiting. According to him, there is no such thing as a reversed peristaltic action. The reflux of the contents of the gut is caused by the direct and forward propulsive action of the impeded bowel; part of the force being expended against the sides of the bowel, which yield and become stretched, part in producing the backward central current of the matters propelled.

The only facts that I am acquainted with in contravention of the absoluteness of this rule, are the regurgitation of food from the cardiac end of the œsophagus into the mouth, and of bile from the duodenum into the stomach.

Observation of the manner, and of the period, in which these consequences of the intestinal obstruction show themselves, furnishes further help towards the special diagnosis in each case.

If the obstacle be situated high up in the intestinal canal, the portion above it will the sooner become full, and vomiting will be an early and a frequent symptom. Conversely, if vomiting (unprovoked by any remedial treatment or injudicious diet) occur early in the case, the obstacle is presumably in the small intestines, and the matters vomited may not be as yet fæcal.

When the obstacle lies in the large intestines, it will take a longer time for the bowel to fill up. The distention of both small and large intestine must be considerable to overcome the impediment opposed to the reflux current by the valve of the cæcum; and that current has also to be continued through a greater distance before it reaches the stomach. In fact, there may be no fæcal vomiting at all. The case may terminate (supposing it to be necessarily fatal, the obstacle being invincible) before any reflux current to the duodenum or the stomach has been established.

Hence, if with evidence of obstruction of the bowels, there be fæcal vomiting late only in the disease, or no vomiting at all, the obstruction is in the large intestines: and generally, though not in any unvarying ratio, the earlier the vomiting the nearer is the obstacle to the stomach, and the later the vomiting the nearer is the obstacle to the rectum.

It has been suggested by Dr. Barlow that the quantity of urine secreted may furnish a clue towards discovering the situation of the impediment. If the urine be copiously voided, the impediment, he infers, cannot be very high up in the small intestines. He argues that if fluids, after being swallowed into the stomach, are unable to find their way into, or far into, the intestines, so as to allow of their passage by absorption or by imbibition into the capillaries of the portal system, those fluids cannot reach the emulgent arteries, and very little urine will be secreted.

Dr. Brinton, however, while acknowledging a general inverse relation between the amount of fluids vomited, and the amount of urine voided, doubts the conclusiveness of Dr. Barlow's test. He finds that the facts do not always accord with the theory; and refers to actual cases in which obstructions near the end of the large intestine were attended with scarcely any urine for many days; to other cases, in which there was the coincidence of a plentiful secretion of urine with obstruction high up in the small intestines; and again to others, in which the quantity of urine secreted varied much from day to day. In fact, there are, besides the situation of the obstacle, other circumstances which may influence the amount of the urine: it may be increased in consequence of the quantity of fluid poured into the affected bowel from its mucous surface; or diminished in consequence of the supervention of inflammation and fever. The secretion may be lessened also from failure of the circulation and of vital power, connected with the approach of collapse.

The place of the obstacle may sometimes be ascertained by exploration of the rectum. Stricture, cancer implying a narrowing of the tube, impacted fæces—any one of these conditions occurring in the rectum, may be reached by an inserted finger. Bougies, and even long flexible tubes, are sometimes introduced into and through the channel of the rectum: but this method of exploration is never without some hazard of injury to the gut, even in practised hands; in unpractised hands this rough procedure is always dangerous. Dr. Brinton justly observes that all, or nearly all, that can be learned by means of a tube, may be more safely learned by means of a lavement. If large enemata, slowly injected, find a ready passage, the obstacle can hardly be lower down than the valve of the cæcum. When it is situated in the large intestine, the gut will receive scarcely half a pint without resenting it. Dr. Brinton is of opinion, from some trials made by him, that the rectum cannot be made to receive more than a pint: that if no more can be got up, the obstacle is probably in the sigmoid flexure. Tenesmus; a frequent desire to go to the night-chair; great distention of the abdomen, with rumbling and borborygmi, and with no escape of the flatus *per anum*;—these symptoms are also eminently significant of occlusion of the large intestines. But the small intestines, in these cases of obstruction, sometimes attain to an enormous diameter.

An expert hand, and a practised ear, may learn much from an external examination of the distended abdomen: nay, the eye may sometimes almost look through its walls, and take cognisance of the state of matters within. The magnified bowel destroys the symmetry of the swollen belly, which bulges here or there; and the bowel itself may from time to time be seen and felt to rise and twist with loud rumblings, attended with paroxysms of sickening pain. Even in the intervals between such paroxysms you may hear, by means of a stethoscope, continual minor gurglings and amphoric cluckings, attesting the gradual shifting struggle of the intestinal gases from one part of the bowel to another. Percussion, gently performed, will reveal, to an experienced listener, the track of the affected gut, and will ascertain where its gaseous, and where its more solid contents, are from time to time disposed: nay, the skill is, I believe, attainable, though it requires much practice, of distinguishing between the different tones of resonance given out severally by the stomach, by the small intestines, and by the large. This power of nice discrimination cannot, however, be universally looked for; yet by these modes of examining the uncovered abdomen the position of the bowels may be mapped out, their shifting contents and movements traced, and the place of the obstacle, with more or less of certainty, determined. It should be remembered that the small intestines may be distended so as to equal or exceed the natural dimensions of the large ones, and that the large ones are capable of being inflated into an enormous magnitude.

Excluding, for the present, those cases in which the obstruction is believed to result from intus-susception, what course are we to pursue when summoned to a patient suffering under chronic obstruction of the bowels? Generally, I say, you will find that purgatives have already been given—often sharp and repeated purgatives of various kinds—and given in vain, or with the effect of producing increased pain, or vomiting: and the first question with which you will be pressed will be whether, and how far, this mode of endeavouring to remove the obstruction is to be adopted, or continued.

Common sense, and common humanity, will teach you that the purgative plan must be abandoned the instant you are convinced, or even suspect, that there is a mechanical obstacle which cannot be overcome. When the symptoms have set in suddenly, with intense and increasing pain, purgatives are to be avoided from the first. In the slower forms of the disease, commencing without pain, and as mere inactivity or unusual inactivity of the bowels, it is natural, and it may sometimes be right, to combat that inactivity by the ordinary means. But to persist in the use of drastic purgatives after the conviction, or the strong suspicion, has arisen that the inactivity is due to an obstacle invincible by drastic purgatives, is to inflict wanton and needless torture upon the patient. But how are you to know this? That is one difficulty. And how are you, believing that it is so, to satisfy the patient's friends that his disorder is irremediable; and, to resist their importunity, that you should try this or that; how persuade them to look patiently on while their relative is slowly perhaps, but surely perishing? These are great and terrible difficulties.

You will be urged with all imaginable suggestions; even the most absurd. Crude mercury may perhaps be one. Pounds of this metal have been swallowed in such cases; in the hope, I suppose, that it would force a passage by its weight. But the obstacle may be in an ascending coil of intestine. And if not, experience does not teach us to put any faith in this rude mechanical remedy. It has often done mischief, and seldom or never done any good. The metal is apt to become oxydized in the body, and then to produce very distressing salivation. Yet in a case recorded by Mr. Adams, in which half a pound of quicksilver had been administered, two ounces and a half of the metal were voided, unchanged, five weeks afterwards. Profuse salivation is an evil which I have known to occur, and to trouble the patient greatly, some time after the ineffectual exhibition of large doses of calomel.

Dashing cold water over the abdomen and the lower extremities is another rough expedient, which is sometimes successful in producing evacuations. It was adopted, after various other measures had failed, in the case in which the bowel was tied down by the adherent appendix vermiformis; and it caused the emptying of that part of the canal which lay beyond or below the internal hernia. It is plain that this partial success can be of little or no use; certainly of none that can compensate for the shock and annoyance of the cold affusion.

Now we have reason to fear that some mechanical impediment has rendered the bowel absolutely and almost hopelessly impassable when, the constipation being obstinate, we discover a tumour, or hardness, in some part of the belly: or when we receive a history of some former inflammatory attack, since which the bowels have been habitually difficult to regulate. Our fears are heightened when the patient is conscious that injections reach a certain spot, and there always stop; and that the intestines rumble, and roll, and propel their contents downwards to the same spot, and no further. And this is the distinctive symptom upon which I desire to fix your attention. The abdomen gradually enlarges, especially if the patient be able to retain food. The intestines fill up above the obstacle; and then throes of pain occur, *spasms* the sufferers usually call them, attended with sickness; and during these pangs you may feel, and if the abdomen be uncovered you may see, immense coils of intestine, as big perhaps as one's arm, rise and roll over, like some huge snake, with loud roarings and flatulence. When this takes place, the time for giving purgatives is certainly over. The distended bowel requires no stimulus; it acts, and strives with all its power, but strives in vain, to overcome the opposing barrier. If you would consult your patient's ease, if you would not embitter and abbreviate his slender remnant of life, harass him no further with active remedies.

The object of our treatment in all these distressful cases must be to unlock, if that be possible, the closed bowels; or, if that be not possible, to allay the patient's sufferings, and to prolong his doomed life, by postponing those consequences of the obstruction which tend to destroy life: vomiting namely, inflammation of the bowel, paralysis of the bowel, rupture of the bowel, and especially and in the foremost place, *distention*, wherein, as their common cause, all these other consequences merge.

Dr. Brinton states, as the results of statistical calculations, that in fatal cases arising from sudden constriction of the gut by bands or other external ligatures, the average duration of the malady is about five days: in cases produced by twisting of the gut, one-half of which occur at the sigmoid flexure, about nine days and a half: by stricture in the large intestine, which occurs four times as often on the left of the mesial line of the body as on the right of that line, about twenty-three days after complete obstruction.

Sometimes, however, the patient lives on, without any evacuation from the bowels, but with long intervals of ease and comfort, for four, five, or six weeks. I had the pain of attending one lady who lived forty-six days after the last fæcal alvine discharge. Even a brief respite may be of infinite value; giving time for the arrival of distant friends, for the settlement of worldly affairs, and for preparation of the spirit against the inevitable hour.

It is in the protracted cases chiefly that we dare hope for eventual recovery. The last forty-eight hours of those which end fatally are generally, according to Dr. Brinton's experience, hours of suffering.

Some few of these protracted cases, when let alone, do terminate at length in solvency of the bowels, to the surprise and delight of the lookers on.

Keeping, then, in view the object of averting, or, if that be not possible, of postponing, undue and unsafe distention of the bowel above the seat of the obstruction, we limit in the first place the supply of food taken by the mouth, and regulate its kind. Thin liquid nutriment must be given, in small quantities at a time, and at frequent intervals, so that the intestine may not get filled up and stretched by the mere ingesta. And as conducive to the same object, by controlling peristaltic action, opiates are eminently useful; while they have the further advantage of checking sickness, and of allaying pain. We seek to restrain within salutary limits the healing force of nature; to moderate without suspending the propulsive movements of the affected bowel. The extract of opium, in half-grain doses, repeated every six hours, is about the average quantity requisite for these purposes. It is surprising how much relief is experienced by the patient, on first taking this remedy, from the vomiting which had previously distressed him, and from the frequent recurrence of those throes of pain which accompany the paroxysms of striving action in the intestine. That the peristaltic movement still goes on, though no longer with excessive and hurtful energy, you may learn by auscultation: and then, as Dr. Brinton has remarked, you have a gentle force continually acting through a wedge or cone of liquid, and tending to dilate the obstructed part. If the impediment be superable, this is the surest and safest way to overcome it. If it be insuperable, the distention, paralysis, and inflammation of the bowel are put off to the latest possible period. Life is prolonged, and pain is mitigated and postponed.

Meanwhile help may be rendered in the opposite direction. Nutritive unirritating enemata, injected slowly and carefully into the rectum, have generally a soothing influence upon the patient, contribute towards supporting his strength, and may sometimes have the further good effect of softening impacted matters within the bowel, and of soliciting, by fomentation of the lower boundary of the obstructed part, the passage of fluids accumulated above it. If they reach the obstructed part, they may help by dilating the bowel below the obstruction.

These measures, with fomentation of the abdomen externally, or gentle friction of the surface with warm oil, constitute the plan which, with such modifications and adjustments as the special circumstances of each case may dictate, is the best that you can adopt in these trying emergencies.

In such cases as we have now been considering, purgatives, from first to last, are to be shunned not only as useless, but as being calculated to work mischief.

Not that I would have you look upon every case in which the bowels seem shut up, and refuse to act under ordinary remedies, as unfit for treatment by brisk purgatives. But you should avoid them when those proofs present themselves which I have already pointed out, that the peristaltic force of the intestines is in active operation. And even before that writhing and striving action of the bowel becomes plain to the touch and to the eye, indications may often be noticed of sufficient movement in the intestinal canal to dissuade you from further irritating it by drastic aperients. Undue distention, and difficult shiftings of the contents of the bowel, reveal themselves to percussion and auscultation before the severer symptoms set in.

Many instances occur of obstinate constipation from mere torpidity of the bowels, warranting the use of purgatives, and yielding to a few grains of calomel, followed after two or three hours by a strong senna draught: or to a full dose of calomel and jalap powder. The intestines may be insensible to ordinary stimuli; and may really require strong rousing. Dr. Abercrombie mentions the case of a gentleman whose bowels were locked up by an accumulation, as the result showed, of black hardened fæces. The obstruction, which had resisted the most powerful purgatives, and was accompanied by an evident and painful distention of a part of the abdomen, yielded at once to the repeated application of galvanism to that part: each application being immediately followed by a copious evacuation. Here the flagging muscular action was restored, apparently by the galvanic stimulus.

Some years ago I saw a similar case in consultation with Dr. Tyler Smith and Mr. Walter Bryant. A gentleman of middle age presented the usual symptoms of intestinal obstruction. All the ordinary means of emptying the bowels were diligently tried, and tried in vain. Water was injected by means of a long tube passed through the rectum—with no better result. Almost in despair we at last had recourse to the magneto-electric apparatus; and a current of the force thus generated was directed through the abdomen. You are aware that the strength of such a current is easily varied. When a low power was employed, the patient scarcely felt it. At a certain high power he suffered pain. But in a degree intermediate between these the sensations produced were agreeable. He said that he felt a 'working' in his bowels; and in a short time they discharged their contents. This effect was repeated upon a repetition of the galvanic process: and our patient recovered perfectly. In this instance there was not much distention of the abdomen, and no rumbling movements of the intestines until they were excited by the electric current.

When the obstruction results from impacted fæces, the question of operation can scarcely arise. The fæcal mass must be considerable, and it may therefore be recognised, and the nature of the case discriminated, by palpation of the abdomen externally. Large enemata—as much as the intestines will patiently receive—gradually and gently introduced, and repeated three or four times a day, may sometimes succeed in breaking down and washing away masses of hardened excrement. And if these enemata are composed of milk or beef-tea, and are suffered to remain as long as they will, they may answer another important purpose; they may contribute sensibly to the nourishment of a patient who cannot retain food in his stomach. They are generally very soothing and comfortable, allaying tormina, and abating sickness; and they are adapted to every stage and variety of the complaint.

Again, when, as sometimes happens, the bowel is plugged by a large gall-stone, which has entered it, by ulceration, from the liver, the precise nature of the case is generally revealed by previous symptoms referable to the liver; by pain in the hepatic region, and by jaundice. I may mention here a case of this kind which was relieved by a sort of happy accident. Some time since, I was called to an elderly lady, who from Wednesday morning to the next Monday noon, had had no alvine excretion, notwithstanding the employment of the most active cathartics. She suffered frequent paroxysms of pain and vomiting: but the abdomen was scarcely, if at all, distended; nor was it tender. At length she complained that what she vomited was stercoraceous—to use her own words, 'what came upwards ought to have passed the other way.' It was a thin, brown-coloured, ill-smelling fluid. Dr. Mayo and Mr. Arnott were now associated with me in the case, and they touched and felt the abdomen as I had previously done. The lady observed that their hands were heavy; and she fancied that the pressure they made had displaced something within. And I believe that it was so: for before our consultation in the next room was over, word was brought us that the bowels had acted. She had passed a liquid motion precisely resembling the stuff she had last vomited. The next day, with one of several similar stools, a hard lump was voided, which proved to be a gall-stone, as big as a small walnut.

As large quantities of warm water had without difficulty been injected, I infer that the concretion had been impacted high up in the bowel, probably at the valve of the cæcum. Some years previously the lady had suffered severe abdominal pains, which at the time were ascribed to the passage of a biliary calculus through the gall-ducts.

In this instance, again, there was no evidence of any strong contractile efforts of the bowel above the place of obstruction.

I would not advise you, however, to try firm pressure in any of these cases. You might easily rupture the distended, thin, unadherent, almost rotten intestine, by rough or careless manipulation.

There is one further expedient which I should recommend in those trying cases which we know (no matter how) are of necessity fatal. In cattle that are 'blown' by over-feeding on wet clover, a rough procedure, that of piercing the distended bowel with a hay-fork, has often been practised by farmers with complete success. The distress from extreme distention of the intestines by wind is so intense, the craving for relief from that distress so importunate, and the comfort from obtaining it so great, that were I the subject of such pressing and prolonged torment, I should beg to have the inflated bowel eased by puncture with a fine trocar, even if I might (what is improbable) so lose a day or two of painful life.

Since this thought was forced upon me by sufferings that I had personally witnessed, I have been gratified to learn, from a communication made to the Clinical Society by Mr. Thomas Smith, that the same thought (as was natural) had occurred to others before me, and been acted on with all the success of which it was capable; by Dr. Braxton Hicks, as well as by Mr. Smith, in this country, and by more than one physician on the Continent.

I have hitherto spoken of cases of invincible obstruction as though they were irremediable: but perhaps I have spoken somewhat too absolutely. Life may, sometimes, be reprieved by a surgical operation. The gut may be punctured above the seat of obstruction, and suffered to discharge its contents through what is called an *artificial anus.* In a very interesting paper on this subject, in the thirty-fifth volume of the 'Medico-Chirurgical Transactions,' Mr. Cæsar Hawkins states that at the time when he was writing, 'four persons were living and in good health in this metropolis, whose lives had doubtless been prolonged by the operation.' This expedient is feasible only when the obstacle is situated in the large intestine. Here only can we ascertain, with anything like certainty, the exact place of the impediment. The colon may be pierced from behind, through either loin, without injury to the peritoneum: this is the procedure known among surgeons as Amusat's. The same gut may be opened through the peritoneum, in either flank: this is Littre's operation. I am not qualified to pronounce a judgment upon the comparative merits of the two. The circumstances of each particular case would tend, I imagine, to determine which was the most eligible. An artificial outlet in the loin would probably be less disgusting to the patient, and less offensive to others, than in front of the body. Mr. Hawkins's statistics are not very encouraging. Of forty-four persons operated on, no more than one-half survived the performance of the operation. Among those who did recover, nine only lived so much as one year, and one only had then lived more than three years after it. A respite, however, of a year, or of a few weeks, may well be deemed a most precious respite.

For my own part, in an appropriate case I should think it my duty to *mention*, though I should be slow to *recommend*, this *anceps remedium.* The risk of failure, and the penalty of success, should both be set fairly before the patient. The choice between inevitable death on the one hand, and the chance of living for a short time with a very loathsome bodily infirmity on the other, must be made by himself.

I should express much the same thoughts concerning that more daring exploit of surgery which proposes to lay open and explore the cavity of the abdomen in the living subject, and to disentangle or set free the intestine, strangulated somehow, within. This procedure differs materially from the former, inasmuch as it contemplates no compromise between certain death, and complete recovery. It is fearful to be called upon for counsel, in seemingly desperate emergencies, respecting these scarcely less desperate remedies. Our doubts and indecision, however, as in almost all questions of conduct, are the doubts and indecision of ignorance: ignorance, in the earlier periods, when the operation might be the more hopefully attempted, whether the obstacle be really insuperable by other means; ignorance whether, if so insuperable, it may be vincible by the help of the operation; ignorance, in short, as to its exact nature and place. These are predicaments in which the patient must be admitted to the consultation: and if the peril were my own, and all other prospect of relief had failed me, I would submit myself to this forlorn hope of rescue.

I know of but two instances, though there may be more, in which this operation has been actually performed. They are recorded in the 30th and 31st volumes of the 'Medico-Chirurgical Transactions,' the one by Dr. Golding Bird, and Mr. Hilton, jointly; the other by Dr. Druitt. Though the operations failed to save the lives of the patients, the narratives of these two cases plainly show how life may, peradventure, be saved under similar circumstances. In both cases the strangulated portion of intestine was found—was found in the expected place—and was liberated. The sagacity which detected the nature and seat of the mischance, the skill and courage which planned and attempted its redress, were sufficiently vindicated; but the patients ultimately sank: Mr. Hilton's from exhaustion apparently; Dr. Druitt's from rupture of an ulcerated portion of intestine not far from the place of the obstruction. Had the operations been performed earlier, they might perhaps have been successful: but consent to their performance was not obtained until the strangulation had already existed for fifteen days in the first case, for fourteen days in the second.

The cases best adapted to this bold measure—could we but discriminate them beforehand—would obviously be those of internal strangulated hernia, or of twisted bowel, occurring in persons previously and otherwise in good health. But even if the exact diagnosis were securely ascertained, and the place of the obstruction accessible, and it were certain that nothing but such an operation could save the patient's life, the operation could be successful only when performed *early*—before paralysis of the bowel, adhesions, and inflammation had been established: conditions that can scarcely ever meet in any given instance.

I have still a few words to say respecting *intus-susceptions* of the intestine, for they have characters peculiar to themselves, and require a peculiar method of treatment. And here again I draw, and draw largely, upon the statements and experience of Dr. Brinton.

Intus-susceptions happen in the small intestines, and in the large; and most commonly of all (in fifty-six instances per cent.) they involve both. Thirty-two times in every hundred they are limited to the small intestines, twelve times to the large, including the sigmoid flexure of the colon. Almost always a portion of the bowel passes forwards into (and not over) the portion immediately below it, in which it is thus sheathed. Of the largest of these three classes, the ileo-cæcal class, one-half occur in infants and children under seven years old.

In the small intestines the inverted middle layer of the bowel carries with it a portion of the corresponding mesentery, and the pressure thus produced upon the large and numerous mesenteric vessels leads to stagnation of their contents, and the effusion of blood both into the adjoining healthier portions of the mesentery, and from the mucous surface of the bowel into its channel. Hence it is that in cases of intus-susception hæmorrhage so generally takes place through the rectum. Another remarkable consequence of these invaginations is this. The mesenteric edge of the sheathed portion being fixed by exudation and inflammation, and the opposite edge being freer to move onwards, the axis of the included part becomes curved.

The displacements of the bowel constituting intus-susceptions, form knots or tumours, the size, shape, and situation of which may often be traced by palpation of the abdomen.

The intercepted portion of the bowel blocks up the passage:

or if it should not of itself do this, the consequent swelling and exudation soon render the obstruction complete.

The ileo-cæcal intus-susceptions commence in the right iliac region, pass across the abdomen to the left, dragging downwards the arch of the colon, and finally engage the sigmoid flexure and rectum. These, and others involving the colon alone, may be distinguished from invaginations in the small intestines only, by the prominence of tenesmus as a symptom, by the greater size, and fixity, and position of the tumour, by the smaller amount of hæmorrhage attending them, and often by the presence of the end of the invaginated part in the rectum.

The average duration of the directly fatal cases of intus-susception is, according to Dr. Brinton, five days and a half.

Any surgical operation for redressing the invagination—for opening the belly and pulling out the fragile bowel—seems out of the question. So far as displacement of the bowel operates as a mechanical obstruction, the treatment must be that which has already been laid down as most expedient in cases of obstruction generally. When the intus-susception is situated in the large intestine, large enemata of warm water should be injected slowly through the rectum, 'so as to distend the bowel to its utmost dimensions.' By these means the invaginated part may sometimes be withdrawn by the natural or assisted efforts of the tube itself, and the danger averted. But this can be hoped only early in the case, before adhesions have formed. The inflation of the bowel with air has been recommended, but warm water is probably the more manageable and the more efficacious fluid for unfolding the entangled gut. I have myself witnessed one instance of successful inflation. I was asked, one Sunday afternoon, by a physician, to see his little daughter, aged ten or eleven. On the preceding Tuesday she had gone to see the Queen open the Parliament, and was supposed to have caught cold there, for she began from that time to complain of pain in her belly. Some red discharge was reported by her nurse; and it was conjectured that this might be a show of commencing menstruation. However, she continued ill, and in pain, and was sick, and her bowels refused to act. On Saturday there was a discharge of blood and mucus. Dr. West was then consulted, who found no marks of menstruation. The discharges had been from the rectum. Purgatives, and ordinary injections, had failed entirely of their purpose. It was pretty evident that the child was labouring under intus-susception. As a last resource, and with a faint hope of rectifying the mischief, it was proposed to inflate the bowel with air. This was done, about midnight, by Mr. Erichsen, by means of the bellows used by the Royal Humane Society for producing artificial respiration. With my hand placed on the child's left flank, I could feel as well as hear the air enter with a rush and a noise upon each action of the bellows. Its entrance gave her some pain. She said, 'There'—'that will do'—'oh don't,' &c. The inflation was continued for some minutes, yet the abdomen did not become much distended. After it was over the patient seemed easier; and in about two hours she passed a natural fæcal stool. She got well without another bad symptom.

A cure of this dangerous form of disease is sometimes effected by nature. The pressure upon the incarcerated gut sets up a process of ulceration, and at length the whole of the included part is separated and voided through the rectum; the continuity of the tube being preserved 'by the annular mass of adhesive lymph surrounding the place of the ulceration.'

You may suppose that this process, though often having a happy issue, is full of most serious peril. The separation, Dr. Brinton tells us, is on an average not complete till the eighth day; and the amputated bowel is rarely expelled before the tenth. He is of opinion that 'the separation of the invaginated part, and the preservation of the intestinal channel, occur in one case of every two, or of every three at the utmost.' Ultimate recovery is more likely when the part expelled is small intestine.

It remains that I should say something more respecting *colic*; which may exist independently of enteritis, and of mechanical occlusion of the bowel: although the three are very often combined and intermixed in the course of the same disease.

In colic there is pain in the abdomen, constipation of the bowels, vomiting often: and these are symptoms which occur also in enteritis. The pain is a twisting or wringing pain generally, round the navel: and such is the character of the exacerbations of pain experienced when the bowels are inflamed. These are the points of *resemblance* between the two maladies: and it is of much importance, as I told you before, to observe their *differences*; and to note the marks by which the one may be distinguished from the other. It is, then, an essential difference that enteritis is attended with fever, and with tenderness of the belly. The pain is increased by all kinds and every degree of pressure; and the patient, fixed in the supine position, breathes with the intercostal muscles only, and carefully avoids any movement which would call the abdominal muscles into action, and so compress the inflamed bowel. In colic, on the other hand, the circulation is tranquil; there is no fever; and the pain is even mitigated by pressure. The patient will lie on his belly for ease; nay, he will press it with the whole weight of his body across the back of a chair, and obtain comfort by that expedient. During the paroxysms the pain is often most violent; what the old writers call *dolor atrox*—atrocious pain: but there are intervals of complete ease. Even when the pain is worst, the patient tosses and shifts from one posture to another in search of relief; and he does not wear that anxious and apprehensive aspect which we see in those who are labouring under enteritis. The pain arises, I imagine, from the distention of the bowel, here and there, by gas; or, it may be, from spasm; or from both these states at once. At any rate, it is often associated with audible flatulence, and with evident outward spasm. The abdomen is hard, and drawn spasmodically inwards towards the vertebral column; and its muscles are partially and strongly contracted, gathered up into lumps and knots. None of these circumstances belong to enteritis.

However, there is good reason for believing that, even in mere colic, the pain may sometimes be augmented by pressure. When a portion of the gut has become rapidly distended, considerable uneasiness may result from its forcible compression, although, as Dr. Abercrombie states, the kind of pain can generally, by attention, be distinguished from the sensitive tenderness of an inflamed peritoneum. Luckily, if such pain on pressure should lead us to mistake a case of pure colic for a case of enteritis, the error is on the safe side: and we must always bear in mind the tendency remarked in colic, when the complaint is neglected or badly treated, to run into actual inflammation. In fact, as any obstacle to the passage of the alimentary matters through the bowels may give rise to colic, colic is sometimes merely the first step towards acute inflammation arising out of a continuance of the obstruction. And having told you that colic may be thus produced, I have at once introduced you to a large class of its causes, which have already been spoken of in the present lecture as frequent causes of inflammation also.

But colic, like inflammation of the bowels, sometimes arises without any apparent or detectible obstruction, of a mechanical kind, to the free transit of the contents of the alimentary tube. And there is one particular form of colic that requires a separate notice. The colica *Pictonum*—so called from its great frequency, heretofore, among the Pictones, or inhabitants of Poictou—is produced by the slow introduction of the poison of lead into the body.

Now the colic which has this origin is not to be distinguished, in its ordinary symptoms, from any other kind of colic. But the abdominal pain is usually, in such cases, a part only of more general disease. The disorder has received, in different places, a variety of names. Colica Pictonum; the painter's colic; the Devonshire colic; the bellain of Derbyshire; the dry belly-ache of the West Indies. In all cases it acknowledges the same cause—the gradual entrance of lead into the system.

In this country we see the disease more often in painters than in any other persons. They use, as you know, white lead in the preparation of their colours; and they are perfectly familiar with this terrible colic. We see it also among all workmen whose occupations bring them habitually into contact with preparations of lead. No doubt there are very great differences in the susceptibility of this effect of the poison of lead. Persons have been known to suffer colica Pictonum, in consequence of their sleeping for a night or two in a recently-painted room. On the other hand, I have myself seen a patient who became affected with the disease for the first time, after working with white lead for nineteen years. Dr. Garrod surmises that the gouty diathesis, and great readiness to be affected by lead as a poison, have something in common. One-fourth of his hospital gouty patients have suffered from lead poisoning. Generally the first attacks of colic are well recovered from. The obstinate constipation of the bowels is at length overcome; the patient obtains ease; and forthwith recurs to his previous habits; and after a period, which varies in different individuals, he is again laid up with the colic. Even the primary attacks are usually attended with pains in the head and in the limbs; sometimes with cramps; sometimes even with epilepsy and coma. At length, in one of these attacks of colic, or after one of them, when the violence of the pain, and the costiveness, have yielded to treatment, the patient finds that he has lost the full power of using one or both of his hands. The wrists, as the patients express it, *drop*. You see at once what is the matter, by the characteristic state of the arms and hands. The extensor muscles of the hands and fingers, and the supinator muscles of the forearms, are palsied: so that when the arms are stretched out, the hands hang dangling down by their own weight; and the patient is unable, by any effort of his will, to raise them. The palsy is local; it does not proceed from any diseased condition of the nervous centres. The affected muscles waste; and the atrophy is very remarkably seen in the bundle of muscles composing the ball of the thumb. Even from this condition the patients often are capable of complete recovery. But if they persist in following their former calling—or if, without knowing it, they continue to be habitually exposed to the exciting cause of the disease—they become miserable cripples, lose their power of sleeping, fall into a state of general cachexia, and sink at length under some visceral disease. The poison accumulates in the body, and saps the powers of life. Francis Citois, a native of Poictou, who published an excellent and one of the earliest accounts of the disease, in the year 1617, has drawn the following graphic picture of its effects. Its cause was not at that time suspected. Speaking of the wretched sufferers, he says, 'Per vicos, veluti larvæ, aut arte progredientes statuæ, pallidi, squalidi, macilenti, conspiciuntur; manibus incurvis, et suo pondere pendulis, nec nisi arte ad os et cæteras supernas partes sublatis, et pedibus non suis sed crurum musculis, ad ridiculum ni miserandum incassum compositis, voce clangosâ et streperâ.'

The course of the disease is usually such as I have just described it. The colic happens first, perhaps several times: and then arrives the palsy. But in a few instances I have known this order reversed. The wrists have dropped, when there had been no preceding colic.

The great cause of this fearful malady was first made out by our distinguished countryman, Sir George Baker. He set on foot an inquiry into the origin of what was called the *Devonshire colic*; so common was it in that county. He found, first, that it occurred chiefly in persons who drank the *cyder* manufactured there: and, by degrees, he traced the source of the malady to the admixture of lead with the cyder; either designedly, for the purpose of sweetening it; or by the inadvertent employment of lead in the construction of the cyder mills and vats. It was under circumstances of the same kind that the colic of Poictou originated. Preparations of lead were used—not fraudulently, but openly and honestly—to prevent the wines of the country from turning sour; the injurious influence of lead upon the human body not having then been ascertained. So also, equally convincing proofs of the adulteration of *rum* by means of lead, giving rise to frequent attacks of dry belly-ache in the West Indies, are given by Dr. John Hunter, in the 'Medical Transactions.' I invite your attention to his papers, and to Sir George Baker's on this subject. They afford capital specimens of medical research and reasoning. Various causes, as you may suppose, had been assigned for this disorder. These are one by one investigated, and set aside; until, by this method of exclusion, the real source of all the mischief is detected. Sir George Baker's papers contain a great deal of curious and useful information in respect of the various modes in which this poison of lead may find its way, without being suspected, into the animal economy. The subject is one of vast importance in its relation to medical police; but my limits will not allow me to follow it beyond the point where it ceases to be directly connected with the practice of physic.

Colica Pictonum is seldom fatal as colic; or during the persistence of the abdominal symptoms: yet instances enough of death occurring while the colic was present, but from other accidental causes, have now been collected, to enable us to say, that no appearances have been met with in the intestinal canal, calculated to explain the pain or the constipation. Andral relates five cases, in which the body was carefully examined after death preceded by the painter's colic. He found neither inflammation, nor any remaining trace of spasm. The intestines were neither dilated nor contracted, but of their natural texture and appearance. Merat, who has written a good treatise on this form of colic, gives the dissections in four fatal cases: fatal, as I mentioned that they sometimes are, by the supervention of coma. There was no discoverable mark of disease; the alimentary canal was empty, and the large bowels contracted; as they were also found to be in rabbits which had died of lead colic. For animals are susceptible of the disease, and it may be produced in them by the slow impregnation of their bodies with the specific poison. Dogs, cats, and rats, that inhabit houses and manufactories wherein lead is much used or prepared, are known to be attacked both with colic and with palsy. With respect to the contraction of the large intestines in these cases, we must not be too ready to attribute it to spasm; for the bowel, when empty, is likely to be contracted.

When the palsy has been of any continuance, the affected muscles not only shrink and waste, but undergo a structural change, which is obvious to the sight. They become pale, almost white, dry. John Hunter examined the muscles of the hand and arm of a house-painter, who died, while thus paralytic, in St. George's Hospital. He found them of a cream colour and opaque ; instead of being of a purplish red, and semi-transparent. And since his time, lead has been detected in the palsied muscles, and in the brain, by chemical analysis. It is doubtless conveyed by the blood, to all parts of the body. Why it fastens solely or chiefly on particular muscles, or particular nerves, nobody knows. The pain it occasions, whether in the abdomen or in the limbs, is generally thought to be neuralgic. It is one of the poisons that do not appear to find a ready exit from the body. Very recently, a most curious symptom, pathognomonic, I believe, of the presence of lead in the system, has been pointed out by Dr. Burton : and now that it has been pointed out, one can hardly understand how it escaped discovery so long. It is a blue or purplish line running along the edges of the gums just where they meet the teeth. Dr. Burton first noticed this six years ago, but wisely refrained from making his observations public until he had had time and opportunity enough to satisfy his mind that he was not mistaken. A paper of his on the subject was read at the Medical and Chirurgical Society last January (1840). I cannot resist the temptation to read to you from my notes the substance of a case which has subsequently occurred to me in the Hospital. Mary Anne Davis, a middle-aged woman, presented herself with dropped wrists. It was an excellent example of palsy of the extensor muscles of the hands and fingers. She could raise her arms, but her hands hung down like the talons of a bird, or like the forepaws of an erect kangaroo. This began nine weeks before. She complained of pain, beginning under the nails, stretching up the backs of her hands, and reaching sometimes to the elbows. The bottoms of her feet had also been tender : and at night were burning hot.

Seeing the dropped wrists, we thought immediately of the poison of lead. But the patient was a female. We do not often meet with these effects of that poison in women ; for obvious reasons. At first we could get no clue to the mode in which lead might have found its way into her system. Her husband was a broker. She had not been living in a newly-painted house : and had (she said) no concern with lead in any way. Lead often creeps in, however, through undetected channels, and I could not help suspecting it here. Mr. Pyper, my clinical assistant, soon elicited another part of her history, which added to our suspicions; namely, that before the palsy occurred, she had had pains in the abdomen, and costive bowels, for five days together. Nay, she had a recurrence of colic after her admission. This was a strongly corroborating fact; but what clenched the proof was the discovery of a decided blue rim along the edges of nearly all her gums. This conclusive evidence led to further cross-examination; and at last it came out that some of her sons (she had seven) had occupied their leisure time in the preceding summer with making bird-cages, and painting them green, in the one room in which she habitually lived. The case was altogether a very neat one.

Mr. Tomes has taken advantage of the opportunities furnished by his office of dentist to the hospital, of inquiring into this remarkable phenomenon; and has come to the conclusion that the colour is produced by some chemical action between the tartar that forms on the teeth, where they meet the gums, and the lead which pervades the system. This woman's teeth, like those of many in her rank of life, were loaded with tartar. In one place was visible a stump level with the gum, and surrounded by a ring of tartar; and *there* was also a corresponding border of blue. In other places there were gaps, where teeth once were: here there was, of course, no tartar; and here there was no blue line on the edge of the gum. The presence of tartar is the thing necessary. The teeth are so, only as affording lodgment for the tartar. When the tartar is thoroughly removed from the neck of a tooth, the blue tinge gradually fades from the corresponding gum, while it persists around the teeth upon which the tartar is suffered to remain. Mr. Tomes extracted 'an aching tooth' for a lady who had taken two or three doses of acetate of lead, for the suppression of uterine hæmorrhage. Her gums exhibited the characteristic blue line. Nine days after the removal of the tooth, the gums had come together, and the union was marked by a transverse blue line. At the expiration of three weeks the blue line had wholly disappeared.

Some instances have occurred, under Mr. Tomes' observation, of strongly-marked blue gum, without any other indication of the presence of lead in the system, and without any evidence of the exposure of the patient to the influence of that metal. In fact, he suspects that other metals may sometimes produce a similar discoloration of the gum. Should this hereafter be proved, the diagnostic value of this test of the operation of lead will be somewhat impaired.

'The colouring material is probably sulphuret of lead, or a similar salt of some other metal. Tartar, being very porous, admits into its substance fluids charged with animal matter, which may there be decomposed, and furnish sulphuretted hydrogen. Supposing a salt of lead to be present, a sulphuret of lead would be formed, which would give the colour in question to the tissue in which the formation took place.'

'The saliva itself contains sulpho-cyanic acid, and from this source sulphur might also be furnished.'

This discovery of Dr. Burton's is not a mere piece of curiosity, but is likely to be of use in various ways. In the first place, it may settle the nature and cause of many doubtful cases; as it did, indeed, of the case just narrated; and of another which I have met with since. I was sent for to Ventnor to see a lady who was thought to be dying of cerebral disease. She had arrived at that place from a distant part of the country, in a partially insensible condition; and she soon became completely comatose. A week previously she had had a fit of convulsions, followed by double vision and ocular spectra. The coma had nearly passed away before I saw her. This lady had been ailing for some time. Two things in particular struck Dr. Martin (whom I met in consultation) and myself. She had suffered repeated attacks of abdominal pain and constipation; and the edges of her gums were blue. A faint shade of the same colour extended over the gums a little way below their edges. Further inquiry brought to light the fact that she had been living in a house where all the water used for drinking and for cookery was taken from a certain cistern; and this water, when subjected to chemical tests, was found to be impregnated with lead. Our patient recovered. Again, the poison of lead produces pains which resemble, and no doubt are sometimes mistaken for, the pains of rheumatism; it has other obscure consequences too: and an inspection of the gums may often greatly elucidate such cases. Dr. Burton found that small quantities of lead prescribed as medicine will sometimes produce the phenomenon. In two instances it occurred within two days. One of these patients had taken fifteen grains of the acetate of lead in that time; the other twenty-four grains. Three gouty persons under Dr. Garrod's care took the acetate of lead in three-grain doses three times a day. Three or four days afterwards gouty symptoms were developed. In such persons the blue line soon presents itself, and colic occurs early. A correspondent of Dr. Burton's produced the blue rim in twenty-four hours; viz. by four doses of five grains each, given at intervals of six hours. He found, too, that the line remains distinct after death; even more distinct than during life. It may afford valuable information therefore to the medical jurist in cases of suspected poisoning with the salts of lead. It is, I say, an *early* consequence of the absorption of lead. It may teach us, as the mercurial affection of the gums teaches us, that the medicine is pervading the system; and admonish us to look out for, and guard against, colic. It may apprise workers in lead that their caution has been insufficient; that the poison has entered; and that they are in peril of belly-aches and palsy. It is a capital diagnostic sign also between colic so arising, and colic from other causes; and between colica Pictonum, and the pain of inflammation of the bowels.

In the year 1852, colic became suddenly epidemic among our troops stationed at Newera Ellia, in Ceylon. More than seven-tenths of the whole force were attacked by this complaint, which in seven instances proved fatal. All the sick persons presented the characteristic blue marks. The disorder was traced by Dr. Alexander Smith to the presence of lead in a sample of coarse sugar distributed among the soldiers from one particular estate in the island. There was strong reason to suspect that, in the manufacture of this sugar, acetate of lead had been added to the cane-juice instead of lime. But Dr. Smith shows how the carbonate of that metal might easily get mixed, in small quantities, with the sugar, as it was ordinarily prepared there: plates of lead being laid over some parts of the apparatus employed.

Dr. Smith paid especial attention to the 'blue line' on the gums. He believes that it is produced by the gradual introduction of lead into the system, and by lead *only*. When caused by lead administered medicinally, he has seen it exist for weeks without the supervention of colic. It never appeared under the continued use of sulphate of copper, or of mercury. In all other points Dr. Smith's opinions respecting this marking of the gums coincide with those of Mr. Tomes.

In Dr. Hassall's remarkable book on Adulterations, some striking instances are given of lead poisoning, from a source which might easily be overlooked—namely, from the use of *snuff*, falsified by the admixture of the red oxide, or sometimes of the yellow chromate, of lead.

In a case published by Dr. Johnson, chronic lead poisoning in a portmanteau-maker, who had largely used and handled the black glazed 'overland cloth,' was traced to the abundant presence of lead in the black glaze which covered the cloth.

In the year 1848 some of the members of the Orleans royal family, then residing at Claremont, and several other persons of their household, became subject to severe attacks of colic and of divers nervous disorders, which Dr. Gueneau De Mussy traced to the poison of lead. Of thirty-eight residents at Claremont, thirteen were thus affected. Water had been brought through lead pipes from a distance into a cistern from which the whole household was supplied for all purposes; and each gallon of the water was found to contain a grain of metallic lead.

In the *treatment* of colic—and especially of the lead colic—the great indication is to get the bowels to act. If the pain of the belly be increased by pressure, if the pulse be at all accelerated, if the face be flushed, and there be the slightest approach to fever, it may be right—it can scarcely ever be wrong—to put leeches upon the abdomen. This is a measure of safety as regards the possible existence of inflammation: and if there be no inflammation present or impending, it will tend to remove the spasmodic state of the muscles which goes along with, and perhaps chiefly constitutes, the disease. When there is no ground for suspecting inflammation, external warmth should be applied; diligent friction, with some stimulating liniment; or, what is much better, a mustard poultice, or a turpentine stupe. My colleague, Dr. John Wilson, has been very successful in relieving these patients, by putting them into a hot bath, and having a large quantity of the water in which they are immersed thrown gradually into their bowels by means of a proper syringe. The bath presently becomes polluted, to the great solace and refreshment of the patient. It will generally be expedient to give a full dose of calomel and opium; ten grains of the one with two of the other. Sometimes the effect of the opiate is to suffer the bowels to empty themselves; showing that the previous difficulty was probably spasmodic. Usually the calomel and opium will soothe the vomiting, the restlessness, and the pain; and then a full dose of neutral salts, or of castor-oil, or (if these do not succeed) of the last-named remedy, castor-oil, quickened by one or two drops of the oil of croton, will produce free evacuations from the bowels; and the patient soon returns to his ordinary state of health. It is sometimes necessary to repeat this practice, this alternation of purgatives and anodynes: but when once the bowels have been freely moved, the disease, in general, becomes very tractable.

Although I cannot vouch the fact from my own experience, I must tell you that *alum* has been highly praised for its efficacy in saturnine colic. When given every three or four hours in full doses—from a scruple to a drachm—dissolved in some bland liquid, it is said to allay vomiting, to abate flatulence, to mitigate pain, and to open the bowels, with more certainty than any other drug; and to be often successful when other powerful drugs have failed.

I have mentioned hot baths. The late Dr. Pereira and others have recommended that these should be medicated, by dissolving the sulphuret of potassium in the water, in the proportion of two ounces to fifteen gallons, using a *wooden* vessel. This solution renders brown or black, and innocuous, any portion of lead that may be lying on the skin, or (perhaps) exuding through it. 'The hands, arms, buttocks, and other parts of the bodies of painters, and workmen in white-lead manufactories, are sometimes completely blackened by the bath; but the blackness is readily removed by a brush.' If it be true that the sulphuret of lead is thus repeatedly formed upon the skin in successive baths, the elimination of the poison through that channel cannot be doubted.

At La Charité, in Paris, there is what is called a specific mode of treatment followed. It is complicated and rough, but not a whit more successful than the simpler plan which is universally adopted in this country. You may see it described, if you are curious on the subject, in several of the French books. I think it is given in detail in RATIER'S 'Formulary of Hospital Practice.'

Though the *colic* may have been subdued, the cure of the patient is incomplete so long as any of the poison still lingers within his body; and of this there is often authentic evidence in the blue margin of his gums, and in his palsied muscles. The dropping of the wrists, which renders the patient incapable of earning his livelihood, he justly regards as a more serious evil than the temporary pain and constipation of his bowels. With a view to eradicate the lurking poison, full doses of the iodide of potassium should now be given. You may remember that when I was speaking of the mercurial tremor, I brought under your notice the experiments of M. Melsens, which seem to prove that this salt is a specific remedy for the chronic forms of both mercurial and lead poisoning. The theory of its sanative operation recommends itself by its simplicity. The poisonous substance is in intimate union with one or more of the tissues of the body, and is retained there in some form which is insoluble in the animal fluids. The iodide, carried to all parts by the blood, combines with the metallic poison, and forms a new and soluble compound, which is again taken into the blood, and discharged from the body through its natural emunctories, and especially with the urine. No good opportunity has fallen in my way of trying the iodide in these cases since I became aware of its alleged virtues. It has, however, been put to the test in this country, and its efficacy and mode of action have been fairly established. Thus Dr. Nicholson, of Redditch, relates an instance in which what he names *plumbism* was plainly marked. The patient was a house painter; he had suffered several attacks of colic; his gums showed the characteristic blue line; and the extensors of his wrists were partially paralysed. Before he commenced the treatment, Dr. Nicholson tested this man's urine for lead, and could discover none. Then he administered the iodide of potassium; and after a while he again tried the urine, and his reagents testified that it contained lead. The medicine was continued about six weeks, by which time the patient was well, and his urine had ceased to furnish any evidence of the presence of lead. Dr. Parkes has published a precisely similar case. No better corroboration of M. Melsens' doctrines could well be had, or desired. Dr. Fleming has lately insisted on the necessity and the safety of giving the iodide in large doses, a drachm daily, largely diluted.

For helping to restore to the damaged muscles their lost power, electricity has been thought useful, applied in the way of sparks at first, and of slight shocks afterwards, along the muscular parts of the extensors of the fingers. In the magneto-electric apparatus we have a more accessible, and probably a not less efficacious means, of furnishing the requisite stimulus. It accelerates the recovery to give the hand and fingers the mechanical support of a splint, made for that express purpose, and so contrived that they are kept extended through the greater part of the day.

It is observable of this disease, as of many, and, indeed, of most others, except certain contagious febrile diseases, that when once it has occurred, it is much more liable to occur again, upon a repetition of the exciting cause, than before. It is of very great moment, therefore, that they who are necessarily exposed to the poison of lead—as painters, plumbers, printers (who handle leaden types), colour-grinders, potters, and glass-blowers (who use the oxide of lead in their respective manufactories), shot-makers, workers in lead mines or lead manufactories, and so on—it is of great importance that these persons should be made aware of the means which are best adapted for their protection against the injurious agency of the poison: and we ought to be able to give them advice in that matter. The rules for their guidance are short and simple; and if carefully observed, I believe they will generally prove successful. They resolve themselves into cautions against the admission of the metal, or its compounds, into the body through any channel.

1. To prevent its introduction through the skin minute attention to *cleanliness* is necessary. The face and hands should be washed, the mouth rinsed, and the hair combed, several times in the day; and bathing and ablution of the whole body should be frequently performed: also, the working clothes should not be made of woollen, but of strong compact linen; and they should be washed once or twice a week at least; and they should be worn as little as possible out of the workshop: and some light impervious cap might protect the head while the workman is at his labour.

2. Care should be taken that none of the poison be admitted into the system *with the food*. The workmen, therefore, should not take their meals in the workroom, and should be scrupulous in cleansing their hands and lips before eating.

3. The entrance of the poison into the air-passages during respiration should be guarded against as much as possible. Masks have been recommended for this purpose: none, probably would be more convenient or more effectual, than Mr. Jeffrey's orinasal respirator.

There is a notion prevalent in some places, which apparently has some foundation, that the free use of fat, and of oily substances, as food, is a preservative against the colic. A physician, near Breda, informed Sir George Baker that the village in which he lived contained a great number of potters, among whom he did not witness a single case of lead colic in the course of fifteen years; and he attributed their immunity to their having lived very much on butter and bacon, and other fat kinds of food. De Haen also was told by a physician, the proprietor of a lead mine in Styria, that the labourers there were once very subject to colic and palsy; but that after they were exhorted by a quack doctor to eat a good deal of fat, especially at breakfast, they were exempt from these disorders for three years. This is a kind of prophylaxis that is very easily adopted.

It has been asserted by Liebig that 'the disease called painter's colic is unknown in all manufactories of white lead in which the workmen are accustomed to take, as a preservative, *sulphuric acid lemonade*, a solution of sugar rendered acid by sulphuric acid.'

If this be so, the 'lemonade' must protect the system by converting any other salt of lead, which might find entrance, into an *insoluble* sulphate: solubility being requisite to give efficacy to any poisonous substance.

Mr. Benson, the manager of the British white-lead works in Birmingham, states (in the 'Lancet') that he has tried this method of prevention. Under his direction sulphuric acid was first added to the *treacle-beer*, used as a beverage by the workmen, in the summer of 1841. Lead colic, which had prevailed before 'to a distressing extent,' soon began to diminish in frequency: and from October in the same year, up to the date of Mr. Benson's communication in December, 1842—a period of fifteen months—not a single instance of the disorder had occurred amongst them. This is very encouraging.

LECTURE LXVI.

Diarrhœa. Sporadic, or Summer Cholera. Epidemic Cholera.

The morbid *fluxes* which proceed from the long tract of mucous membrane lying between the stomach and the anus are many in number: and they vary much, both in kind and in cause. *Hæmorrhages* are not uncommon. I have already described the disease called *melæna*, which is characterised by the discharge of black semifluid matters, resembling tar, from the bowels, and in most instances from the stomach also by vomiting. The matters vomited, and the matters passed by stool, are composed principally of blood, which has been rendered black, and otherwise modified in appearance, during its progress outwards in the one direction and in the other. Again, hæmorrhage from the bowels is apt to occur in *typhoid fever*; as I shall show you when we come to that disease. Hæmorrhage takes place also from the rectum in *hæmorrhöis*, or *bleeding piles*: a malady that falls chiefly to the care of the surgeon. Blood comes away, too, mixed with a greater or less quantity of mucus, in *dysentery.*

The remaining forms of profluvia from the intestinal canal I shall proceed to consider *seriatim*; at least the most important of them.

There are several very different affections classed together under the head of *diarrhœa*: by which term is usually signified the occurrence of frequent, loose, or liquid alvine evacuations. Thus diarrhœa is a very common symptom of pulmonary phthisis; and this form of the disorder has been already mentioned. It is very often met with also in typhoid fever, and during the decline of the febrile exanthemata, of which I have yet to speak. Stubborn diarrhœa attends malignant disease of the lower bowel. But diarrhœa is not unfrequently the main symptom of the illness under which the patient labours; and constitutes, at any rate, the chief object of our treatment. I shall touch briefly on some of its varieties.

In the first place, there is that common form of the complaint which results from over-repletion of the stomach; or from the ingestion of food that is not wholesome, food that disagrees (as the phrase is) with the patient's stomach and bowels at that particular time. We may call it, with Cullen, by way of distinction, diarrhœa *crapulosa*; in which fæces are discharged in a more liquid state, and more copiously, and more often, than is natural. These cases are in truth slight cases of irritant poisoning. The ingesta irritate the mucous surface, and probably the muscular coat also; the secretions into the intestines are poured forth in unusual abundance, and the peristaltic motions become more strong and active; the object or upshot of these changes being that of getting rid of the offending substances: a salutary and conservative effort, which we assist and imitate in our *treatment* of this form of diarrhœa.

The symptoms by which this species of diarrhœa is marked must be well known to us all. There are often nausea, flatulence, griping pains in the bowels, succeeded by stools of unnatural appearance and odour, and of fluid or watery consistence. There are often, also, a furred tongue, and a foul breath; but the disorder is attended with little or no fever; the pulse retains its ordinary frequency; and the temperature of the body does not rise.

There are certain things which, more than others, tend when taken into the stomach to cause this crapulous diarrhœa: and there are certain circumstances which increase the disposition to be affected by the ordinary exciting cause.

We frequently see this disorder supervene upon a debauch, in which case the *mixture* of various articles of food, and of drink, each of which by itself might have been perfectly innocent — and the actual *quantity* of the mixed ingesta—have occasioned the irritation and disturbance. But where there has been no intemperance in eating or in drinking, some kinds of food are more likely than others, *cæteris paribus*, to provoke diarrhœa. I do not speak of idiosyncrasies, which show the truth of the old proverb, that what is one man's meat is another man's poison, and which cannot be reckoned upon beforehand; but I refer to the average of systems and stomachs. And among these less-digestible and irritating substances we may place *raw vegetables* of many kinds; such as cucumbers and salads, sundry sorts of fruit, especially if they are hard, immature, and acid; crude apples, plums, melons, pine-apples, nuts, and so forth. Mushrooms, even when cooked, may be added to the list. *Putrid* food, or food which, in the more refined phraseology of gastronomers, is termed *high*, has the same effect upon some persons: and so, in a particular manner, have some kinds of *fish*; shell-fish, crabs, and muscles for instance, in this country: and in other countries, in the West Indies, there are several species of fish which are actually poisonous, and cannot be safely eaten at all. Similar disorder is frequently produced in children by any sort of food, other than the natural sustenance furnished by the mother. The new kind of nutriment disagrees with them: and the very same thing is apt to occur in adult persons. An article of diet which is perfectly wholesome and digestible, and which the stomach bears well after a little habit, will sometimes cause griping and purging, when it is taken for the first time. It is upon this principle that the diarrhœa to which Englishmen are subject upon their first visiting the towns upon the Continent, is to be explained. I do not know that it is so, but I think it very likely that Frenchmen, and Germans, and Italians, suffer in the same way when they first come to this country, and adopt our habits and regimen.

Another curious exciting cause is to be found in certain *mental emotions*, and especially the depressing passions: grief, and above all, fear. A sudden panic will operate on the bowels of some persons as *surely* as a black dose, and much more *speedily*. Among the circumstances which *predispose* most persons to this kind of malady, we may particularly specify *season* —the hot weather of summer and autumn. And it is probably consistent with the experience of most of you, that the atmosphere of the dissecting-room has a similar tendency.

Now this diarrhœa, from occasional irritation, produced by the presence of substances that offend the stomach or bowels, will generally cease of itself. The purging is the natural way of getting rid of the irritant cause. We may *favour* the recovery by diluent drinks, and by making the patient abstain from all further use of food which is not perfectly easy of digestion; and we may often *accelerate* the recovery by sweeping out the alimentary canal by some safe purgative, and then soothing it by an opiate. Or we may give the aperient and the anodyne together, and the one will not interfere with the operation of the other. A table-spoonful of castor-oil, with six or eight minims of laudanum dropped upon it: or from fifteen grains to a scruple of powdered rhubarb, with half as much of the *pulvis cretæ aromaticus cum opio*. By some such medication as this, emptying the bowels, and quieting them, the cure is generally accomplished with ease, and speedily: *tutò, citò, et jucundè.*

We sometimes however meet with cases in which diarrhœa *runs on*: the stools being composed of fæcal matter in an unnaturally fluid state; and the precise condition on which this disposition to an over-loose state of the bowels depends, evading our detection. In these cases it is of importance that the patient should restrain himself to a simple and scanty diet. If the disorder be slight, it will often yield to the astringent and bitter medicines. The infusion of cusparia, with the tincture of cinnamon, may supply a convenient formula. If it be more severe, or obstinate, we have recourse to chalk mixture, which neutralizes acidity; combined with catechu, or with rhatany, which are direct astringents of the tissues; and with laudanum, which calms irritation. And in extreme cases the sulphate of copper has been found to have a powerful effect in restraining the flux. It is apt to gripe, and should be combined therefore with opium. A quarter of a grain of each, in a pill, given three or four times a day, I have frequently found successful, when previous attempts to remove the diarrhœa had failed. Tannin is another substance which is often effectual in arresting chronic diarrhœa; and it is very useful in cases where opium is not well borne. Ten grains of it may be given at intervals of four or six hours.

I have alluded to the influence of *hot weather* in predisposing the system to be affected by the exciting causes of diarrhœa. Dr. Farr remarks that diarrhœa 'is as constantly observed in English towns when the temperature rises above 60°, as bronchitis and catarrh when the temperature falls below 32°.' And there is a complaint—of which diarrhœa is one prominent symptom, but which is something more than mere diarrhœa—that shows itself in this country more or less every autumn, and prevails extensively in some years, as a minor epidemic. It is rightly enough named *cholera*; for it is attended with, and consists mainly of, a remarkable flux of *bile*. Sydenham held that the disease is limited to the month of August; and that bowel affections, with vomiting, occurring at other times, are not genuine cases of cholera. But this was one of that great man's crotchets. The symptoms that mark this complaint are vomiting and purging of liquid matter, deeply tinged with, and principally composed of, bile; violent pains in the stomach and bowels; cramps of the legs and of the abdominal muscles; a great depression of the vital power, and a tendency to syncope or collapse.

The attack is generally sudden. At first the ordinary contents of the alimentary canal are discharged; and then a quantity, an enormous quantity sometimes, of a turbid, yellowish, acrid fluid is expelled with violence both from the bowels, and by vomiting. The patients complain of a burning sensation in the epigastrium. As the vomiting and purging go on, clonic spasms of the lower extremities, and especially of the gastrocnemii, occur; the surface of the belly is drawn up into knots: and after a while, the patient, exhausted by the pain and the spasms, and still more by the copious evacuations, grows cold and faint. Sometimes actual syncope happens: and sometimes death.

Death, however, is an uncommon event of this form of cholera, in this country.

The chief cause of hot weather cholera, such as has now been described, is probably, in great towns at least, the inhalation of the gaseous products of decomposing organic matters, sewer gases, and the like. The actual attack seems to be often *determined* by some of those causes of irritation which I just now mentioned when speaking of simple diarrhœa: and particularly by imprudence in eating and drinking.

I believe that no better *treatment* can be followed in this disease than that long ago laid down by Sydenham. He observes that any attempt to stop the purging and vomiting by strong drastic aperients, under the notion of expelling the irritant matter, would be like endeavouring to extinguish fire by pouring oil upon it; and that to try to lock up the acrid discharges in the alimentary canal by means of narcotics or astringents, would be equally hurtful. He therefore was accustomed to dilute the contents of the stomach and bowels by emollient drinks, and injections, especially by chicken broth; and so to favour their expulsion: and if any faintness or sign of sinking began to show itself, to administer *laudanum* in full doses. We are seldom summoned to these cases in the outset. Generally the vomiting and diarrhœa have continued for some hours before we see the patient; so that it may be expedient to give the opiate without delay. A mild laxative, however, even then, is not only safe but successful often; and the use of opium demands care. If the stomach be very irritable, solid opium in the form of pill may be preferable to laudanum; or an opiate clyster—or an opiate suppository—may be introduced into the rectum. While the diarrhœa lasts, a skim-milk diet should be observed. When the skin is cold, and the pulse sinking or irregular, carbonate of ammonia, or brandy and water, may be given by the mouth: and a mustard poultice, or a bag of hot salt, or a moist and hot flannel sprinkled with oil of turpentine, should be applied to the abdomen. The cramps of the extremities may be relieved by diligent friction with the hand; or some stimulating liniment may be rubbed upon the affected muscles. When the collapse is great the patient should not be allowed to raise himself out of the horizontal posture, lest fatal syncope should follow. The opiate, when opportunely given, sustains the flagging powers, while it quiets the gastro-intestinal irritation.

After an attack of severe cholera, the patient is apt to be left extremely feeble; with soreness of the muscles of the trunk and limbs: and sometimes, symptoms of *inflammation* of the mucous membranes will supervene; pain and tenderness of the belly, a white tongue, thirst, and fever. And these symptoms may require some of the *remedies* of inflammation.

Such is the disease which had long been familiar to English practitioners as *cholera*: but about the end of the first third-part of the present century, this country was visited by a severe epidemic disorder, which was also called cholera; or by way of emphasis, *the* cholera; or sometimes *spasmodic* cholera; or *Asiatic* cholera; or *malignant* cholera. The symptoms of this new disease resembled, in some points, those of the old-fashioned cholera: but differed from them in more points, and in points of more importance. So that the application of the term cholera, or cholera morbus, to both these morbid conditions, is greatly to be regretted, for it has produced very much confusion and inconvenience.

I scarcely know how to name the newer and severer disorder. I have no right to alter the received nomenclature; and choosing from among the many appellations which have been given to the complaint, that epithet which seems the least objectionable, I might call it *epidemic* cholera: although this term is not unobjectionable, since the other malady, to which Dr. Farr has given the appropriate name of *summer* cholera, is sometimes also epidemic. In the 'Nomenclature' the terms adopted are respectively malignant cholera and simple cholera.

The *epidemic* cholera so far resembled the *summer* cholera, that it was attended by profuse vomiting and purging, by extreme prostration of strength, and by cramps. But it differed remarkably in *these* respects; in the circumstance that the matters ejected from the stomach and bowels during the acme of the disorder contained no bile (and this alone is a good reason against calling the disease *cholera*); in the early supervention of the symptoms of collapse; and in the great mortality of the disorder.

The amount of the fluid matters thrown up from the stomach and discharged by the bowels was really in many cases wonderful. At first, perhaps, the patient would have so copious a stool—a consistent dejection it might be, but so large in quantity—as to lead him to think that the whole contents of the intestines had been discharged at once. Yet soon afterwards a turbid whitish liquid would again and again pour from his bowels in streams, and be spouted from his mouth as if from a pump: not in general with pain or much effort, but easily and abundantly. The matters thus discharged were thin, and for the most part of a whitish colour, like water in which rice has been boiled; having a very peculiar odour, but without *fæcal* smell; and containing small white albuminous flakes. There were some varieties in the evacuations, but the kind I have mentioned, resembling rice-water, was the most common and the most characteristic: and however *else* their sensible qualities might vary, *this* circumstance was universal, that they contained no *bile*.

With all this there was early sinking, and collapse, as it was called. This term *collapse* expressed a general condition, made up, in the most exquisite cases, of the following particulars:—A remarkable change took place in the circulation, and a striking alteration in the appearance of the patient. The pulse became frequent, *very* small and feeble, and at last, even for hours sometimes, extinct at the wrists. The surface grew cold; and in most, or in many instances, blue as well as cold. The lips were purple; the tongue was of the colour of lead, and sensibly and unpleasantly cold to one's touch, like a frog's belly; and the breath could be felt to be cold. With this coldness and blueness there was a manifest shrinking and diminution of the bulk of the body. The eyes appeared sunk deep in their sockets; the cheeks fallen: in short, the countenance became as withered and ghastly as that of a corpse. The cadaverous aspect that sometimes precedes death in long-standing diseases would come on in the course of an hour or two in this complaint. If the physician left his patient for half an hour, he found him visibly thinner on his return. The finger nails became blue; the hands and fingers shrivelled, white, corrugated, and sodden, like those of a washerwoman's after a long day's work. The skin was bathed in a cold sweat.

The voice became husky and faint. So peculiar was this change, that the sound was spoken of as the *vox cholerica.* These are the symptoms which the single word *collapse* was meant to express.

Another very striking feature of the disorder was the muscular cramp; affecting the muscles of the thighs and calves of the legs, rendering them as hard and rigid as wood; and drawing up into knots the muscles of the abdomen. These spasmodic contractions were attended with severe pain, and constituted the greater part of the patient's suffering. During the continuance of the symptoms that I have been endeavouring to describe, not a drop of urine was passed or secreted. One man who was under my own observation and care, and who recovered, did not void a drop of water from Sunday morning till the afternoon of the following Wednesday.

Even in the extreme state of collapse the intellect remained quite clear: the patients would continue to talk rationally to the last moment of their lives; and, for the most part, they seemed singularly indifferent and apathetic about their condition.

In the fatal cases—and a very fearful proportion of the whole number *were* fatal—death took place sometimes in the course of two or three hours; and it was seldom delayed beyond twelve or fifteen. In those that recovered, the favourable symptoms were the cessation of the vomiting, purging, and cramps; the return of the pulse, of the voice, and of warmth to the surface; the disappearance of the blueness of the skin, and of the hippocratic countenance; the *re*appearance of bile in the alvine evacuations; and the restoration of the secretion of urine. These last two points of improvement were of most promising augury.

The course of the symptoms varied a good deal in different persons. Sometimes the vomiting and purging soon ceased, and sometimes there was neither sickness nor diarrhœa at all, but rapid collapse and sinking. These were deemed to be the most formidable cases. However, the peculiar secretions were *poured forth*, in some, at least, of the instances in which none of them were *ejected from the body.* A patient died of cholera in the Middlesex Hospital without any vomiting or purging: but on examining the dead body, we found the intestines quite full of the rice-water serous fluid. Sometimes the cramps were not very troublesome. The cutaneous blueness was not a universal phenomenon. The patients were in general tormented by thirst: and when attempts were made to bleed them, the blood was found dark and thick, like treacle, and scarcely moving, if moving at all, in the veins: in some cases it could not be made to flow out. Considerable hurry and anxiety of the breathing were also symptoms that I omitted to mention before.

Examination of the dead bodies threw no clear light upon the nature of this frightful disease. The alimentary canal generally was found to contain a white liquid, having whiter flakes in it; such as had previously issued from the bowels: and the mucous glands of the intestines, both the solitary and the agminated, were unusually large and conspicuous. The veins were loaded with thick, black, tar-like blood; and the urinary bladder was always found empty, and contracted into the size of a walnut. Even when the blue colour had existed in a marked degree during life, it often quickly disappeared after death. And another most singular phenomenon was occasionally remarked in the dead body. A quarter, or half an hour, or even longer, after the breathing had ceased, and all other signs of animation had departed, slight, tremulous, spasmodic twitchings and quiverings, and vermicular motions of the muscles would take place; and even distinct movements of the limbs, in consequence of these spasms.

The disease, of which I have drawn but a faint outline, was not known in this country till the autumn of the year 1831. There are persons, I am aware, who hold that it has always existed among us; only not in such numerous instances as at that period; and they appeal to Morton, and other early writers on the diseases of this country, in support of their opinion. But the malady was too striking to be overlooked, or ever forgotten, by anyone who had once seen it. Certainly, till that year, I never saw anything like it. To be sure I had not at that time been very many years in practice here. The late Dr. William Babington, however, told me that it was quite new to *him*. He had, for a very long period, been in extensive business, in those parts of the metropolis and its vicinity where the epidemic cholera raged most; and when it first came among us he had the curiosity to ask every medical man whom he met whether he had seen any case of the cholera; and if the answer were '*yes*,' he went on to inquire whether, before that year, the person had ever met with the same complaint; and the reply was always, without a single exception, '*no*.' Yet I say there were, and are, a few practitioners who denied, and deny, that it was anything more than the common and well-known English complaint, raging with unusual frequency and violence.

But we have evidence of a different kind of the newness of the epidemic cholera to these kingdoms. Its approach was discerned afar off, as distinctly as a storm is foreseen by the rising of the clouds from the horizon in the direction of the wind. The disorder began to rage with terrible severity, in the Delta of the Ganges, in the year 1817. I do not mean that it then broke out there for the first time. It had again and again desolated those regions before. But from its irruption in the year I have mentioned, when it committed frightful devastation in our armies in the north-eastern districts of India, its course can be distinctly traced to our own shores; towards which it approached with slow and halting, but with sure steps, in a north-western direction. From India it spread to Persia; from thence to Russia; and across through Poland to Germany: and at length it was found at Hamburg. It was predicted before that time that the distemper *would* at length reach Great Britain. Our government had even sent two physicians into Russia to meet it, and to investigate its nature, in the fearful anticipation that its march across the earth would continue progressive; and accordingly at the expiration of fourteen years, it made its appearance on the *eastern coast* of this country, in Sunderland, and in due time extended over every part of these islands. I say its arrival had been *foreseen* and *foretold*; and it is absurd to suppose that a vast number of persons would fall sick, and die, with symptoms quite strange to the great mass of practitioners here, merely to fulfil this prediction.

The progress of the disorder did not end here. Crossing the Atlantic, it invaded America; turning, at the same time, in a south-easterly direction, it ravaged France and Spain, and the north coast of Africa, and Italy.

Moving thus onward, as it did, in defiance of all natural or artificial barriers, under opposite extremes of season, temperature, and climate, in the teeth of adverse winds, over lofty mountain chains, across wide seas, through 'hot, cold, moist and dry'—in what manner, you will naturally ask, was this wasting pestilence *propagated*?

The first thought which occurs to one's mind is that man also, and man alone, so far as we perceive, overcomes all these kinds of hindrances to his locomotion, and therefore that the exciting cause of cholera in all probability accompanies and depends upon the journeying of human beings upon the earth.

Upon this question various and discordant opinions are entertained. Many persons believe that the complaint spread by contagion: more, however, that it was not contagious at all, but arose from some deleterious cause with which the general atmosphere of the place was pregnant. Now I cannot reconcile the phenomena of the appearance and extension of the malady with either of these hypotheses, *exclusively*. It must, I think, be granted that the complaint, in every instance, was excited by the application of some noxious material to the body, some positive poison. It is certain, also, whichever hypothesis may be chosen, that many more individuals came near the agency of this poison than were injuriously affected by it. This exemption from the disease no more invalidates the doctrine of contagion than it invalidates the doctrine of some diffused atmospheric influence: nay, it is more explicable upon the former than upon the latter supposition; for while many may avoid a specific contagion, all are immersed in, and all breathe, the common atmosphere. But the exemption shows *this*: that the exciting cause, to be effective, required a fit recipient; that the susceptibility of being hurt by the poison in its ordinary dose, intensity, and modes of application, varied much in different persons; and in the majority was very faint, or wanting. It is clear that the poison travelled. It is equally clear to my mind, that it was *portable*; and therefore communicable, directly or indirectly, from person to person. I even believe that it was capable of being conveyed, and was actually conveyed, from one spot to another, by persons who were themselves proof against its effects; or who, at any rate, were unaffected by it. The innumerable authentic instances of coincidence, in point of time, between the first outbreak of the disorder in a particular place, and the arrival at that place of some person or persons from an infected locality, prove that the poison could be thus carried. Of this direct importation of the disorder into new and distant places, by infected individuals, and of its subsequent extension from those individuals to others who had intercourse with them, you may see a vast number of examples collected by Dr. James Simpson, in the forty-ninth volume of the 'Edinburgh Medical and Surgical Journal.' The evidence there adduced of the portability of the poison is abundant, and to my mind irresistible. Between the two epidemic distempers, influenza and cholera, there were numerous and striking points of similitude and analogy. They have observed the same, or very nearly the same, geographical route. Both, issuing from their cradle in the east, have traversed the northern countries of Europe, till, arriving at its western boundary, they have divided into two great branches; the one proceeding onwards, across the Atlantic, the other turning in a retrograde direction towards the south and east. The main differences between them have been, that whereas the poison of influenza spared very few of the community, inflicting a disease which, of itself, was seldom fatal—the poison of cholera, on the contrary, smote very few, but with so deadly a stroke that as many sank beneath it, probably, as recovered. Both were *general* disorders, affecting the whole system, but in both the most prominent of the symptoms had reference, in the majority of cases, to the mucous membranes: to those of the air-passages in the influenza; to those of the alimentary passages in the cholera.

Now, this strong analogy has been made use of as an argument that the cholera was not contagious. 'The influenza (say the objectors) had no contagious properties; therefore it is, *à priori*, likely that the cholera had none.' But I demur to the major proposition. Cullen thought the influenza *was* contagious, and I adverted, in a former lecture, to some facts which favour that belief. Supposing it, however, to be so, the proof of its contagious property must, from the very nature of the case, be extremely difficult. Its visitations are so rapid, widely spread, and multitudinous, that there is no time for its transference from house to house, or from person to person; yet it may nevertheless be transferable. Its inherent rate of locomotion outstrips and precludes the tardier conveyance of the poison by man. Its contagious qualities (granting them to exist) are hidden in its universality, and can seldom be traced but by accident. I therefore esteem this argument from analogy as worthless; and my own creed respecting the cholera is, that it *was* contagious; but that its contagious power was not very distinctly marked, and that comparatively few persons, in this country at least, were susceptible of its operation, except under circumstances of predisposition. At the same time, I believe that a great majority of the cases of cholera were not attributable to direct contagion, but to the poison diffused through the atmosphere, or contained in the water used for drinking. There is nothing inconsistent in the supposition that this noxious matter travelled sometimes by virtue of its own peculiar qualities, sometimes made use of vehicles.

Whatever obscurity may overhang the *exciting* causes of the epidemic cholera, we are quite sure that certain circumstances exercised a strong *predisposing* influence upon the human body, to render it more than usually susceptible of the disease. The predisposing causes, as might well be imagined, were such as tended to debilitate the system: and therefore *poverty*, which implies scanty nourishment, and frequently also the confinement of several persons to a narrow space, and the want of fresh air; poverty which includes these and other evils, was found to predispose the body to a ready reception of the malady. But to *intemperance*, more than to any other *single* cause, may the proclivity to become affected by this species of cholera be ascribed; and especially to the intemperate and habitual use of distilled spirits. This fact was peculiarly manifested in the selection, by the disease, of its victims in this country; and it has been remarked almost everywhere else. Nevertheless, men the most sound and vigorous were liable to take and to succumb to the disorder.

The epidemic cholera made its attack in two different modes. In one it seized upon the patient suddenly, and without warning. This was comparatively rare. Much more commonly the specific symptoms were preceded, for some little time, even for some days perhaps, by painless diarrhœa. And this I take to have been the most important practical fact that was ascertained during its first prevalence among us. When the disease was once fairly formed, medicine had very little power over it; but in the preliminary stage of diarrhœa it was more easily manageable. Unfortunately people are inclined (especially those classes of the community among whom the cholera most raged) to regard a loose state of the bowels as salutary: and to make no complaint of it, and to do nothing for it. The proper plan of proceeding, I am convinced, was, to arrest the diarrhœa as soon as possible after its commencement: by what means I shall consider presently.

But when the specific symptoms, peculiar to the severe form of cholera, had set in, medicine, I repeat, had very little influence upon it: and accordingly, as might have been expected, a hundred different cures of the disease were announced, most of them all but infallible. Some persons held that timely bleeding would save the patient: others relied confidently upon mustard emetics. Hot air baths were manufactured and sold to a great extent, to meet the apprehended attack in that manner without delay. Certain practitioners maintained that the disease was to be remedied by introducing into the system a large quantity of neutral salts, which were to liquefy and redden the blood, and to restore the functions of the circulation. But of this practice it was said in a sorry but true jest, that, however it might be with pigs and herrings, *salting* a patient in cholera was not always the same thing as *curing* him. In a great number of the sick the blood was mechanically diluted by pouring warm water, or salt and water, into their veins. Some physicians put their trust in brandy, some in opium, some in cajeput oil, which rose to I know not what price in the market; some, again, in calomel alone.

Now, I would not willingly mislead or deceive you on this point, by speaking with a confidence for which I really have no warrant, of the success or propriety of any of these expedients. I believe that each in some cases did good, or *seemed* to do so; but I cannot doubt that some of them did sometimes also do harm. I had not more than six severe cases under my own charge; and I congratulated myself that the mortality among them was not greater than the average mortality. Three died, and three (I will not say were cured, but) recovered. The three that died I was called in to see when the disorder was at its height: in each case it went on with frightful rapidity, in spite of all the means adopted, and proved fatal a few hours afterwards. The three that recovered I saw somewhat earlier, but still not till the specific symptoms were present: one was a girl in the hospital. They all recovered under large and repeated doses of calomel. Yet (as I said before) I do not venture to affirm that the calomel cured them. In the first case which was treated in that way, I merely followed up the plan that had been begun by Dr. Latham, who had visited the patient for me when I was accidentally absent. I found that he had felt better, less sick and less faint, after taking half a drachm of calomel at a dose; and I repeated the same dose many times, for after every dose his pulse rose somewhat, and he appeared to rally. This was the same man whom I mentioned before as having made no urine from the Sunday to the Wednesday: all that time he kept discharging rice-water stools. At last, on the fourth day, he passed a *little* water, and his alvine evacuations became rather more consistent, and began to look *green*: and from that time he gradually got well. Afterwards I treated my hospital patient in the same way, and with the same event. Yet I will not pretend to say that these persons might not have done quite as well if they had been left entirely to themselves.

Some of the expedients recommended had certainly a very marked and immediate effect upon the condition of the patients, especially the injection of warm water into their veins. Many instances of this were related at the time. One I myself saw. The patient was a young man, who was nearly moribund apparently. His pulse had almost, if not quite, disappeared from the wrist; his voice was faint and husky; he was very blue, and his visage was ghastly and cadaverous: in one word, he was in an extreme state of collapse. Out of this he was brought in a few minutes by injecting warm water into one of the veins of his arm. The pulse again became distinct and full; and he sat up, and looked once more like one alive, and spoke in a strong voice. But he soon relapsed; and a repetition of the injection again rallied him, but not so thoroughly: and in the end he sank irretrievably. Dr. William Babington told me of a patient whom he saw, speechless, and all but dead, and whose veins were injected. He then recovered so as to sit up, and talk, and even to joke, with the bystanders: but this amendment did not last either. Yet even this temporary recovery might be sometimes of great importance: might allow a dying man to execute a will, for example. And some of the persons thus revived got ultimately well. We had for some time a woman in the Middlesex Hospital acting as a nurse, who had been rescued, when at the verge of death in cholera, by the injection of warm water into her veins.

It was remarked of those who recovered, that some got well rapidly, and at once; while others passed into a state of febrile reaction, which frequently proved fatal some time after the violent and peculiar symptoms had ceased. Some, after the vomiting and purging and cramps had departed, died comatose; *over-drugged* sometimes, it is to be feared, by opium. The rude discipline to which they were subjected might account for some of the cases of fever. And the process of artificially replenishing the veins was certainly attended with much danger. The injection of *air* with the water—inflammation of the vein from the violence done to it—an over-repletion and distention of the vessels by the liquid—*might*, any one of them, and sometimes, I suppose, *did*, occasion the death of a patient. Never, certainly, was the artillery of medicine more vigorously plied—never were her troops, regular and volunteer, more meritoriously active. To many patients, no doubt, this busy interference made all the difference between life and death. But if the balance could be fairly struck, and the exact truth ascertained, I question whether we should find that the aggregate mortality from cholera, in this country, was any way disturbed by our craft. Just as many, though not, perhaps, the very same individuals, would, probably, have survived had no medication whatever been practised.

Since the foregoing lecture was written and delivered, the pestilence of cholera has twice revisited this country. I suffer the lecture to remain unaltered, because I still believe it to present, so far as it goes, a faithful outline of that terrible disorder in its intrinsic features and in its external relations.

In their general course and character, the three epidemics of 1831–32, of 1848–49, and of 1853–54 manifested a strong mutual resemblance. Each had a period of invasion—then a pause—and then again a subsequent fiercer outbreak, determined apparently by atmospheric conditions. All three fell with unequal severity upon different parts of the kingdom; and the parts which suffered the most, and the parts which suffered the least, were, with few exceptions, the same in them all. In each the absolute mortality from cholera was the highest in the months of August and September. More persons died of it in 1849 than in 1832, fewer in 1854 than in 1849. To give you some notion of the desolating power of the disease, I may tell you that during its second visitation there died in England of cholera and diarrhœa upwards of seventy thousand persons. I couple the two advisedly. Each of the three cholera epidemics was preceded and accompanied by diarrhœa, which was unusually fatal as well as unusually frequent. There can now be no doubt that the excess of diarrhœa above the average of common years was partly due to the exciting cause of cholera; or rather, that many of the cases registered under the head of diarrhœa were really cases of cholera, running a comparatively slow course, and shorn of its more striking symptoms. This fatal choleraic diarrhœa occurred most often at the two extremes of life, while the deaths from fully-developed cholera were most numerous in its middle period. It appears from the statistical tables published under the authority of the Registrar-General, that the average duration of fatal diarrhœa was about sixteen days, of summer cholera about five days, of epidemic cholera about two days.

A fourth great visitation of cholera—that of 1865–66—has come and gone since I last addressed you on this subject, and it has been far more fertile of instruction on many interesting points relative to the disease than any of the three preceding epidemics.

Very few, I imagine, of the original doubters remain unconverted to the doctrine which I have held from the beginning, that epidemic cholera is *catching*: that it results from a material poison which is portable, capable of being conveyed from place to place, and communicated from person to person —or from inanimate substances to which it clings, such as articles of furniture, or clothing. That the morbific matter floats also in the air, and may be wafted about by its currents, is a general and well-founded belief. I think, with Dr. Baly, that when it travels over great distances, as from one country or region to another, it uses the vehicle of human intercourse; but that it may be and often is diffused over smaller spaces, as from one part of a town to another, or from a tainted port to a ship anchored to leeward, by the movements of the atmosphere. The long migrations of the disease are not made rapidly. Its rate of progress never exceeds, and is often slower than, that of modern travelling. Its primary appearance in an island or a kingdom is always at its outer boundary. In our own country, for example, it first planted its foot in a seaport town on the *east* coast, over against the mainland where cholera was raging, and whence ships had very recently arrived. The same is true of its subsequent visitations. On the other hand, the crews of vessels sailing from healthy places remain free from the disease until they have entered an infected port, or held intercourse with an infected shore.

In his statistical report of the Royal Navy, published in 1858, Dr. Bryson says:—'The medical records of the (naval) service have been searched in vain to discover an instance in which either cholera morbus or yellow fever made its appearance amongst a ship's company, unless one or more of the men or officers had previously—within at most twenty-one days—been exposed in some house, ship, or locality where the infectious virus which emanates from persons ill of the one or the other of these diseases existed. The spontaneous origin of either malady, far away from an infected locality, is unknown in the naval service.'

That the atmosphere forms one vehicle of infection seems clearly proved by some incidents ascertained respecting the last epidemic before it struck this country. I copy them from the 'Times' newspaper for October 15 or 16, 1865:—'Five miles from Gibraltar stands the little town of San Roque; and San Roque and Gibraltar were abruptly smitten by the plague, not only on the same day, but almost at the same moment. At Gibraltar it was a sudden access of the malady; at San Roque a first outbreak. At a small town near Toulon the plague fell upon the place in the night; and thirty cases occurred simultaneously between evening and morning.' (This, let me observe in passing, might *possibly*, though not probably, have happened from the use of drinking water as a vehicle of the poison.) 'At Constantinople it was observed that, while the cholera was raging, all the seagulls which used to flit over the waters of the Bosphorus deserted the place, nor did they reappear till the disease had departed and the atmosphere became pure once more.'

Compare this with an extract from the 'Dublin Morning Register' respecting the first epidemic—that of 1832:—

'A Westport correspondent, upon whose veracity we place reliance, has communicated to us the following extraordinary fact:—In the demesne of the Marquis of Sligo, near Westport House, there is one of the largest rookeries in the west of Ireland. On the first or second day of the appearance of cholera in this place, I was astonished to observe that all the rooks had disappeared; and for three weeks, during which the disease raged violently, these noisy tenants of the trees completely deserted their lofty habitations. In the meantime the revenue police found immense numbers of them lying dead upon the shore near Erris, about ten miles distant. Upon the decline of the malady within the last few days, several of the old birds have again appeared in the neighbourhood of the rookery; but some of them seemed unable, from exhaustion, to reach their nests. The number of birds now in the rookery is not a sixth of what it was three months ago.'

A striking proof that the air may be a vehicle of infection—that the poison may enter the lungs with the breath—is furnished by the fact that two pilots took the disease in consequence of having their open boat towed by a ten-fathom rope at a considerable distance astern of the steamship *England*, on board of which cholera was raging. They were never on board the vessel. Both of them had cholera, and one of them died of it. Both took the disease home, and transmitted it to their families, near Halifax, where the disease had been unknown for many years.

But although the infection thus proceeding from the bodies or the excretions of the sick, and entering by the lungs the bodies of the healthy, may strike and destroy individuals here and there, it seems very doubtful whether the disorder can become epidemic, except in certain conditions of the atmosphere.

It appears from the extremely interesting report of Mr. Glaisher on this subject, that 'the first three epidemics were attended with a particular state of atmosphere, characterised by a prevalent mist' (he is speaking of London and its immediate neighbourhood)—'thin in high places, dense in low. During the height of the epidemic in all cases, the reading of the barometer was remarkably high, and the atmosphere thick. In 1849 and 1854 the temperature was above its average, and a total absence of rain, and a stillness of air amounting almost to calm, accompanied the progress of the disease on each occasion. In places near the river the night temperatures were high, with small diurnal range.' He goes on to enumerate, as characteristic of the atmosphere at these periods, 'a dense torpid mist; and air charged with the many impurities arising from the exhalations of the river and adjoining marshes; a deficiency of electricity; and (as shown in 1854) a total absence of ozone, most probably destroyed by the decomposition of the organic matter with which the air in these situations is strongly charged.'

The ozone here mentioned is endowed, as I told you formerly, with peculiar purifying properties. It has a high oxidising power, in virtue of which it unites with, decomposes, and so destroys miasmata, while it is at the same time itself proportionately destroyed. There is no ground for ascribing cholera, as some have done, to the absence of ozone—except in the sense of there not having been a sufficient quantity of it in the atmosphere to counteract all the poisonous miasm which actually produces that disease. The total absence of ozone affords presumptive evidence of the presence of atmospheric impurities.

A remarkable law of *altitude*, that is of elevation above the level of the Thames, has been announced by Dr. Farr as governing the mortality from cholera in this metropolis: and if here, so doubtless, under similar circumstances, elsewhere. 'The elevation,' he says, 'of the soil in London has a more constant relation with the mortality from cholera, than any other known element.' The mortality is inversely as the altitude.

This law of altitude—so important and so practically valuable—is but an expression of the result of many concurrent circumstances. The material poison of cholera will be likely to gravitate, as the marsh poison gravitates, with which it has many points of analogy, to the lowest part of the atmosphere; where the high barometrical pressure is the greatest, and vaporous diffusion therefore the least; where unwholesome exhalations from the soil and from the water are the most abundant; where the dispersing and diluting influence of winds is least felt. Indeed the air may be completely stagnant while on the neighbouring heights a brisk breeze is blowing. The lower regions of the atmosphere are the hotter also as well as the moister; and under the agency of a high temperature the organic impurity with which the air is charged runs more readily into decomposition. The inverse law of altitude is therefore an intelligible law. We see also how it may sometimes be disturbed or broken, under exceptional circumstances.

At the time when Mr. Glaisher's observations were made, the river Thames had become, without metaphor, the common sewer of this enormous and ever-growing town. Foul with the daily and hourly influx of abominable filth, it was offensive to the senses, and a cause of added foulness to the incumbent atmosphere. When we learn from Mr. Glaisher that during the summer months the night temperature of the river is considerably above the minimum temperature of the air, and that its vast area was simmering all night long, and throwing off clouds of noisome and noxious vapour, we need be at no loss to account for the special unhealthiness of those quarters of the town which lie nearest to its banks.

But however unwholesome and pernicious the atmosphere may thus become, it cannot generate cholera, unless the specific exciting poison of that disorder be present also. In the autumn of 1859 the Thames stank horribly; yet we had no cholera. On the other hand, there is good reason, I say, to believe that this poison can never create a spreading pestilence, unless it meets with a congenial atmosphere. The foul air lends force and diffusion to the poison, and aids, or causes, its increase.

Notwithstanding that the choleraic poison in an invisible and impalpable state may thus pervade, and be communicated through, the air, it had long been conjectured, and it is now perfectly certain, that (horrible thought) we may eat and drink the poison, and so obtain the disorder. That, as I shall have to tell you, is the case also with enteric fever; the discharges from the alimentary canal are at once the main outlet for the poison and the chief source of infection. The late Dr. Snow was the first to broach the notion that the poison may be *swallowed* with the food which we eat, or the water which we drink; and that its multiplication takes place within the system, whence, by the alimentary canal, a new and abundant stock of it is voided. He showed how easily portions of the rice-water excretions, colourless and but slightly odorous as they are, may without our notice come to adhere to our food during its preparation, or its consumption. And the horribly disgusting fact had been made too certain by the unchallengeable disclosures of the microscope, that the water which is supplied by the several water companies for domestic purposes to this great city habitually contained visible particles of human ordure. Some striking facts had been collected by Dr. Snow, which warranted the presumption that a most fearful outbreak of cholera in Soho was attributable to the water of a certain pump, contaminated from a neighbouring sewer. A remarkable converse fact had been reported by the late Sir William Lawrence. Bethlem Hospital, and an asylum for children called the House of Occupation, stand near together on an open space of ground between fourteen and sixteen acres in extent, lying in the parish of St. George, Southwark. Being dissatisfied with the filthy water then supplied by the Lambeth Company, the Governors some forty years ago sank Artesian wells on the premises, and the pure water thus procured is used exclusively in the two institutions, which between them number about seven hundred residents. There was not a single case of cholera in the Hospital or in the House of Occupation in any of the first three epidemics; although the disease prevailed extensively in the parish, and in the streets in their immediate vicinity.

The result of an inquiry suggested by the Board of Health into the effects of the consumption of impure water during the second and third cholera epidemics was favourable to Dr. Snow's theory. Mr. Simon reported that 'the population drinking dirty water appeared to have suffered 3½ times as much mortality as the population drinking other water.'

That cholera may indeed be contracted by drinking a mixture of choleraic discharges and water, is demonstrated with all the force, if not with the reality, of an experiment, by the facts thus stated by Mr. Macnamara, a gentleman practising in India:—'I may mention the circumstances of a case in which the most positive evidence exists as to the fact of fresh cholera dejecta having found their way into a vessel of drinking water, the mixture being exposed to the heat of the sun during the day. Early the following morning a small quantity of this water was swallowed by nineteen persons. (When partaken of the liquid attracted no attention either by its appearance, taste, or smell.) They all remained perfectly well during the day, ate, drank, went to bed, and slept as usual. One of them, waking next morning, was seized with cholera; the remainder of the party passed through the second day perfectly well, but two more of them were attacked with cholera the next morning; all the others continued in good health till sunrise of the third day, when two more cases of cholera occurred. This was the last of the disease; the other fourteen men escaped absolutely free from diarrhœa, cholera, or the slightest malaise.'

At the time of this remarkable occurrence there was no cholera in the neighbourhood, nor had there been for several years, nor, so far as Mr. Macnamara is aware, has there been since.

Mark here the period of incubation, varying from twenty-four hours to two or three days; mark also that the majority of those who drank the tainted water escaped unhurt; in other words, that some persons take the complaint more readily than others.

The epidemic of 1865–66 has illustrated in a very remarkable way the soundness of Dr. Snow's theory. The prevalence of the epidemic in this country was clearly a step forwards in the progress of the malady in its rapid advance from Mecca to Egypt, and thence to various places on the eastern and southern coasts of Europe and in the basin of the Mediterranean. Mr. Simon lays it down as an axiom to be generally accepted in State Medicine, that 'contagions current on the continent of Europe must be deemed virtually current in England.' The disorder first showed itself here in the autumn of 1865, as usual at a seaport—Southampton. Then, as usual, it slept apparently for a while, to reappear and diffuse itself, after fresh importations from the Continent, and at its customary rate of increase, in the spring and summer of 1866; when, in the middle of July, there occurred in the eastern part of London an increase of the disorder so sudden, vast, and rapid as to warrant its being spoken of as an explosion. This outburst was limited to a certain definite and remarkable area, the line of limitation having an obvious relation, not to *soils*, but to *houses*; it was contemporaneous over that area, and stopped short abruptly within and along the line of limitation. It had a week's duration only. Its cause began to act during the week ending on July 14, and ceased to act in the week following. On the sixth day of its increased activity cholera had appeared in every portion of the before-mentioned area; the rate of its increase, as compared with the previous week, was nearly seven times greater than in the rest of the metropolis; while in the subsequent week the rate of increase became virtually the same over the whole of London. It is worthy of remark that there had been no undue prevalence of diarrhœa in the affected area.

This strange and definite outbreak must have had some adequate and definite cause; and, upon careful search, there was found evidence only just short of demonstrative proof—evidence which I cannot stop to state in detail, but which you may study in the ninth Report of the Medical Officer of the Privy Council—that this local calamity was produced by the temporary distribution to the area in question of unfiltered and infected water from certain reservoirs of the East London Water Company.

The peculiar blue mist which was noticed in the epidemic of 1854 was present also in the last epidemic. Mr. Glaisher says of it:—'On some days no trace of the mist has been visible; on other days it has been seen for parts of the day only. It has extended from Aberdeen to the Isle of Wight. This mist increased in intensity when viewed through a telescope; usually no mist can be seen when thus viewed.' As in previous epidemics, there was a marked deficiency of ozone in the atmosphere. In other respects the meteorological phenomena were in remarkable contrast with those which had occurred during previous visitations of the cholera, and the law of altitude was broken by the predominance of more powerful influences.

With respect to the mode of propagation of the disease, Mr. Simon uses this strong language:—'It cannot be too distinctly understood that the person who contracts cholera in this country is *ipso facto* demonstrated with almost absolute certainty to have been exposed to excremental pollution. Excrement-sodden earth, excrement-reeking air, excrement-tainted water—these are for us the causes of cholera.'

He adds: 'The local conditions of safety are, above all, these two: (1) that, by appropriate structural works, all the excremental produce of the population shall be so promptly and so thoroughly removed, that the inhabited place, in its air and soil, shall be absolutely without fæcal impurities; and (2) that the water supply of the population shall be derived from such sources, and conveyed in such channels, that its contamination by excrement is impossible.' And he concludes with the pious hope that 'for a population to be poisoned by its own excrement will some day be deemed ignominious and intolerable.'

Our knowledge of the *morbid anatomy* of cholera has become more complete and more exact during the last epidemic.

Drawing my conclusions not from any experience of my own, but from numerous and very careful *post-mortem* inspections made by Dr. Parkes, by Dr. Johnson, by Dr. Sutton, and by others, I believe it may be stated as a rule—a rule broken sometimes, no doubt, by disturbing but intelligible circumstances—that

In cases of death *during collapse*, when the examination is made sufficiently early, the lungs are found to be shrunken, light, dry, and pale—in one word, unnaturally bloodless; the left ventricle of the heart is contracted and nearly empty, its right cavities, the trunk of the pulmonary artery, and the systemic veins much distended with blood, the mucous membrane of the intestines free from congestion, and pale.

In some of these cases the lungs, though very light in weight, are of a dark colour, which gives them an appearance of congestion. This colour Dr. Johnson refers to a backward engorgement of the *bronchial* veins and capillaries, consequent upon the block in the pulmonary artery and its branches.

When death has occurred during incipient and imperfect reaction, the morbid conditions disclosed by dissection are the reverse of these. The lungs *are* congested, sometimes even inflamed; and the mucous membrane of the intestines is also loaded with blood.

These are points which bear closely upon the pathology of the disease; and upon its true pathology rests its rational treatment. To these I now turn.

There are two conflicting theories as to the pathology of cholera; and there are two conflicting principles—which accord with and flow from these theories respectively—as to its proper treatment. Upon this momentous problem of treatment, the final appeal must clearly be made to experience.

It is acknowledged on all hands that the primary and special danger in cholera lies in its period of collapse. Now it was a very natural and plausible theory which attributed this state of collapse to a drain upon the blood by the profuse and repeated fluxes from the stomach and bowels, whereby the blood, being robbed of its more liquid ingredients, and made thick like tar or treacle, became incapable of flowing freely, if at all, through its natural channels; and thus the circulation coming ultimately to a stop, life stopped also. And the practice suggested, and put in force, as a direct corollary to this theory, was that of endeavouring to arrest the destructive flux by astringent drugs and by opium, to sustain or urge on the lingering circulation, and to restore the spent strength and the lost animal warmth by alcoholic and other stimulants. Upon similar grounds was advocated the dilution of the thickened blood by water injected into the veins.

It is affirmed, on the other hand, that the condition called collapse is not due to the excessive discharges from the body; that those discharges are really eliminative of the poison, or of the products of the poison, which caused the disease, and are to be permitted, or even encouraged, rather than checked; that to pen the poison and its products within the body is to fight against the conservative forces, and to do what art can do to ensure the mortal agency of the poison, and, therefore, that astringents and opiates can do no good, but are, on the contrary, positively hurtful.

Were the first-mentioned theory true, there must be a discernible relation between the alleged cause and its effect. The greater the amount of the intestinal discharges, the more certain and the more decided should be the resulting collapse. But no such proportion has, in fact, been observed. Nay, the very reverse not seldom obtains. The most hopeless cases are those of collapse after very scanty discharges, or with no discharges at all.

Again, if the collapse were indeed owing to the drain upon the blood effected through the intestinal discharges, it would be prolonged, deepened, and rendered more perilous, by the continuance of those discharges; whereas it is notorious that patients emerge from the state of collapse, and recover, while the evacuations nevertheless go on, and that the cessation of the evacuations during collapse is a fatal sign. 'It may confidently be asserted,' says Dr. Parkes, 'that there is no one who has seen much of cholera, who does not know that, exclusive of the mildest forms of the disease, a case with little vomiting or purging is more malignant and more rapidly fatal than one in which these are prominent symptoms.'

Tested, then, by the evidence of acknowledged facts, this theory must be pronounced a failure, and the treatment founded upon it a mistake.

In truth, a fallacious analogy has been assumed between the collapse or exhaustion arising from a drain upon the blood and the collapse in cholera. In one single point—namely, the smallness and weakness of the arterial pulse (in other words, the defective circulation of the blood)—the two may seem to touch each other. In almost every other point, they differ widely. A person exhausted by loss of blood, or by a long continued drain upon that fluid, is in a state that is very near to syncope. When the exhaustion is extreme, if he assume the erect posture, he faints outright, and becomes unconscious. To walk, to stand, or even to sit up, is simply impossible; whereas, in the collapse of cholera, a patient, with death stamped apparently upon his features, with no pulse to be felt at his wrist, with a blue and icy-cold skin, may be able to walk about the room, and to perform many of his usual functions. He does this indeed at the peril of his life, but the fact that he is capable of such an effort proves that there is an essential difference between cholera collapse and ordinary syncope. The exhausted man, if he recover, recovers slowly; the repair of his impoverished blood is necessarily a gradual process. The cholera patient rallies from his collapse at once, if at all. He may be in full collapse to-day, and convalescent the day after to-morrow, and apparently but little the worse for the terrible disorder through which he has so recently passed. 'I have seen,' says Mr. Grainger, 'a man stand at his door on Wednesday, who on Monday was in perfect collapse.' Again, the way in which remedies tell upon the two contrasted conditions is totally and instructively unlike. The coldness and faintness of exhaustion are relieved at once by a glass of wine or of brandy; the pulse instantly acknowledges the virtue of the stimulus. But alcoholic stimulants do not warm or invigorate, even for a moment, the patient in choleraic collapse: rather, they seem to make matters worse. On the other hand, bloodletting has often brought marvellous relief under collapse; while to draw blood from a person who is fainting from exhaustion would probably ensure his death, and would certainly aggravate his danger. Take the following instance, recorded by Sir Ranald Martin, of the effect of venæsection. 'On visiting my hospital in the morning, the European farrier-major was reported to be dying of cholera. I found that during the night he had been drained of all the fluid portion of his blood. His appearance was surprisingly altered; his respiration was oppressed; the countenance sunk and livid; the circulation flagging in the extremities. I opened a vein in each arm; but it was long before I could obtain anything but trickling of dark treacly matter. At length the blood flowed, and by degrees the darkness was exchanged for more of the hue of nature. The farrier was not of robust health, but I bled him largely; when he, whom not a moment before I thought a dying man, stood up and exclaimed, "Sir, you have made a new man of me." He is still alive and well.'

The question has naturally been put, 'Is it possible to reconcile facts of this kind with the theory that the collapse of cholera results from the loss of the liquid constituents of the blood? If Sir R. Martin's hypothetical statement that his patient "had been drained of all the fluid portion of his blood" were an accurate expression of facts, can we conceive it possible that he could have "made a new man" of him by abstracting largely the blood which remained in the vessels?'

The main advocate in this country, and, as I think, the triumphant advocate, of what may be called the evacuant or cleansing practice in cholera, is your present able Professor of Physic, Dr. George Johnson. To him is justly due the great merit of having established, by his persevering efforts in the face of much opposition and discouragement, the worth and efficacy of that practice; although he was not the first to recommend or to adopt it. It was in fact tried, with favourable results, nearly half a century ago, by English practitioners in India; its professed object being that of getting rid of offensive morbid secretions. The practice thus vouched for by Dr. Johnson is directly in accordance with, and serves to confirm, that view of the pathology of cholera which, by a methodical display of numerous facts, and by a process of close and conclusive reasoning, he may fairly challenge as his own. Briefly, he holds, as many before him held, that the phenomena of cholera result from the entrance of a peculiar poison into the blood, where it probably undergoes, like that of small-pox, a rapid process of self-multiplication, and spoils certain of the blood-constituents, which are then ejected through the mucous membrane of the alimentary canal; that the feelings of general oppression and malaise sometimes experienced before the onset of the bowel symptoms, are indicative of blood-poisoning; that the copious discharges are expressive of the efforts of nature to throw off a noxious material, and really form, therefore, a necessary part of the process of recovery; and that, if the pouring forth of the vascular excretion be checked (as it can, perhaps, be by opium), the risk of fatal collapse is thereby increased. He declares that the results of his own practice, founded on these views, have amply justified them; and a considerable body of other evidence has now been furnished in support of the same plan of treatment.

It is plain that, if 'elimination' be a condition of recovery, the method of elimination is Nature's method, which Art may help or hinder—help by the cleansing method, hinder by the astringent.

In discussing the principle of treatment I have shot ahead of several points in the novel, interesting, and to my mind satisfactory exposition of the general pathology of cholera by Dr. Johnson.

Remember the abrupt contrast seen, upon early examination of the body after death during collapse, between the anæmic condition of the lungs, and the gorged condition of the trunk of the pulmonary artery and of the systemic veins. What is the explanation of this sudden arrest of the stream of blood in the small arteries, just before it reached the capillaries? Were the arrest of motion due to gradual thickening in consequence of the continued abstraction of its liquid portion, it would be found stagnating in the capillaries, as well as in the arteries. Bear in mind that one characteristic symptom of cholera—that symptom which, irrespectively of the fatality of the disease, renders it truly a disease to be dreaded—consists in very painful cramps of the larger muscles of the body. These contractions, it may be assumed, are produced by the choleraic poison, just as we know they are producible by the poison of strychnine. Dr. Johnson supposes that a similar spasm or cramped state of the muscular fibres which embrace the minute pulmonary arteries, is caused by the same choleraic poison, and bars these slender channels against the advancing blood: that the stop-cock action which I have so often explained to you, comes here into play. The thickening of the blood is a consequence, and not a cause, of the arrested circulation and the collapse. Precisely the same blood-thickening occurs as a result of the impeded circulation through the lungs which is associated with long continued, extreme, and fatal apnœa, as I have explained to you in a former lecture.

The true explanation of the fact that mere diarrhœa, however profuse, does not thicken the blood, is probably, as Dr. Johnson suggests, that water is rapidly absorbed from the soft tissues to take the place of that which escapes from the alimentary canal. Acting on this principle of physiological hydraulics, we remove a dropsical accumulation by the action of a hydragogue purgative.

Surely the theory that I have now placed before you seems a reasonable theory. It is founded on a true analogy; it is consistent with the symptoms noticed during life, and with the conditions discovered after death. We may, therefore, legitimately regard it, until fairly refuted, as a sound as well as a most ingenious and important theory. In truth, it derives strong confirmation from the fact that it unlocks, like the right key, the whole of the pathological intricacies of the disease. Thus the emptiness of the systemic arteries accounts for the extinction of the pulse at the wrist, for the cadaverous sinking in of the eyeballs and falling of the features, for the blueness and coldness of the skin, and for the absence of syncope. The circulation stops, not from debility of the heart, as in exhaustion, but in consequence of a direct mechanical impediment to the onward course of the blood. We can understand the impotence of brandy against this condition; and how, on the other hand, bleeding may help, both by relaxing the spasm and by unloading the distended right heart, to restore the circulation. Into this explanation Dr. Johnson presses, plausibly enough, the singular effect of the injection of fluids into the veins of these patients. It appears that, to be most influential, the fluids must be hot; and he concludes that they act partly by diluting the morbid blood, but chiefly by relaxing, through their warmth, the spasm of the smaller arteries. The blood then flows on again, and the symptoms of collapse are for a time removed. Again, the husky whispering voice is owing, not to muscular weakness, but to the small volume of tidal air in the respiratory currents. As but little venous blood reaches the lung tissue proper, there is but little demand for air to meet and decarbonise it. The respiration accordingly becomes shallow, and the vocal pipe, feebly blown through, refuses to speak. Under the temporary impulse of the warm injections, the voice regains its usual tone and note. Once more, there are chemical and less obvious changes which receive their explanation from this theory, and further attest its truth. The stream of blood through the pulmonary capillaries being greatly lessened, the supply of oxygen is proportionally reduced in quantity. Hence during the stage of collapse there is defective oxygenation of the blood and of the various tissues of the body, coldness and blueness of the surface, diminished exhalation of carbonic acid, and suppression, nearly absolute, of bile and of urine—carbonic acid, and the chief constituents of bile and of urine, being all results of oxidation. That this is the correct explanation of the suppression of bile and urine during collapse is rendered all the more probable by the curious fact that, when a nursing mother becomes the subject of cholera, and falls into collapse, the secretion of milk continues unchecked. Now the chief constituents of milk—casein, sugar, oil, and water—may be obtained from the blood without the addition of oxygen. They are *not* products of oxidation.

If the doctrines advanced by Dr. Johnson be well founded, as I firmly believe them to be, it must be wrong to dam the choleraic poison and its products within the body. Even when those products have, in one sense, been separated from the system, they may produce highly noxious effects if they remain shut up in the stomach or bowels, there to ferment and decompose. Admitting, as we must, that a minute quantity of the morbid excretions swallowed with water may suffice to produce the disease, a large quantity retained, through weakness of the expulsive powers or otherwise, can scarcely be harmless. Rather may we expect that its expulsion will tend to liberate the patient from danger and discomfort; just as the opening of large abscesses, and the discharge of foul pus and imprisoned gases, are often seen to rescue, as if by magic, a sick man from apparently impending dissolution. Whatever may have been Dr. Johnson's earlier purpose, he does not now propose to *excite* discharges from the mucous surface of the digestive canal; but simply to facilitate the removal of matters lodged there. And this he would do by emetics, by draughts of tepid water or other diluents, or by castor-oil, of which the action is both speedy and gentle. The recommendation of the evacuant plan must, after all, lie in its comparative success, and its worth has already been put closely and extensively to the proof.

In the fiftieth volume of the 'Medico-Chirurgical Transactions,' there is a most instructive communication from Drs. M'Cloy and Robertson. They show that, of 375 cases of cholera admitted into the Liverpool Parish Infirmary in the last epidemic, 161 proved fatal—a gross mortality, under all the modes of treatment adopted, of 42·93 per cent. Of these cases, 91 were treated with astringents and stimulants, camphor and iced water, applications of ice, and hypodermic (opiate) injections; and the mortality per cent. of these cases was 71·42. 87 cases were treated with castor-oil, and with a liberal use of food and alcohol; and the mortality was 41·37 per cent. 197 cases were treated with castor-oil only, and the mortality was 30·45 per cent. The authors of the paper declare that 'recovery never occurred without the continuance of the intestinal discharges; or their restoration, if previously arrested.'

The late Inspector of Prisons, Mr. Perry, had charge, in 1832, of the cholera patients in the Marylebone Workhouse. He told me that, though he had no specific notes to refer to, he distinctly remembered that about thirty patients were treated with castor-oil; and that they did better than any of the others.

When I last spoke on this subject in these Lectures, I stated that the few recoveries which I had witnessed had all taken place under large and repeated doses of calomel, but that I could not venture to affirm that the calomel cured them. At present, I am much disposed to believe that, by its cleansing action, the calomel may have helped the recovery; and, after all that I have since seen, heard, read, and thought upon the matter, I must confess that, in the event of my having again to deal with the disorder, I should feel bound to adopt, in its generality, the evacuant theory and practice; and to avoid alcoholic stimulants and opiates.

Now, if this theory and practice in respect of cholera be true and right, the practice ought to be right in respect of the associated diarrhœa also; and it is strongly affirmed by those who have largely tried it, that it is right, inasmuch as it is eminently successful. Dr. Johnson avers that he has found it so.

Hear the concurring testimony of Drs. McCloy and Robertson. 'Our experience of diarrhœa was very extensive. Several thousand cases came under our observation in the different dispensaries connected with the West Derby Union and in the Liverpool Parish Infirmary. Among these were doubtless many which would have recovered under any mode of treatment, or by the *vis medicatrix naturæ* alone. But there were many, too, of a most severe choleraic type. The treatment adopted was generally evacuant in its nature; and consisted in the administration of castor-oil, calomel, rhubarb, or magnesia. In every case, relief was afforded "pleasantly, quickly, and safely." It was but seldom that more than two or three doses of oil were required.' The medical officers of the Bootle Dispensary depose to the same effect: 'We certainly had less trouble with the evacuant mode of treatment. Our patients seldom gave us a third visit; two doses of castor-oil or rhubarb mixture being generally sufficient to cure the disease.' 'We never saw a diarrhœa patient, treated with evacuants from the commencement of his attack, require subsequent removal to hospital. In a large proportion of our cases there was "premonitory diarrhœa" which had been treated, often for four or five days, with astringents. Diarrhœa patients undoubtedly recover when treated with astringents; but the recovery is not consequent upon the arrest of the discharges, as these are invariably restored before the patient feels well.'

In the face of this and of much similar evidence, I feel bound to say that the rules laid down by Dr. Johnson for the treatment and prevention of diarrhœa and cholera, seem now to me safer and better than the less discriminating advice which heretofore I gave you, 'whenever a suspicion arose that cholera was present in the community, not to try, in cases of diarrhœa, to carry off the presumed offending matter, but to quiet the irritation and to stop the flux as soon as possible, by astringents, aromatics, and opiates.'

No doubt, the true indication of treatment is, 'to stop the flux as soon as possible;' but this may sometimes be best effected (as also in crapulous diarrhœa, and in the summer cholera of Sydenham) '*by* carrying off the offending matter.'

In language chiefly his own, but partly also mine, Dr. Johnson writes as follows:

'*Diarrhœa ought not to be neglected, even for an hour.*'

'One important and guiding rule of treatment is, '*not to attempt by opiates, or by other directly repressive means, to arrest a diarrhœa, while there is reason to believe that the bowel contains a considerable amount of morbid and offensive materials.*'

'The purging is the natural way of getting rid of the irritant cause. We may *favour* the recovery by directing the patient to drink copiously any simple diluent liquid—water (cold or tepid), toast-water, barley-water, or weak tea; and we may often accelerate the recovery by sweeping out the alimentary canal by some safe purgative, and then, if necessary, soothing it by an opiate. Castor-oil, notwithstanding its unpleasant taste, is, on the whole, the safest and best purgative for this purpose. It has the advantage of being very mild and unirritating, yet withal very quick in its action. A tablespoonful of the oil may be taken, floating on cold water, or any other simple liquid which may be preferred by the patient. A mixture of orange-juice or of lemon-juice with water forms an agreeable vehicle for the oil. If the dose be vomited, it should be repeated immediately; and the patient should lie still, and take no more liquid for half an hour, by which time the oil will have passed from the stomach into the bowels. Within an hour or two, the oil will usually have acted freely. Then a tablespoonful of brandy may be taken in some thin arrowroot or gruel; and, if there be much feeling of irritation, with a sense of sinking, from five to ten drops of laudanum may be given in cold water. These means will suffice for the speedy arrest of most cases of choleraic diarrhœa. If the patient have an insuperable objection to castor-oil, or if the oil cannot be retained on the stomach, ten or fifteen grains of powdered rhubarb, or a tablespoonful of the tincture of rhubarb, or a teaspoonful of Gregory's powder, or a few grains of calomel in powder washed down with cold water, may be substituted for the oil.'

'If the diarrhœa have continued for some hours, the stools having been copious and liquid; if there be no griping pain in the bowels, no feeling or appearance of distention of the intestines; the abdomen being flaccid and empty, and the tongue clean—we may conclude that the morbid agent has already purged itself away. There will, therefore, be no need for the castor-oil or other laxative; and we may immediately give the brandy in arrowroot, and the laudanum, as before directed. The rule in all cases is, *not to give the opiate until the morbid poison and its products have for the most part escaped; not to close the door until "the enemy" has been expelled.* While there are some cases in which the evacuant dose is not required even at the commencement of the attack, there are many more in which the opiate is unnecessary in the later stage. In some cases of severe and prolonged diarrhœa, it may be necessary to repeat the oil and the laudanum alternately more than once, at intervals of three or four hours. Practical skill and tact are required to discriminate these cases. It must be borne in mind that, when the choleraic secretions are being actively poured out from the blood-vessels, the bowel, though it may have been completely emptied by a dose of oil, may quickly again become filled with morbid secretions, and hence the need for an occasional repetition of the evacuant dose.'

'If the diarrhœa be associated with vomiting, this should be encouraged and assisted by copious draughts of tepid water. The vomiting affords relief, partly by the stimulus which it gives to the circulation, but mainly by the speedy ejection of morbid secretions.'

'If there be nausea without vomiting, and more especially if the stomach be supposed to contain undigested or unwholesome food or morbid secretions, an emetic may be given—either a teaspoonful of powdered mustard, or a tablespoonful of common salt, or twenty grains of ipecacuanha powder, in warm water.

'In all cases of severe diarrhœa, the patient should remain in bed.'

Whenever cholera is prevailing epidemically, great care should be taken to insure the purity of the water used for drinking: when its purity is doubtful it should be boiled and then filtered.

The literature of cholera is endless and perplexing. Dr. Farr's letters in the Reports of the Registrar General; the Report on cholera to the College of Physicians, by Drs. Baly and Gull; Dr. Acland's memoir of the cholera at Oxford; Dr. Sutton's paper in the London Hospital reports; that of Drs. McCloy and Robertson in the 'Medico-Chirurgical Transactions;' and the several reports issued on the subject by the Privy Council; but especially Dr. Johnson's writings on the subject, are the best sources of authentic information that I have seen.

LECTURE LXVII.

Dysentery. Diarrhœa Adiposa. Intestinal Concretions. Worms.

Another of the morbid fluxes from the alimentary tube, of which I have yet to speak, is *dysentery*.

Its characteristic symptoms are, griping pains in the abdomen, followed by frequent mucous or bloody stools, straining, and tenesmus. In chronic cases pus is sometimes discharged from the bowels. The acute form or stage of the disease is attended with fever.

The differences between dysentery and diarrhœa are obvious enough. Both of them may be accompanied by griping pains: in both the stools are frequent and loose: but in diarrhœa they are fæcal; in dysentery there is retention of the natural fæces, or they are expelled from time to time, in small, hard, separate lumps, termed *scybala*. But scybala may accompany, and even cause, diarrhœa. Again, straining, and tenesmus, and the excretion of mucus, which often is tinged with blood, form no necessary features in diarrhœa; whereas in dysentery those symptoms are prominent and constant. These nosological distinctions are true and useful, although in our actual intercourse with the sick we do not find them always or strictly observed. Some of the worst forms of dysentery *commence* with the ordinary symptoms of diarrhœa.

Dysentery consists, essentially, in inflammation of the mucous membrane of the large intestines; yet not, I apprehend, of the whole of that long surface indiscriminately. Observation of the course of the disorder, during life, and of the morbid appearances visible after death, leads to the conclusion that in simple dysentery, marked by tormina and tenesmus, and frequent dejections of sanguinolent mucus, without fæcal matter, the inflammation chiefly affects the *rectum* and the *descending colon*. When the earlier portions of the large intestines are involved in the diseased process, the stools at the outset are often composed in great measure of excrement in an unnaturally fluid state, and mingled with blood and slime. We generally speak of these circumstances as constituting *dysenteric diarrhœa*.

Slight and simple dysentery may occur and run its course with very little or no disturbance of the circulation. When it is acute and severe, it is attended with more or less pyrexia. The acute disease may terminate in recovery; or in early death; or in chronic dysentery, which usually, in the end, is fatal.

The wards of our metropolitan hospitals place frequently under our notice severe cases of chronic dysentery in the persons of soldiers and sailors, who bring the disease home with them from hot climates. With these exceptions, dysentery, now-a-days, is neither a very common nor a very serious disorder in this country. I say now-a-days, for the time was when it raged in London like a plague. The second Dr. Heberden, in his valuable essay, 'On the Increase and Decrease of different Diseases,' shows, that in the seventeenth century the number of deaths set down, in the weekly bills of mortality, under the titles of *bloody flux*, and *griping in the guts*, was never less than 1,000 annually, and in some years exceeded 4,000. For five-and-twenty years together, viz. from 1667 to 1692, they every year amounted to above 2,000. During the last century, the number gradually dwindled down to twenty. Dysentery is one of the pests of hot climates. In most tropical regions, at certain seasons of the year, it is very prevalent and destructive. But it is in fleets and armies, and especially among troops in actual service, that the distemper most displays its terrible power. There is no single malady which is so crippling to an army in the field as this. Sir James M'Grigor, to whom was entrusted the superintendence of the medical department of the army on 'the two greatest services on which the military force of this country has, of late years, been employed, namely, that in Walcheren, and that in the Peninsula,' calls dysentery 'the scourge of armies,' and the 'most fatal of all' their diseases. In two years and a half, the British army in Spain lost no less than 4,717 men by this complaint.

How are these facts to be explained? Wherefore is dysentery, which was so familiar to our ancestors, so happily rare among us? Why does it thus wait upon and afflict the march of armies? Upon what depends its frequency in hot climates? We may expect to obtain some answer to these questions by searching into the *causes* of the disorder.

It has been ascribed to exposure to wet and cold; to the use of unwholesome food; to the agency of malaria; to contagion.

Weather and season have a manifest influence in the production of dysentery. In temperate climates, like our own, it is an autumnal disorder. In tropical countries it is observed to be more common and more severe when rains succeed to a long-continued drought. With respect to this, as to other bowel affections, a high diurnal temperature of the air appears to be the predisposing, and exposure to cold the exciting cause. I stated, on a former occasion, that great vicissitudes of temperature are very frequent and very pernicious, even under the torrid zone. Scorching days are followed by extremely cold nights. The dysentery which arises under these circumstances is apt to run on into the ensuing winter. Soldiers in the field against an enemy are peculiarly obnoxious to the agencies which generate or favour the complaint. Marching, or engaged in actual conflict, during the day; bivouacking at night, often in the open air, and under every variety of weather; ill-provided too often with clothes and bedding; their food scanty, precarious, or of bad quality; seizing the many opportunities which their dreadful trade supplies of license and intemperance; depressed, it may be, by disaster or defeat; we need not wonder either at the prevalence of dysentery among them, or at its untractableness while they remain subject to the same morbid influences. Neither can the causes be warded off from the patient; nor, in general, can the patient be removed from the causes. Yet occasions do arise which show distinctly enough this alleged relation of cause and effect. *Præsens morbum facit —sublata tollit.* Take, on the one side, the following facts from Sir John Pringle's book 'On the Diseases of the Army.' The men who had fought at Dettingen lay that night on the field of battle, without tents, exposed to a heavy rain. For the next night or two they encamped on better, but still wet ground; and they wanted straw. Nearly half of these troops were soon after affected with dysentery; while three companies which had not been engaged in the battle, nor exposed to rain, nor lain wet, escaped the complaint entirely. Take this converse fact, related by Desgenettes. Four hundred of the French 'army of Egypt,' reduced to a state of extreme weakness and emaciation by dysentery there contracted, embarked at Alexandria on their return towards France; were carried away, in short, from the alleged causes of their disorder. Nineteen died at the very outset of the voyage; which had, however, so good an effect upon all the rest, that before they reached Malta they were thoroughly convalescent.

The very frequent coincidence or alternation, in some places, of dysentery with intermittent fever, has given rise to the opinion that both these diseases are alike attributable to the malarious poison. But dysentery prevails where there is no other evidence of the presence of malaria. You may recollect that when we were upon the subject of ague, I showed you that its repeated paroxysms were attended with extreme and increasing congestion of blood in the internal organs; of which congestion the tumid spleen, the ague-cake, was an effect and a token. Now whatever gorges the splenic vein, gorges its tributary, the inferior mesenteric, which carries the blood from the rectum and the descending colon. Upon such congestion of the mucous membrane inflammation is readily engrafted; and in this indirect way dysentery may be said to result from the marsh effluvia. Ague is an effect of malaria; and dysentery is, sometimes, a sequela of ague. In precisely the same manner, dysentery, in hot climates especially, is apt to supervene upon or to accompany *hepatic* congestion and disease. On the other hand, Dr. Budd has taught us how dysentery may lead to a peculiar kind of suppuration in the liver.

That dysentery, in itself, is a *contagious* malady, I can neither affirm nor deny; from which you may infer that it is at any rate not very conspicuously contagious. In this country, in its simple form, it is not observed to spread from person to person. If it possess infectious qualities, these reside, I apprehend, in the effluvia proceeding from the morbid alvine dejections. We shall hereafter see reason to believe that typhoid fever is communicable chiefly in that way: and dysentery is a prominent symptom in some epidemic visitations of typhoid fever.

The remarkable decline of dysentery in this metropolis, has been contemporary with that of some other severe disorders: and is due to the same combination of causes. For upwards of two centuries we have had no *plague* among us. *Agues*, formerly very rife in London, have almost disappeared. *Continued fevers*, which used to break out annually in hot weather, are comparatively unfrequent. I believe that we may trace these great blessings to an event which was regarded by many, at the time, as a national judgment; I mean the great fire that, in 1666, consumed everything between Temple Bar and the Tower. The streets and houses thus destroyed had been filthy in the extreme, close, densely crowded, and consequently most unhealthy. The impurity of the air excited, perhaps, some maladies; and it certainly predisposed those who dwelt in it to various kinds of disease, 'the seeds of which (says Dr. Heberden), like those of vegetables, will only spring up and thrive when they fall upon a soil convenient for their growth.' To the better construction of the houses and of the streets in the rebuilt city; to the increased means of ventilation; to the general formation of drains and sewers; to the more copious supply of water; and to the more temperate and cleanly habits of the people; we may fairly ascribe our present comparative exemption from dysentery, from ague and continued fever, which are sometimes the parents of dysentery, and from the plague itself. In very many parts of this overgrown place there is still too much room for improvement.

The pyrexia that accompanies dysentery sometimes begins before the local symptoms declare themselves; more frequently it succeeds their manifestation. Occasionally the fever runs high, the pulse is hard and frequent, the skin hot, the face flushed, and the tongue furred; and the patient complains of headache and thirst. But in this as in other abdominal diseases, the pulse soon becomes small and weak, the strength rapidly declines, and the temperature of the body sinks.

In acute cases the pain is often severe; but it is subject to remissions and exacerbations. It occupies the hypogastrium, or some part of the course of the colon, where there is usually more or less tenderness on pressure. The patient is tormented by a sensation as if there were some excrement ready to be dislodged, goes perpetually to the night-chair, and is irresistibly impelled to strain violently to get rid of the irritation. But the efforts are ineffectual; he discharges but little; and what is voided is either altogether a jelly-like mucus (in which case the complaint has been called the *dysenteria alba*, and the *morbus mucosus*), or more commonly it is mucous and bloody (the *bloody flux* of our old authors), mixed with films and membranous shreds, and fragments that resemble flesh. In many of the dejections there is no genuine fæcal matter at all; or the small indurated balls which I just now mentioned come away occasionally. Frequently the ejected mucus is variegated in colour; green, or black, or reddish, like the washings of meat, and horribly fœtid. Sometimes pain and difficulty in making water are added; there is *dysuria*, the irritation of the rectum being reflected upon the bladder through the lower portion of the spinal cord. Sometimes the stomach sympathises, and nausea and vomiting ensue. With all this local suffering there is a continuance of febrile distress; the patient passes sleepless, or dreamy and disturbed nights, and is low-spirited and desponding. In the fatal cases the pulse becomes very small and rapid, the features sharpen, and the surface grows cold. Death begins at the heart.

Inspection of the dead body discloses more or less ulceration, chiefly in the large intestine. The glands that lie scattered over its inner surface are enlarged and prominent, looking somewhat like small-pox pustules, for which indeed they have been mistaken. They probably form the points of origin of most of the ulcers, which are sometimes narrow and oblong, lying across the gut; sometimes very large and irregular, with here and there islands or ridges of thickened, half-dead mucous membrane. In the worst cases the whole extent and circumference of the bowel presents, internally, one irregular, confused, and tattered mass of disorganisation.

When submitted to early *treatment*, and when its exciting causes can be averted, or avoided, dysentery is not always an intractable disorder. Sir James M'Grigor remarks of the camp dysentery in the Peninsula, that it had two stages, which it was of consequence to note, because they required different and almost opposite modes of treatment: the inflammatory stage, and the stage of ulceration. A plan proposed by Dr. Somers appeared to Sir James so judicious, and proved so successful, in the first attacks of the pure unmixed disease, that he recommended its general adoption in the army. It was this.

First, the patient was freely bled. Immediately afterwards twelve grains of Dover's powder were administered. This dose was repeated three times, at intervals of one hour. Plenty of warm barley-water was at the same time given, and profuse sweating encouraged for six or eight hours. A pill, containing three grains of calomel and one of opium, was directed to be taken every second night; and to be followed on the following mornings by two drachms of Epsom salts, dissolved in a quart of light broth. The venæsection was repeated, while the strength and the pulse permitted it, until the stools were free, or nearly free, from blood; and Dover's powder, as a sudorific, was always given after the blood-letting. When the pains were great, and attended with much tenesmus, the warm bath gave instantaneous relief. 'This plan being steadily persevered in for a few days, the inflammatory diathesis of the intestinal canal, which had excited symptomatic fever throughout the general system, was found gradually to yield, and to make way for returning health.'

This plan would, I conceive, have been equally successful, and certainly more in consonance with the principles I have advocated respecting the abstraction of blood, if the application of leeches over the track of the affected bowel had been substituted for the venæsection.

If the disease were not cut short by this method, but advanced into the second stage, and became chronic, the most effectual remedies appeared to be laxatives, and opiates, given alternately; and combined with such medicines as promote perspiration. The abdomen should be swathed with flannel, or covered by a warm adhesive plaster. Much benefit may be obtained from the employment of clysters, if there be not too much tenesmus to admit of the introduction of the pipe of the injecting syringe. Warm starch, with laudanum in it—not exceeding in quantity a couple of ounces, lest the irritable bowel should expel it again—will sometimes afford signal relief. Or if the pain and tenesmus are so great that a clyster-pipe cannot be used—or the enema be not retained—a grain or two of solid opium inserted into the rectum, beyond the sphincter ani, will often allay the distress. The food should be farinaceous and simple; and great care must be taken during the convalescence to prevent a return to improper diet, and any fresh exposure to cold.

Is mercury, pushed to its specific effect upon the system, a fitting remedy for acute dysentery? In my opinion it is not. There are no *à priori* grounds for expecting advantage from it; and amid the striking discrepancy of opinions expressed by persons who have had large experience of the disease in India and elsewhere, the evidence clearly predominates against its employment. It may be of use in occasional doses, as Sir James M'Grigor suggests, when the dysentery is complicated with disease or disorder of the *liver*; when together with the dysenteric symptoms there are present a dull pain in the hepatic region, and in the right shoulder, a yellowish brown colour of the skin and of the conjunctivæ, and uneasiness when the patient lies in any other posture than on the right side. But he adds: 'In the early stage of the acute and unmixed disease, and before venæsection has been performed, mercury will aggravate the symptoms. In the more advanced stage of the disease, particularly when there is hectic fever, with extensive erosion or ulceration of the intestine, it is invariably found to hurry it on to a fatal termination.'

There is one remedy for acute dysentery, which counts many experienced advocates, I mean ipecacuan, given in a large dose: half a drachm of the powdered root at a time for example; repeated or not according to circumstances. These large doses do not generally excite vomiting, especially if a moderate opiate be taken half an hour previously and the patient refrain from every kind of drink for some time afterwards. The drug thus given acts upon both the bowels and the skin.

For sporadic dysentery, such as we sometimes meet with in this country, leeches are to be applied, in the track of the colon, wherever there is much tenderness on pressure. A full dose of castor-oil may then be given; and after that an opiate; and no form of opiate can be better than Dover's powder. It is the practice of some physicians to prescribe laxatives and opium together; but in this complaint it is better to alternate them. Opiate enemata are of service for relieving tenesmus. These remedies will be much assisted by the warm bath; by hot fomentations to the abdomen; and by such means as promote the natural secretions of the skin. If there be any reason to suspect that the portal system is gorged with blood, complete relief to the dysenteric symptoms may often be obtained by the practice which I recommended as proper in some cases of melæna—viz. the administration of five grains of calomel at bed-time, and of a senna draught the next morning, for two or three days in succession.

The fluxes of which I have hitherto spoken have all consisted in an immoderate discharge of some of the usual contents or secretions of the alimentary tube, in an altered and unnatural state. But matters are sometimes voided from the bowels totally unlike any of the healthy discharges. *Adeps* is not an intestinal excretion; yet it is sometimes passed, in great abundance, by stool. Many unquestionable instances of this are on record, both in ancient and in modern literature. I have not seen more than one, and therefore have but little to say upon the subject; yet I must not pass it over altogether. A certain quantity, sometimes it has been a *large* quantity, of oil, of liquid fat, has been poured forth, in a sort of diarrhœa. Sauvages was aware of the disorder, and calls it, in his 'Nosology,' *diarrhœa adiposa*. In a paper in the 'Medico-Chirurgical Transactions' upon this affection, Dr. Elliotson refers to an example of it described by Tulpius, in which a woman discharged every day, for fourteen months, a considerable quantity of yellow fat, that lay upon the fæces like melted butter. When voided into a vessel of water it floated, like oil, upon the surface; and when cold it assumed the consistence and appearance of fat. Like fat, it was very inflammable, and burned with a bright flame. With all this there was no kind of distress, nor any wasting of the body; and the patient was in excellent health sixteen years afterwards. Dr. Elliotson had a case of this kind under his own care. The man had diabetes also and phthisis. The symptoms were precisely the same as those described by Tulpius. Dr. Prout and Dr. Faraday analysed portions of the adipous matter, and they pronounced it to be genuine fat. Mr. Lloyd, of St. Bartholomew's Hospital, has given us the details of a case in which the evacuation of grease was associated with jaundice. The excretion looked like melted fat, but when cool had the consistence of butter. It swam on the surface of water, melted at a moderate heat, and burned readily. In this instance the head of the pancreas, and the duodenum, were involved in a mass of scirrhous disease. And this is a very curious fact: for Dr. Bright also states that in three different persons, each of whom he had known to pass fat from the bowels during life, and whose bodies he had the opportunity of examining after death, he found scirrhous disease of the pancreas, and fungous disorganisation of the duodenum.

The remarkable coincidence, occurring so often, of cancerous disease of the pancreas and duodenum, with these fatty profluvia, is the more memorable, because it falls in with Dr. Claude Bernard's theory already mentioned, that one important purpose of the pancreatic fluid is to promote the absorption of fatty matters, by forming with them a permanent emulsion, capable of entering the lacteals.

The single case of which I have had any cognisance is that of a tradesman now living in my neighbourhood, who from October 1843 to August 1844 suffered frequent attacks of this kind. First, he had severe pain in the epigastrium and right hypochondrium, with little or no fever, or acceleration of pulse. Occasionally, when the pain was extreme, it was accompanied by nausea and vomiting. After the pain came jaundice and white stools; and, lastly, a discharge from the bowels of a quantity of white fragments, looking exactly like coarse chewings of walnuts. Sometimes much larger lumps of the same sort were voided. These lumps and fragments were found to be composed of adeps. They floated, many of them at least, and for a while, upon the surface of water. They felt greasy; and burned, like fat, with a flame.

After this series of symptoms the patient would recover perfectly, save that the attacks reduced his strength somewhat. They occurred about once a fortnight, so that he had about a score of them in ten months.

The symptoms resembled those which declare the passage of gall-stones; and upon one occasion two concretions were detected among the fragments of fat. One of these was yellowish; the other black, like a cinder. Once, also, a considerable portion of what seemed membrane came away. My neighbour, Mr. Francis E. Hicks (to whose kindness I am indebted for the opportunity of seeing this patient), is of opinion that a cyst, which secreted the fat, existed in the liver, and discharged its contents periodically; and that the membranous fragment was a part of this cyst.

Projecting a little into the epigastric notch, I could feel what appeared to be the edge of the left lobe of the liver. This spot was slightly tender, and dull under percussion.

I had previously been consulted by the same patient for a chronic and obstinate cough, which after harassing him for three or four years, ceased suddenly, upon the occurrence of the first of these strange seizures. On that occasion he voided at least a quart of the fat. Since August 1844, he has experienced only two or three slight threatenings of an attack; but lo, his old cough has returned.

Mr. Hicks tells me that a female relation of his own laboured for four or five years under a perpetual dry cough, which was most distressing to herself, and very irksome to the hearers of it. A pint of fatty matter was then passed by stool, and the cough ceased at once, entirely and permanently. This happened more than twenty years ago.

All that we know of the disease seems to amount to this: that it is not a *common* complaint; and that it is not necessarily a *fatal* complaint. Persons who have passed great quantities of fat in that way have lived in good health for many years afterwards. Yet though not *necessarily* fatal, it has frequently been found associated with incurable malignant disease in the duodenum and pancreas. Dr. Prout informs us also that in cases in which a similar oily fluid has been passed through the *urethra*, the *kidneys* have been found in a state of organic malignant disease.

With respect to the treatment in such cases, all the hints I can give you are such as are furnished by the two following facts:—

Mr. Howship, in his book on morbid anatomy, mentions the instance of a lady who was affected with this diarrhœa adiposa, and parted with vast quantities of fat; and who was cured upon the principle of *similia similibus curantur*, for she recovered after swallowing a pint of sweet oil. And Dr. Elliotson, acting on this hint, gave *his* patient, who was labouring at the same time under diabetes, a quarter of a pint of olive oil; and the voiding of fat greatly diminished from that time, and soon ceased entirely.

Whether these were really cures, or whether they were coincidences, is a question which we want larger experience to help us to determine.

When I was speaking of the causes of enteritis, I adverted to the presence of *foreign substances,* as they are called, in the bowels, and to *intestinal concretions.*

There are some points connected with these subjects which I had not then leisure to pursue, but of which you ought not to be ignorant.

Intestinal concretions are very common in some of the lower animals—in horses and oxen especially. They are composed chiefly of hairs which the animals had licked from their own skins, and swallowed. Most of you have seen, I dare say, immense intestinal calculi of this kind, and great numbers of them, in the Museum of the College of Surgeons. The old remedies called *bezoars* were of the same nature.

They occur also, these intestinal calculi, in the human entrails, and in various parts of them: chiefly, however, in the cæcum and large intestines, but sometimes in the stomach: indeed, very large ones have occasionally been met with in the latter organ. Bonetus describes one which weighed nine ounces, and was as big as a hen's egg. Generally they are few in number in the same person; one only, perhaps, exists; or there may be two, or three. Yet as many as thirty have been found together in the stomach in one case: and in another case nine. One of the Monros of Edinburgh (Monro primus) detected twelve in the colon of a boy, during life, by the touch. Monro secundus took a concretion that weighed four pounds from the colon of a woman. They have been known to measure as much as eight inches in circumference. In the twenty-fourth volume of the 'Edinburgh Medical and Surgical Journal' is an account of one long one, or, perhaps, of three that had become united together, weighing twelve ounces. Dr. Turner, of Keith, has published the case of a man, named Gordon, who, in May 1841, passed fourteen large intestinal concretions, and recovered completely. At the close however of the year 1843, he began again to suffer, as he had previously suffered, from costiveness alternating with diarrhœa, and from pain and distention of the abdomen. In September 1847, he one day voided three concretions, each as big as a hen's egg; and on the two following days, fifteen more, varying in size from that of a partridge's egg to that of a filbert. So that in all there came from this patient's bowels thirty-two of these hard bodies.

Now what are these substances, and how do they get there? What is the pathology of the malady? Why, they seem to be formed, in many instances, by the deposit of saline particles, intermixed with animal matter, upon and around some accidental nucleus which has entered the alimentary canal, and there stopped. A gall-stone may form the nucleus: the centre of the calculus has several times been found to consist of pure cholesterine. Those matters over which the gastric juice has no power, and which pass the pylorus unchanged—such as the stones of fruit, husks of grain, many unbroken seeds, portions of bone, and the like. Other of these intestinal concretions are evidently composed of a mass of short fibres, matted, or interwoven together, after the manner of *felt*. These calculi have a somewhat soft and velvety feel, yet are too hard to be much compressed. Sometimes they involve a nucleus, and sometimes they do not. Their composition has been discovered in rather a curious manner. Mr. Clift, who, as you know, had long the main charge of the Hunterian Museum, fancied, after attentively examining some of the specimens there collected, that they might be formed somehow of the beards of oats; and the late Dr. Wollaston, at Mr. Clift's suggestion, I believe, undertook to analyse them somewhat more rigidly; and he found that Mr. Clift's conjecture was well-founded. If you have ever looked closely into the structure of an oat which has been separated from its husk, you may have noticed that one end of it is formed somewhat like a tiny brush; made up of very minute needles or beards. Dr. Wollaston found that these ends were identical in their shape and composition with the fibres of the intestinal concretions.

The accuracy of the result of this analysis is singularly confirmed by the fact that this particular kind of intestinal calculus is almost peculiar to the bowels of Scottish people; among whom, as you know, oats form a very common article of diet, in the shape of oat-*meal*. The man Gordon, whose case is related by Dr. Turner, lived chiefly upon this meal. Even after his first deliverance, neglecting the advice which had been given him, he recurred to his old habits of diet, and two-thirds at least of his solid food consisted of oatmeal. Sections of the concretions presented the appearance of concentric layers, arranged round a thin shell of phosphate of lime. They were mainly composed of hairs of the *caryopsis*, and fragments of the envelopes of the oat; and they were studded internally with minute crystals.

Concretions of the same species have also been found in the intestines of Lancashire persons; and they also use oatmeal a good deal as food. Mr. Children gives an account of some in the 'Philosophical Transactions' for 1822. The fibres were cemented together by mucus; and the concretions contained albumen also, phosphates of lime and soda, and common salt.

The hairs of the caryopsis of the oat have in themselves some natural *felting* property. Dr. Turner has recently sent me a specimen of masses procured from the sieves employed in an oatmeal mill. These masses have a close outward resemblance to the intestinal concretions, only they are less dense, from the absence of the saline element.

I mentioned formerly the danger which attends the incautious or excessive use of magnesia, whether for stomach complaints or for urinary disorders. When this substance is taken habitually, and when due care is not used to ensure its habitual expulsion from the intestines, it is liable to accumulate and concrete there, especially in the cæcum and colon. Large masses of this kind have been met with, composed almost entirely of carbonate of magnesia.

And the habitual use of any other indigestible substance may have the same ill consequence. The seeds of figs; unbruised mustard seeds, which (as I mentioned before) are taken daily by some persons; the woody knots found in certain pears; all these have been known to form the material of concretions, or of hard injurious masses in the bowels.

Now concretions of this kind come at length to produce symptoms by the pressure and distention they occasion, by the ulceration to which they sometimes give rise, and, above all, by the obstacle they oppose to the passage of the contents of the intestines. They generally cut the patient off by exciting inflammation.

But they may exist for a long time without producing any definite symptoms, or any serious injury. And when symptoms do arise, or when we ascertain that such concretions have formed, we are often at a loss for a remedy. From the *colon* we may hope at last to dislodge them: by mechanical interference when they are near the outlet: by frequent injections of warm water, or soap and water, whereby they may be softened or broken down, and washed out, when they are beyond the reach of the finger, or of instruments passed into the rectum. In Gordon's case no means were employed till a late period of his disorder. The concretions were voided, with immediate relief from severe suffering, after the daily injection of emollient enemata, and the free use of opiates, which seem to have had the effect of relaxing muscular spasms of the irritated intestine. The patient again regained a state of robust health.

We have an illustration of the patience of the alimentary canal under the presence of these masses, in what often happens when foreign bodies of some magnitude are swallowed, and lodge in the tube.

In one of the earlier volumes of the 'Medico-Chirurgical Transactions' you may read the history of a celebrated knife-eater. A sailor, in a drunken bravado, swallowed a clasp-knife. This was followed by no immediate bad consequences, and he used to brag of the feat he had performed. And afterwards, either to satisfy the scruples of those who did not believe his assertions, or else for the sake of rewards which some people were thoughtless or cruel enough to offer, or else to win wagers, he stupidly repeated his folly, till he had swallowed some thirty-seven knives of various kinds and sizes. They killed him at last, and their remains were found in various parts of the alimentary tract. But he had no serious symptoms for some time.

Mr. Wakefield has given us an account of a culprit, confined in the Coldbath Fields Prison, who had swallowed seven half-crowns before his incarceration. One day out they all clattered into the pan of his night-chair.

I saw a prisoner myself, some time ago, in the Penitentiary at Millbank, who, after some sickness and tenderness of the belly, voided a half-crown from the rectum. This was in November 1839. He had swallowed the piece of money two years and a half before—viz., in March 1837; and, until within a week of his passing it, he had enjoyed excellent health.

That thieves, swindlers, and the makers of false money should swallow coins, genuine or base, with the purpose of escaping conviction, is intelligible enough. But strange instances have occurred of the habitual ingestion of substances quite indigestible, in the indulgence of a sort of perverse propensity, or vicious trick, until life has at length been destroyed by their accumulation. Masses of long hair, and of string, have been found obstructing fatally the stomach and bowels. The most wonderful case of that kind that I know of is that of a lady who for years had been in the custom of swallowing pins, after somehow bending them in her mouth. Upon her death nine ounces by weight of uncorroded purple-black pins were found in the pyloric end of her stomach, and a tightly-packed mass of similar bent pins in the duodenum, completely blocking it up. Such an occurrence may seem scarcely credible, but you may see these very pins, enclosed in their unnatural receptacle, among the morbid preparations in the Museum of the College of Surgeons.

Single pins, and other hard and dangerous substances, are not seldom swallowed accidentally, by children and others. It may not be superfluous to caution you against a not unnatural error which, under these circumstances, is often committed by parents and friends, and sometimes perhaps by medical men—the error of giving castor oil, or some other aperient, to promote the passage onwards of the thing swallowed. The proper plan—and this the rogues who swallow coins are quite aware of—is to abstain from purgatives and to eat plentifully of a constipating diet, so as to allow the offending substance to get enveloped in solid fæcal matter, and thus to perform its desired journey *safely.*

Before I proceed to any other of the viscera of the abdomen, I may as well take such notice as the nature and limits of these lectures requires and admit, of the subject of *worms*; in which subject the intestinal canal is more concerned than any other part of the body. It seems a strange, as it is a somewhat humiliating fact, that the human body should furnish food and a habitation for many of the inferior creatures; not only after death, but while it is yet alive. The parasitic animals which thus prey upon man have been much studied from time to time, and especially of late, in their relations to natural history: and some of the facts that have been ascertained respecting them you ought to be acquainted with. But I shall pursue the subject no further in this place than it concerns us as pathologists and physicians. Its natural history will, no doubt, be fully taught you by the professor of comparative anatomy.

It is, then, a notorious fact that numerous parasites do crawl over our surface, burrow beneath our skin, nestle in our entrails, and riot, and propagate their kind, in every corner of our frame: producing oftentimes such molestation and disturbance as require the interference of medicine. Nearly a score of animals that have their dwelling-place in the interior of the human body have been already discovered and described: and scarcely a tissue or an organ but is occasionally profaned by their inroads. Each, also, has its special or its favourite domicile. One species of *strongle* chooses the heart for its place of abode, another inhabits the arteries, a third the kidney. Myriads of minute worms lie coiled up in the voluntary muscles, or in the areolar tissue that connects the fleshy fibres. The *guinea-worm* and the *chigoe* bore through the skin, and reside in the subjacent reticular membrane. *Hydatids* infest various parts of the body, but especially the liver and the brain. A little *fluke*, in general appearance much like a miniature flounder, lives, steeped in gall, in the biliary vessels. If you squeeze from the skin of your nose what is vulgarly called a maggot—the contents, namely, of one of the hair-follicles—it is ten to one that you find, in that small sebaceous cylinder, several animalculæ, extremely minute, yet exhibiting under the microscope a curious and complicated structure. Even the eye has its living inmates. But it is, I repeat, in the alimentary tube that we are most apt to be plagued with these vermin.

Independently of minute scientific divisions into genera and species, there are some broad lines of distinction between these creatures. Thus, some kinds of worms occupy, as I have said, the interior of our bodies; these are called accordingly *entozoa*: some dwell externally, and are named *ectozoa*; or, more properly perhaps, *epizoa*.

There are five or six sorts of *intestinal* worms, sufficiently common to make it likely that you will meet with some or most of them in your future practice. I shall, on that account, direct your attention first of all to them.

1. A frequent tenant of the human intestines is the round worm, so like in shape, size, and general appearance to the common earth-worm. It is from this species, no doubt, that the whole class are called *worms*. This round worm is often erroneously termed a lumbricus. It is a species of ascaris, and it has been named by naturalists *ascaris lumbricoïdes*—the ascaris that is like a lumbricus. Oxen and hogs are subject to this entozoon.

2. The *ascaris vermicularis*; or the *oxyuris vermicularis*. These animals resemble slender maggots rather than worms. They are often called simply *ascarides*; or, in the vernacular,

thread-worms; and to the naked eye they look very like bits of white thread.

3. The *tricocephalus dispar*: also a small worm, but longer than the last; its vulgar denomination is accordingly the *long* thread-worm.

4, 5, and 6. Three kinds of *tænia*; long, flat, articulated animals, resembling pieces of tape. The *tænia solium*; the *tænia medio-canellata*; and the *tænia lata*. These are ill-named, for the first is not always solitary, which is what I suppose Linnæus to have meant by the epithet *solium*; and the last is not truly a tænia at all. The first two are of common, the last of only occasional, occurrence in this country. Many of the inferior animals are infested each with its own peculiar species of tape-worm. From their band-like appearance, the tæniæ are also styled *cestoid* worms.

Of all these I proceed to mention a few more particulars.

The ascaris lumbricoïdes, or round worm, is, I say, very like the common earth-worm, and used to be thought identical with it. It runs from five or six inches to about a foot in length, and it is of a reddish-brown colour, with a tinge of yellow. The female worm (for they are of both sexes) is much more common than the male, which is smaller also, and may be distinguished by a curved state of its tail, and by the genital organs. Sometimes young ones are met with, about an inch and a half long.

I shall not go into any minute description of the anatomy of these worms. You cannot mistake them, except for earth-worms; and the points of distinction between the two, when known, are easily perceived. The earth-worm, then, is redder than the intestinal worm, and less pointed at its two ends. The mouths of the two differ much. That of the earth-worm is a short longitudinal fissure, or slit, placed on the under surface of its small rounded head. In the ascaris lumbricoïdes, the mouth is situated at the extremity of the worm, is of triangular shape, and is surrounded by three little knobs or tubercles.

It is curious that similar differences, only reversed, exist with respect to the other aperture of the alimentary canal, the anus. In the earth-worm this is terminal, at the very end of the cylinder: in the ascaris it is a transverse slit *near* the extremity, and on the under surface of the animal.

Again, the earth-worm has rows of little projections, like bristles, upon its under surface; feet they may be called, for they appear to serve the purpose of locomotion. In the parasite there is nothing resembling this.

By attending to these plain marks, you may avoid being deceived by impostors, who pretend that they are afflicted with worms, and to prove their case bring you an earth-worm or two in a bottle.

The *habitat* of these worms is the small intestines. They may, and do, pass upwards into the stomach, or downwards into the large bowel: in either case they are generally soon voided. Sometimes they are vomited up: but they have been known to *creep* into the œsophagus, and thence into the nostrils. Andral states that he saw a case in which a child was strangled by one of these worms, which had turned back and become entangled in the larynx. They have been found also in the excretory ducts of the liver. This Andral has witnessed; as has also Dr. Baron in this country.

It was formerly thought that these animals were capable of perforating the coats of the intestine: but that opinion is now generally exploded. They do not appear to possess the means, if they have the inclination, to bore through. What gave rise to this notion was the circumstance of their sometimes passing out of the bowel, through ulcerated or other openings, into the peritoneal sac; or into the vagina or bladder; or out of the body through hernial apertures.

The number of these worms existing at the same time in the same person is very variable. The late Dr. Hooper mentions a girl, eight years old, who voided upwards of 200 in the course of one week. An instance is recorded of a soldier who passed 367 in six days. Another patient got rid of 460 in a fortnight.

Fifty or sixty have been found in the same dead body. They often lie in packets. The corresponding portion of mucous membrane has in some cases been red, in others quite natural. Most commonly they are few in number. Sometimes two worms are met with; sometimes one only. So that we cannot infer with certainty that because one such worm has been voided, more remain behind; although that is always probable.

This worm is more common in the early periods of life than afterwards.

The other species of ascaris, the ascaris vermicularis or thread-worm, resembles the former in some respects, but differs from it remarkably in size. Here also the female is longer and larger than the male; the one being perhaps half an inch in length, the other scarcely two lines, and very slender.

The thread-worms live principally in the rectum, and sometimes are collected there in vast numbers; thousands: and they pass out, or are ejected, matted together with mucus in the shape of balls, or entangled in portions of excrement. Sometimes they emerge of their own accord, and crawl about the neighbourhood, getting into the vagina in females, and even into the urethra, and causing intolerable irritation, itching, and distress.

They are seen, when recently expelled, to be very lively; moving their anterior extremity briskly and continually. To this restlessness and activity the animal owes its name, which is derived from the Greek word *ἀσκαρίζειν*, to leap. The Germans call it *springwurm.*

This worm also belongs chiefly to infancy and childhood. It does sometimes infest adults; but generally as the patient grows older the animals cease to trouble him, whether curative means are employed or not. Bremser, however, knew a person eighty years old who was nearly killed by them.

The third kind of these round worms is the *long* thread-worm; the tricocephalus dispar. It is from an inch and a half to two inches in length. One extremity, that to which the head belongs, is extremely fine and small; and then suddenly bulges out a thicker body. The thinner portion is about twice as long as the thicker. Its name is derived from this variation of size. *θρίξ*, a hair, and *κεφαλή*, the head; the portion to which the head is appended being as fine as a hair. At one time the head was mistaken for the tail, and then the animal was called *tricuris*, from *θρίξ*, and *οὐρά*, the tail. The thicker or body part is rolled up in a spiral form, especially in the male, the female being straighter. This worm is of a white colour, unless tinged by its food. It also affects the large intestine as its place of abode; but the opposite end of that gut, the cæcum, is its favourite spot. It is sometimes met with in great numbers, each attached to the mucous membrane by its head; the body hanging loose.

Although generally overlooked, it is said to be extremely common, and to occur in most bodies. I have seldom seen it; but then I have never hunted for it. It infests the dog, the fox, the monkey, and other mammalia.

This species of entozoon attracted a good deal of attention nearly a century ago; it being then first observed in Germany during the prevalence of an epidemic fever, which was characterised by profuse mucous diarrhœa. Rœderer and Wagler have given an excellent account of this disorder, under the title of *morbus mucosus*. It was thought to have been excited by these worms, which were found in abundance in the cæca of the dead. This opinion must have been erroneous, for the animals had been noticed in other places, long before; and they produce, in general, no appreciable inconvenience.

The tape-worms are more formidable beasts. With a strong general resemblance between them, there are marked particular distinctions.

The tænia *solium* has a minute hemispherical head, which is furnished anteriorly with a double circle of little hooks; and behind these lie four suction disks, whereby it adheres to the inner surface of the intestine. Its body is long and flat, of a whitish colour, composed of many pieces curiously articulated together. The articulated pieces are quadrilateral. Very short, small, and indistinctly marked in the creature's neck, they become gradually larger and square as the distance from its head increases, and at length are longitudinally oblong. So that the worm is narrow and thin at its anterior extremity; one-third or one-quarter of a line perhaps in breadth: while at its broadest part it may be from three to six lines wide. The young tæniæ *seem* to be merely wrinkled, but they also are really articulated. The segments of the animal, or the *joints* as they are called, have foramina on their margins, leading to ovaries within. The foramina, which are very conspicuous, are placed alternately on the one side of the animal and on the other: on the right edge of one joint, on the left of that next to it. This arrangement is, however, subject to occasional irregularities. Each joint is let in, as it were, to that immediately in front of it; and the connection between them is not very firm. It is less firm in proportion as the animal is older, and as we approach its posterior extremity: so that the ultimate segments fall off and are apt to come away, by stool, separately. They have somewhat the appearance of the seeds of cucumbers or gourds; and the parasites, for that reason, are sometimes called *cucurbitine* worms. Blumenbach and others have supposed that each articulated piece was a distinct worm: but that is not the case. The head of the animal, and the way in which it grows, and multiplies its kind, forbid this belief.

Within each joint—within at least each of the larger and hinder joints—is contained a complicated male and female apparatus, capable of producing thousands of fertile ova: and the spontaneous separation of these riper segments appears to be a natural provision for disseminating the minute eggs. Meanwhile, as the animal shortens by thus shedding its hindmost joints, some of those which are anterior divide into two by a transverse fissure, which two, after attaining a certain size again divide, and in this way new joints are formed, and recede gradually from the head. But at a certain distance from the head, the divisions and subdivisions cease, and the whole nutritive power is expended in the development of the organs of generation: and at length ova begin to fill the uteri of the joints. Such is the process (as I learn from Professor Owen's Lectures) which has been actually observed by Dr. Eschricht, of Copenhagen, in a species of tape-worm (the *Bothriocephalus punctatus*) extremely common in a sea-fish called *Cottus scorpius*; and it may be presumed that the increase and generation of the human tape-worms proceed in a similar manner.

You will observe that this mode of growth and of multiplication is closely analogous with that of many pod-bearing plants.

Specimens of this worm are preserved upwards of twenty feet in length. Much exaggeration seems to have existed formerly about its size. It has been said to measure 150, and even 300 feet. In all probability separate portions of several worms have been estimated as forming parts of one and the same worm. There is one case well authenticated (it is cited by Bremser from Robin), in which a tape-worm was found to extend from the pylorus to within seven inches of the anus; adhering to the mucous membrane all the way. The animal has the power of motion. Its movements are felt by the patients, within them. When recently expelled, and placed in tepid water, it may be seen to shorten itself; nay, portions protruding many feet from the anus have been known to draw themselves back again.

This kind of worm is more frequent in adults than in children: yet it is said to have been sometimes met with even in the fœtus. It has been badly named *ver solitaire*, for it is not always single. It is not only found in company with different worms, but also with others of its own species. Its natural place of abode is the small intestines: but it extends sometimes into the large, and sometimes into the stomach. Vandoverer declares that after an emetic one of his patients vomited forty Dutch ells of the worm, and might have got rid of more 'if he had not been afraid of puking out all his guts, and for that reason bit the worm off.'

The tænia *medio-canellata* is shorter, broader, and thicker. Its head is unlike that of the tænia *solium*, inasmuch as it is not armed with hooks. The more appropriate epithets of *armata* and *inermis* have therefore been applied by some writers to these two varieties of tænia. The head and black suckers of the last named are very large, and it adheres to the intestine with extreme tenacity.

The head of the so-called tænia lata—which in reality is a species of the Bothriocephalus of naturalists—differs from those of the other two in having neither a coronet of hooks, nor a circle of suckers; in that its joints are shorter and broader, and adhere together in a different manner; and that the pores leading to the oviducts are situate, not on the edge of each joint, but in the centre of its flat surface. This variety is not so easily broken across as the former; and therefore its segments are less liable to be voided in a separate form. It is shorter also than the tænia solium. Fifteen feet have been supposed its average length. Marvellous stories, however, are told on this head. Boerhaave declares that he effected the expulsion of one, from the bowels of a Russian, which was 300 ells long.

The geographical distribution of these species of tænia forms a curious part of their history, and throws some light upon the long controverted question of their origin. In England, Holland, Germany, and Denmark, the tænia *solium* is common, and the tænia lata very rare. In Russia, Poland, and Switzerland, it is just the reverse; the tænia *lata* prevails, the tænia solium is seldom seen; while in the French provinces adjoining Switzerland the one species is nearly as frequent as the other.

LECTURE LXVIII.

Tape-worms continued. Cyst-worms. Hydatids. Trichinæ. Guinea-worm. Liver-fluke. Strongulus gigas. Spurious worms. Spontaneous generation. General symptoms of the presence of Intestinal worms. Particular symptoms, and remedies, of the common Round worm, of Thread-worms, of Tape-worms.

I RESUME the subject of tape-worms. What may be called their family history is so curious and interesting that I will endeavour to explain it before I go further. To do so I must quit the intestines for awhile, and speak of a class of entozoic forms, called from their appearance *cyst-worms*, or *bladder-worms*, which infest the inner substance, or the closed cavities of the body, and which are therefore quite distinct, as to their locality, from tape-worms that occupy parts communicating with the external air. These single bladder-like worms, having short retractile necks, bear the generic name of *cysticercus*. Some similarity between the head of the cyst-worm and the head of the tape-worm had been noticed by Wepfer towards the close of the seventeenth century; but the true relation between the two kinds of worm was entirely unknown and unsuspected until M. Steenstrup, a Swedish naturalist, announced, in 1842, the doctrine of *alternating generations*.

'The young of most of the entozoa undergo metamorphoses.' Certain entozoa, the parasites of certain animals, have been ascertained to present merely a transition state of other entozoa, the parasites of certain other animals. In the last edition of his Lectures, Professor Owen stated the probability that the whole of the *cystic* family of entozoa were nothing else than the *larvæ* of the whole *cestoid* family: a doctrine which may now be said to be fully established. It was first suggested by definite resemblances and relations subsisting between the two sets of entozoa on the one hand, and between the two kinds of animals respectively infested by them on the other. The first, or lower sort of these entozoa (the cystic), are so far imperfect that they are sexless and sterile; have no generative organs. The second, or higher (the cestoid), possess, as I have told you, an apparatus for most abundant reproduction. Parts of the first have a close rudimental likeness to corresponding parts of the second: their heads, especially, exhibit a striking conformity of structure. This is the sort of relation which is observed between the *entozoa*. That which exists between the two *infested animals* is the relation of natural prey and devourer: and it involves the noteworthy fact that, in almost all cases, the alternation takes place reciprocally between herbivorous animals on the cystic side, and carnivorous or omniverous on the cestoid. To take an example by way of illustration. The common domestic mouse and rat are subject to a species of cyst-worm, the *cysticercus fasciolaris*. The cat, which preys on these animals, is subject to a species of tape-worm, the *tænia crassicollis*. Now this tænia, Professor Owen informs us, is remarkable, among tæniæ, for the disproportionate size of its head, its short and thick neck, the position of its four suctorial disks, and the shape and number of the hooklets of its proboscis: and all these peculiarities are repeated in the cysticercus of the mouse and rat, which cyst-worm he regarded accordingly as the larval form of the tape-worm of the cat. 'All the cysticerci manifest their affinity with the cestoidea by the organisation of their heads.' I may add that their necks are divided into segments resembling the joints of the tæniæ. They look, in fact, part hydatid, part tape-worm.

This doctrine has since been confirmed by the experimental researches of several eminent naturalists, both German and English. Küchenmeister was the first, I believe, to feed dogs and cats upon flesh that contained living cyst-worms. After a while he found corresponding tape-worms in the intestines of those dogs and cats. But he was not content with practising upon these *corpora vilia*. He conceived and executed the horrid and revolting project of sowing (as it were) cyst-worms in the human bowels. He contrived to mix cysticerci, collected from pigs, with the food of a criminal under sentence of death for murder, passing them off as grains of rice in warm rice soup, as bits of paste in vermicelli gravy, and as small lumps of fat in black-puddings. These were swallowed by the unconscious and wretched man at various periods from seventy-two to twelve hours before his decapitation. Forty-eight hours after it, ten young tape-worms were seen attached by their hooks or their suckers to his small intestines.

Similar experiments upon the lower animals were carefully repeated, with similar results, by Professor Siebold. The transformation of the imperfect cyst or bladder-worm into the perfect tape-worm was thus satisfactorily demonstrated. To clench the proof, the converse experiment has since been often made. For instance, joints full of ripe ova, from the tape-worm of a dog, were given with their food, by M. Haubner, of Dresden, to certain lambs. In about a fortnight all these lambs (and no others in that flock) became affected with what is called 'the staggers;' and when they were killed, at different intervals, cyst-worms, peculiar to the sheep, were discovered, in various stages of growth, in their brains, and in other parts of their bodies. Again, mice were induced by Professor Leuckart, of Giessen, to eat mature joints of the tænia *crassicollis* of the cat; and the livers of those mice were found thereafter to be studded with the cysticercus *fasciolaris.* Swine, fed with the eggs of the tænia solium, and killed some weeks afterwards, were occupied throughout their whole bodies with the cysticercus *cellulosæ.* The cysticercus *pisiformis* of the rabbit and hare becomes in the fox, which eats them, the tænia *crassiceps.* In fact each species of cysticercus thrives only in the intestines of particular species of animals.

Reasons were suggested, in 1852, by Dr. Nelson, for thinking, what is now indisputable, that the cysticercus *cellulosæ,* hatched in the bodies of measly pigs, is matured in the human body into the tænia *solium.* In like manner the cyst-worms found in calves and oxen produce in man the tænia *medio-canellata,* or *inermis.* Fat pork is eaten raw by many farm-labourers in this country, and with it the cystic larva of the tænia solium must be often transplanted into their bowels. The taste for raw or half-cooked meat is not uncommon. According to Dr. Cobbold the tænia *inermis* is much oftener met with in English bowels than the tænia *armata*; so that raw or rear beef and veal should be not less rigidly avoided than raw or half-cooked pork. The larval form of the tænia inermis, the cysticercus *bovis,* is very much smaller than that of the tænia armata, so that the measly condition of pork is more perceptible than of beef or veal, and as the full-grown animal must have been oftener exposed to the risk of swallowing with its food the tænial eggs, beef is more frequently found measly than veal. It is a reasonable conjecture that the tænia *lata,* or bothrio-cephalus *latus,* which is endemic on the shores at the head of the Gulf of Bothnia, and frequent in the neighbourhood of the Swiss lakes, may be the adult form of a cystic entozoon from some marine or fresh-water animal. Professor Siebold believes that certain minute worms which infest a species of *slug,* are the produce of ova from the tænia of some bird. The ova are voided with the excrement, and may adhere to and hatch in the body of the first slug that crawls near them: and then, if they have the additional good luck to be swallowed, with the slug, by the proper bird, they regain a fitting nidus for their further and perfect development. Our red grouse, a bird peculiar, I believe, to the British Islands, are very subject to tape-worms. In some years thousands of them die of this distemper. If any of you can succeed in tracing the birth and nursery habits of these destructive parasites, so as to prevent their final development in the grouse, you will secure the praise and thanks of a great number of English sportsmen. None of the gallinaceæ disdain or abstain from animal food, though they all live mainly upon vegetable. It seems probable that the grouse, the young birds especially, pick up molluscs, planariæ, or worms or insects of some kind, in the moister parts of the moor, and pick up with them the cyst-worm, which, in the eater, becomes a tape-worm. The tænial disease in these birds has certainly an intermittent endemic prevalence; and it is further probable that in certain years, or certain places, the creatures which nourish the cyst-worm may, under peculiar atmospheric or other obscure influences, be endemically abundant.

Of the innumerable eggs produced by the human and by other tape-worms, very few can ever reach the fitting nidus for their first hatching into the cystic state, and fewer still the remoter place of their final mutation into perfect tæniæ. Professor Owen remarks that many of these little ova, in great cities especially, must eventually find their way into streams of water, for whose minute inhabitants—or for terrestrial creatures under other circumstances—they probably furnish food: being thus analogous to the seeds of the fruitful *cerealia*, which 'minister far less to the perpetuation of their own species, than to the sustenance of man.'

After all, it is a mortifying rebuke to human vanity, that for the birth, the nurture, and the support of a repulsive, a noxious, and (so far as our finite faculties can perceive) a useless parasitic worm, the subservience should be required of two other creatures of higher grade, and (as we speak) of nobler dignity in the scale of life; one of which is no less a being than that 'paragon of animals,' Man.

The cyst-worm being demonstrably the offspring, and the transitional larval condition, of the tape-worm, how, it may be inquired, does it get to those inner parts of the body in which it is found, so to speak, imprisoned. It is believed that the outer covering or shell of the ovum is digested by the gastric fluids, and that the emergent embryo bores its way through the intestinal wall towards, and into some neighbouring viscus, which it seems instinctively to prefer, or into the stream of the blood, whereby it is carried and deposited in this or that organ. It is seldom that a cyst-worm is met with in an animal that is liable to the corresponding cestoid-worm. Yet this deviation from the ordinary rule does sometimes occur. The cyst-worm of the pig, productive of measly pork, the cysticercus *cellulosæ* (an adjectival epithet agreeing, I suppose, with *telæ* understood) —this cyst-worm is an occasional, though a rare, inhabitant of the human body. How does this happen? Possibly one or more of the ripe joints of the tape-worm may ascend into the stomach, where the embryo is set free by the digestion of its cases; or a pod may be broken by accident or violence while yet within the bowel; or some of the eggs may chance to be swallowed by man. In either case the liberated embryo pursues its natural instinct, migrating in the usual way, and gets fixed in the uncongenial soil of a wrong animal. This *error loci* sometimes takes place in the organ of vision.

A most remarkable instance occurred a few years ago, in Glasgow. In the eye of a child, who had suffered repeated attacks of ophthalmia, Mr. Logan discovered one day, to his extreme astonishment, a semi-transparent body, about two lines in diameter, floating unattached in the anterior chamber. It seemed almost perfectly spherical, except that from its lower edge there proceeded a slender process, of a white colour, with a slightly bulbous extremity, which appeared to be heavier than the globular part, for it always turned downwards. This head, or neck, was seen to project or elongate itself from time to time; and occasionally it was drawn up and completely hidden in the cystic portion. When the patient sat still, in a moderate light, the animal covered the two lower thirds of the pupil. 'Watching it carefully (says the gentleman who has recorded the case), its cystic portion was seen to become more or less spherical, and then to assume a flattened form; while its head I saw at one moment thrust suddenly down to the bottom of the anterior chamber, and at the next drawn up so completely as to be scarcely visible.' The child's head was now turned gently back, and instantly the parasite revolved through the aqueous humour, so that its head fell to the upper edge of the cornea, now the more depending part. Upon the child's again leaning forwards, it settled, like a little balloon, in its former position, preventing the patient from seeing objects directly before her.

The animal was sedulously watched for three weeks; and no other change was noticed than a slight increase in the bulk of its cystic portion. In six weeks it had evidently grown bigger, the eye became injected, and the iris less free in its movements; and pain ensued. Extraction of the worm was then attempted; but the patient was unruly; the lens was forced out, and the animal ruptured and expelled in shreds: the iris became entangled in the wound of the cornea, and vision in that eye was spoiled.

This cyst-worm has been met with in the brain also, in the heart, and in some other muscular parts. It is generally about half an inch in length.

Among these larval parasites must be reckoned, I believe, those important structures popularly known as *Hydatids*. Bags or bladders of water they look like. They are also called *acephalocysts*, headless bags. They possess, however, very peculiar characters and qualities of their own. In size they vary from the bigness of a pea to that of a swan's egg. They look like, or rather they are, spherical membranous bladders, filled with a thin colourless liquid which holds in solution a large quantity of common salt. Floating in this liquid, and therefore enclosed within what must be considered the primary hydatid, there are usually found smaller ones, of various sizes; sometimes few in number, sometimes in countless multitudes. This is a consequence of the peculiar mode of propagation of these animals, which is by what is called *gemmation*. The wall of the cyst is laminated, and the young hydatids bud from between its layers. In the species that infests the human frame they are born into the cavity of the parent; in some other species they are detached externally. We find, then, a parent bag, containing other smaller bags, which again are pregnant, as it were, with their own offspring, the grandchildren of the primary cyst: and so on, somewhat after the manner of a nest of pill-boxes. The primary cyst itself lies in close contact with, but does not adhere to, an external sac, formed apparently by the pressure of the enlarging hydatid upon the texture of the organ in which it is imbedded. These hydatid tumours are much more common in the liver than in any other single organ. I lately mentioned the case of a woman, Harriet Baldwin, who died in the Middlesex Hospital, and whose liver contained thousands of these globular bodies. The enlarged gland had completely sealed up, by its pressure, a portion of the inferior cava. They are found also in the lungs, in the spleen, in the mesentery; more seldom in the brain, in the kidney, in the heart; and occasionally in other parts of the body.

Within several of the transparent hydatids which were taken from the liver of the woman Baldwin, a number of small, opaque, white grains were visible. These were examined by Mr. Tomes and myself, under the lens of his powerful microscope. They were plainly minute animals: baglike, with an orifice or mouth which, in some instances, protruded a little from the bag, in others was evidently contracted and drawn inwards. Around this orifice was arranged a circlet of small, flat, hook-like rays, somewhat resembling a vandyke collar. Many of these rays, or spines as they have been called, were detached, and lying loose in the surrounding liquid. Being then very ill-informed in this department of natural history, I paid less attention to these creatures than they deserved;—fancying indeed that they might be common and well known, or rather that they were juvenile hydatids. I first became aware of my mistake through reading a paper, by Mr. Curling, in the twenty-third volume of the 'Medico-Chirurgical Transactions;' where he more minutely describes precisely similar phenomena. The subject has since attracted the attention of many observers, and it appears certain that in almost every hydatid are included more or fewer of these animalcules; which from the spines encircling their heads have been named *echinococci*—hedgehog mites. In some cases, however, no echinococci have been discoverable. Of this difference I can give you no explanation.

Now these echinococco-cysts, as Professor Owen would have them called, are referable, it is said, to a single species of tapeworm, which infests the dog and the wolf only. If so, they must be derived from the dog when they occur in the human body. The ova are supposed to be swallowed in water or food, with which they have somehow got mixed. The parent tænia has been called the tænia echino-cocci; it is the smallest of all the known tæniæ. Its larvæ have this further peculiarity, that they are found dwelling in a great variety of animals. According to Dr. Cobbold they infest 'men, monkeys, sheep, oxen, deer, camels, the giraffe, and other ruminants; also the horse, ass, zebra, several feline animals, and perhaps the squirrel. Von Siebold has described an example of echino-coccus (larva) from the lung of a turkey.'

Hydatids are fearfully common in Iceland: a fact intelligible enough when we learn that for every inhabitant there are eleven head of cattle; that every peasant possesses on an average six dogs; that cattle, dogs, and men live together, crowded during winter within a small space; and that cleanliness is not one of the virtues of the Icelanders.

Minute in their origin, hydatids may enlarge and multiply till the tumour formed by them attains an enormous size, and at length destroys life by its bulk and pressure; or by bursting and pouring the contents of the cyst into the serous sacs, or into the large blood-vessels in the neighbourhood. Of course the direct consequences of the pressure will depend much upon the parts occupied by the hydatids. You may readily imagine what kinds of symptoms are likely to ensue when they are lodged within the abdomen; within the less yielding thorax; within the unyielding skull. Occasionally the contents of the cyst find their way out of the body by some road, always more or less perilous. Thus a patient of Dr. Macmichael's died in the Middlesex Hospital after expectorating for some time quantities of yellow fluid, consisting partly of pus, partly of pure bile. On examination of the dead body, a cyst was found in the liver, containing pus and many hydatids. Even when the discharge of the hydatids takes that dangerous direction, it is not necessarily a fatal direction. In several instances I have known a hepatic abscess empty itself through the lungs, without killing the patient: and I may give you one very interesting example of the escape of hydatids from the liver, with a prosperous event, both by the way of the lungs and by the way of the alimentary canal.

In April, 1848, a surgeon from the country, aged thirty-eight, came to my house, and told me the following curious history:—

For eight or ten years, at intervals varying from ten to fourteen months in duration, he had suffered a series of attacks precisely resembling such as are commonly produced by the passage of a biliary concretion through the excretory ducts of the liver. In May, 1847, just after one of these attacks, while searching for a gall-stone, he discovered among the excretions from the bowels, two or three small hydatids. In July he again experienced for four or five days the same train of symptoms, and then vomited a green hydatid, as large as a pigeon's egg, but more elongated. Soon afterwards a short dry cough set in, with pain in a circumscribed spot on the right side of his back, and also at the tip of the right shoulder. These pains became very severe, and in the first week of August he expectorated, with coughing, matters which were yellow in colour, and bitter in taste ; and after another week he spat up a hydatid like a grape. Pressure upon the hepatic region posteriorly, always produced an immediate paroxysm of coughing. This state continued for six or seven weeks, and during that period he expectorated some hundreds of hydatids, of which there are specimens preserved in the Museum of the College of Surgeons. At the conclusion of the fourth week he began to cough up from time to time large quantities of bile. The hydatids ceased to appear towards the end of November ; the bile, in the second week of February, 1848. Once he coughed up four ounces of blood.

When I saw this gentleman he still had some cough, and expectorated mucus streaked with blood, and felt some pain in the situation of the liver. He was recovering his lost flesh. His pulse had never been much accelerated : nay, while he was coughing up bile, and voiding none at all by the bowels, it fell to forty-nine beats in the minute, and remained at that rate for five or six weeks, until bile again began to be visible in his stools. During the same period the urine was very dark-coloured, and extremely scanty, not exceeding six ounces in twenty-four hours.

Milk and eggs appeared always to cause a great increase in the quantity of bile secreted and discharged. A similar increase was produced within five minutes of his taking any kind of acid. Magnesia as constantly reduced the quantity.

It was a remarkable part of Mr. L——'s history, that throughout all this illness he carried on a large country practice. For weeks he scarcely slept at all. While at the worst, and living upon a very slender diet, he began for the first time in his life to have acne rosacea of the face.

This account I took down from his own lips: and I am happy in being able now, twenty-three years later, to add that he is still in good health, and engaged in the active exercise of his profession in Northamptonshire.

It is clear, from what has been said, that we are in much more peril from the larval, half-hatched tenants of our bodies, than from the mature and perfect parasites themselves. These may be regarded as being mostly tenants at will, and capable of summary ejectment. But the occupants whose natural destiny cannot be fulfilled unless we ourselves chance to get eaten, are tenants for life; too often they are quite inaccessible, and their presence involves not only their own destruction, but that of their unlucky landlord. *Prevention*, therefore, is what really demands our attention; and in respect of this most persons are either ignorant or careless.

I have said that hydatids are much more commonly met with in the liver than in any other part of the human body. Now, you may conjecture that an enlarged liver contains a hydatid cyst, if upon its smooth surface you can feel a firm, elastic, globular prominence, painless, even under pressure, and not obviously impairing the general health of the patient. When you further make out that a projecting boss of this kind has been for some time stationary—and still more when it has been gradually diminishing in size—let it alone. But if you learn that it has been steadily though perhaps slowly increasing, you will do well, without much delay, to puncture it with a fine trocar, and to ascertain the nature of its contents. If a clear limpid fluid comes forth, containing no albumen, no phosphates, and holding in solution a considerable quantity of common salt—which may readily be detected by testing it with nitrate of silver—you may be sure that the tumour was caused by a hydatid cyst; 'for these characters,' as Dr. Murchison has justly remarked, 'belong to no other fluid of the body, healthy or morbid.' The two main dangers belonging to this operation are the occurrence of acute peritonitis from the escape of some of the irritating hydatid fluid into the peritoneal sac, and suppuration of the cyst from the admission of air. Both of these may commonly be averted by using a very fine trocar; which should be removed before the whole of the fluid is drawn off, the punctured portion of the abdominal wall being meanwhile pressed gently against the cyst. The patient should be kept strictly at rest in the recumbent position for the next forty-eight hours. The withdrawal of the liquid—which does not re-accumulate—is followed by the death both of the cyst and of its offspring, and neither of these undergo putrefaction.

All risk of the intrusion of air into the cyst during the operation of puncturing may be prevented by using the 'Aspirator,' invented by Dr. Protheroe Smith.

Sometimes, indeed, the cyst refills, and a fresh puncture becomes necessary; and the issuing fluid is then found to be more or less puriform. Under these circumstances the cyst requires to be laid more freely open, and to be kept open so long as pus continues to escape.

The notion had long existed that a galvanic current, or an electric shock, passed through the cyst containing these creatures, might kill them. Quite recently, Dr. Hilton Fagge and Mr. Durham have treated eight cases of hydatid disease of the liver by 'electrolysis.' Two needles were, in each case, passed into the tumour, and were connected with the negative pole of a modified Daniells' battery of ten cells. The positive pole, terminating in a moistened sponge, was placed upon the abdomen. The current was allowed to pass for a period varying from ten to twenty minutes. The needles were then withdrawn. The operation was followed in most of the cases by rapid diminution of the tumour, and ultimately by recovery.

The comparative eligibility of this method of electrolysis, and of the simple puncture of the cyst with a very fine trocar, has yet to be determined by further experience.

These hydatid cysts sometimes increase to an enormous size. Dr. John Harley records a case in which nineteen pints and a half of fluid were taken away. The case at length did well.

Hydatid tumours are quite incapable of cure by medicine given internally. It has, indeed, been fancied that the entozoa in these cases might be poisoned by alcoholic potations, or by drugs that are not seriously prejudicial to man, such as mercury, iodine, turpentine. But these, I fear, are mere dreams of our baffled art. Frerichs records that he was unable to detect any trace of a compound of iodine in the fluid of a hydatid cyst removed from a woman who had been taking iodide of potassium for many weeks; and Dr. Murchison testifies to similar experience on his part. Sometimes the hydatids are let out, to the surprise perhaps of the operator, who only thought that he was dealing with an abscess which required puncturing. Mr. Arnott put a lancet into a fluctuating tumour in the epigastrium of one of my patients; very offensive pus issued, with the shrivelled skins of sundry defunct hydatids. The cyst was situated, I believe, in the liver. Although the orifice was slow to heal, the patient ultimately got well. Sometimes the whole colony perishes while yet hid in its dwelling-cave, all the enclosed hydatids losing their vitality, and shrinking up as their fluids are absorbed. It may be that they increase in number and in size till the crowding and pressure prove fatal to them. Their former domicile now becomes their tomb; and effectually precludes any contamination of the fluids of the body, or irritation of surrounding textures, by their remains. This may be deemed a sort of natural cure of such a malady. In the year 1827 I accidentally detected in the liver of a young nobleman a large tumour, which, from its size, shape, position, and smoothness, I concluded was produced by a hydatid cyst. He was not even aware of the tumour, and experienced apparently no inconvenience from its presence: it might have been there for years. After some time the tumour became somewhat less prominent. The patient died, of another disease, in 1849; and an old cyst was found containing the dry and withered hulls of numerous extinct hydatids. In similar cases the hooklets of the echino-cocci, scattered through a putty-like substance, have been recognised in the collapsed cyst, when no other trace remained of its original interior condition.

When speaking of thread-worms I should have included among them the *trichina spiralis*, a microscopic parasite, dwelling sometimes in myriads, in the human body. It was first discovered in this country; and the extreme interest and importance of the discovery have of late years been painfully manifested in Germany, where endemic outbursts of this kind of vermination have occurred in which the multitudinous occupation of the body by the parasites has given rise to severe symptoms, and in very many cases proved fatal to life. In the year 1833 Mr. Hilton, of Guy's Hospital, noticed 'a peculiar appearance in human muscle, probably depending on the formation of very small cysticerci.' In 1835 Mr. Paget, then a student at St. Bartholomew's Hospital, made out, and described at a meeting of the Abernethian Society, the true nature of the mouldy or speckled appearance of portions of human muscle, such as Mr. Wormald, the demonstrator of anatomy there, had frequently observed. Other portions of the affected muscles were examined by Professor Owen, who found that each minute speck was a shuttle-shaped cyst, containing a very minute cylindrical worm, coiled up in two, or two and a half, spiral turns. The worm measures, when unrolled, no more than $\frac{1}{30}$th of an inch in length, and $\frac{1}{700}$th of an inch in diameter; and of course requires, for a satisfactory examination, to be seen through a microscope. The longer axis of the containing cyst lies between, and parallel to, the fibres of the muscle.

The muscles thus beset with these parasites are the voluntary muscles: and those which lie superficially are fuller of the worm than the deeper seated. The pectoralis major, latissimus dorsi, and other large flat muscles, usually present them in great abundance. They have been detected in the muscles of the eye; and even in those belonging to the little bones of the ear, of the action of which we are wholly unconscious. They occur also in the diaphragm, in the muscles of the tongue and of the larynx, in those of the soft palate, in the constrictors of the pharynx, in the levator ani, in the external sphincter ani, and in the muscles of the urethra. They have not yet been seen in the muscular tunic of the stomach and intestines, in the detrusor urinæ, or in the heart. Mr. Owen makes this interesting remark—that all the muscles infested by the trichina are characterised by the striated appearance of their ultimate fasciculi: whereas the muscles of organic life, which the animal does not inhabit, have, with the exception of the heart, smooth fibres, not grouped into fasciculi, but united reticularly.

It was thought for some time that the infection of the muscles by this microscopic worm was attended by no very marked symptoms. Thus the worms were found by Mr. Curling in the muscles of a man who, while in the apparent enjoyment of robust health, was killed by a fracture of the skull. Dr. Cobbold states that M. Zenker of Dresden was the first to demonstrate that these parasites were capable of causing a violent disease in the human body. No doubt the absence of notable disturbance, the presence of severe symptoms, and the termination in death, are severally dependent upon and proportional to the number of the invading army, which may vary from one or two worms to several millions. Of the ravages of the distemper which they may produce let me give you, out of many, one brief example. At Heldstädt, in Prussia, in October 1863, 103 persons sat down to a feast, of which sausage-meat formed a part. Within a month after the dinner 20 of these persons had died of, and more than 80 were then suffering from, 'trichiniasis,' which was distinctly traced to a certain pig from which the sausage-meat had been derived.

You may read in the seventh Report of the Medical Officer of the Privy Council a full account of the whole subject by Dr. Thudicum: by whom specimens also of the living and moving worm were shown, magnified by a magic lantern and thrown upon a screen, at a Soirée of the College of Physicians in 1864.

Animals used for human food, and pigs especially, are apt to be profusely infested with this encysted entozoon. When it has been transferred, still alive, to the human intestines, it matures with wonderful rapidity into a minute viviparous thread-worm, which rapidly also propagates its kind; and its offspring begin at once to migrate from the bowel, either entirely by their own boring powers and instincts, or using partly, perhaps, the circulating blood as a vehicle, until they reach the voluntary muscles, where, and where only, they become encysted, and the cycle of their generation is complete. The whole process is apparently run through in a month or six weeks. By degrees the encysted worms tend to calcify; particles of lime and magnesia are deposited around them, and give to the specks the gritty quality so often noticed.

The symptoms that mark the severer cases are feverishness and frequency of pulse, feelings of general malaise and depression, pains and a sense of tension in the limbs, resembling and sometimes mistaken for rheumatic pains, a general condition suggesting the suspicion of typhoid fever, ill-smelling perspirations, stiffness or immobility of the limbs, contractions of the joints, great sensitiveness of the surface, œdema of the face and legs, or even sometimes slight general anasarca, but without albuminous urine, and (in fatal cases) death by exhaustion.

When the complaint is known to be prevalent, some of the German physicians are in the habit, in cases otherwise equivocal, of extracting a fibre or so of a voluntary muscle by means of a sort of miniature harpoon, and of so detecting (perhaps) the entozoon.

As of the cysticercus cellulosæ, so of the trichina, the pig is the main purveyor. The last of these two parasites has not yet established itself in any large numbers among the swine of this country: but there is no security for a continuance of this comparative immunity; and too much care cannot be taken for avoiding all risk of swallowing these creatures in *pork*. They are destroyed by a certain amount of heat—the heat of boiling water. Never eat fresh pork, boiled or roasted, which has any shade of redness in it. Do not trust to any smoking process. Beware of underdone sausages. How far the curing of hams may suffice to render them harmless I do not know; but even cured hams should be thoroughly well boiled. Bear in mind that two different kinds of worm make both the fat and the lean of pork suspicious.

The *Filaria Medinensis*—*Dracunculus* or *Guinea-worm*—has its residence in the subcutaneous areolar tissue. It is a long, slender, round, uniform animal, like a fiddle-string, or a piece of bobbin; as you may see in the specimens before you. Its length varies from five or six inches to twice as many feet. Men's lower limbs, their feet and legs, are the parts most commonly possessed by the worm; but it occurs also in the scrotum, in the parietes of the belly, in the arms, beneath the conjunctiva of the eye, and in almost every superficial situation. It is sometimes solitary; but several may coexist or succeed each other in the same individual; nine or ten perhaps. A Dr. Marrudri, a friend of the celebrated Clot Bey, had suffered from twenty-eight of them in succession.

This entozoon is endemic in the hot intertropical regions; in Asia and Africa; upon the coast of *Guinea*, whence its trivial name. It sometimes abounds after the manner of an epidemic. Sir James M'Grigor tells us that the 86th and the 88th regiments, stationed at Bombay, were much plagued by this pest. The 86th was free from it upon entering the fort, in September, 1799; and so continued till the setting in of the monsoon in 1800. In the course of the monsoon nearly 300 of the men were attacked. The 88th regiment relieved the 86th. No case of Guinea-worm appeared among them for nearly a month after their coming into the barracks at Bombay, in October, 1800. In the latter end of November, they embarked for the Egyptian expedition; and in the course of the voyage, in one ship alone 199 men out of 360 were crippled and laid up with this loathsome disease. Of 181 instances, of which Sir James M'Grigor gives a tabular account, the feet were the parts affected in 124. This fact is illustrative of the mode in which the animal becomes a parasite. It is believed to be one phase of a very minute nematoid worm (about $\frac{1}{60}$th of an inch long, and exceedingly slender) which, abounding at the bottom of tanks, and on low muddy shores, after the periodical rains, in the regions where the dracunculus is endemic, is there called the *tank-worm*. These tank-worms attach themselves to whatever naked parts of the human body they may chance to come in contact with; and bore their way into and through the skin. Accordingly the Guinea-worm is frequently met with in the shoulders and backs of water-carriers. In its new habitat the intruder grows immensely, and becomes the nurse or parent of a numerous offspring. In its full-grown state it is found to be stuffed internally with a countless progeny of little filariæ. If this stage of parasitic life be essential to its fertility, other animals besides man must, in all probability, furnish the requisite temporary residence.

These worms remain for a considerable time in the areolar tissue without producing inconvenience, and therefore without betraying their presence. Hence they are sometimes brought over to this country. The great navigator, Dampier, had no symptom of a Guinea-worm which he carried about with him until about half a year after he left the place in which he contracted it. The period of latency is commensurate with that of growth and incubation, which are probably complete when the young filariæ are ready to disperse and to enter upon their independent life. This period seems to lie between six and twelve months.

The symptoms which do at length arise are the following:—itching of the part affected; a sensation as if there were something creeping under the skin; sometimes a cord-like ridge can be felt in the track of the worm; at length a vesicle, or a pustule, or a little boil forms, from which, when it breaks, the head of the animal protrudes. This process is often attended with fever; and in certain parts of the body the local suffering is considerable: the areolar tissue sloughs; and sometimes dangerous hæmorrhage occurs.

The only treatment which these cases appear to admit of, is the gradual and careful extraction of the worm. Lest that part of it which already protrudes should recede, or be broken, it is gently wound, day by day, round a small stick, or a little roll of adhesive plaster; pains being taken not to pull upon it so much as to risk its being torn asunder. The roll is protected by a bandage. Whenever, by accident, the animal is broken, very serious consequences are said to ensue; violent inflammation of the part, abscesses and sinuses, and high irritative fever. This mischief is ascribed by some to the presence of dead animal matter, by others, of young filariæ, in the subcutaneous tissue. The extraction is tedious work. Where, indeed, the areolar tissue is very loose, as in the scrotum, the worm is occasionally drawn out at the first attempt: but the process sometimes occupies weeks; and its average period appears to be not less than ten days. When the course of the animal is quite superficial and obvious, the natives are accustomed to make an incision in the skin, at about the middle point, and to pull the worm through from both ends. When once it is out, the parts presently heal. All other medication (and much has been tried) has been found useless; except, perhaps, the administration of assafœtida; and that not as a means of cure, but of prevention. It is said that the Brahmins in India, who are in the constant habit of using this drug, are exempt from attacks of the dracunculus. Cleanliness was also found, in the army, to be a considerable protection. Oiling the uncovered parts of the body has been suggested as a possible defence against the tank-worm.

There is a species of filaria peculiar to the eye; and another to the bronchial glands: but these are extremely rare.

You will hear farmers complain that their sheep have got the *rot*; which means that they are infected with the flounder-like fluke, the *Fasciola Hepatica*, which I have already mentioned, and which inhabits, often in great numbers, the gall ducts of the sheep. Large flocks are thus apt to be rendered unhealthy and unfit for human food, or to be absolutely destroyed, to the great loss or utter ruin of the farmer. It has been computed that more than a million of sheep and lambs die, rotten, in this country every year. To give you one example of the occasional magnitude of the evil, I may quote Professor Simonds' statement that in 1824 an epidemic of this complaint swept off three thousand pounds' worth of a Mr. Cramp's sheep in the Isle of Thanet; a loss which compelled him to give up his farm.

There can be no doubt that the sheep become thus diseased by swallowing, either with the water they drink or with the herbage they crop, some mollusc containing the larvæ of the liver-fluke. It is observed that sheep fed in low and marshy pastures are much more liable to the rot than others pastured on higher and dry situations: but that sheep which feed on *salt-water* marshes escape. Mr. Abernethy once told me the following curious story:—A shepherd had to drive a flock of healthy sheep to a distant part of the country. The journey occupied two or three days. On the road one of the animals broke its leg, and was carried the rest of the way on horseback. All the flock, except this hurt individual, were turned for one night into a marshy pasture. The broken limb was set, and the patient got well; and was the only one of the whole flock that did not subsequently become affected with the rot; the only one that escaped having flukes in its liver. The larvæ of these parasites must have been swallowed with the herbage cropped by the sheep in the damp meadow. Professor Owen suggests that 'the young flukes may pass instinctively from the duodenum through the ductus choledochus to the gall-bladder.'

I have mentioned this parasite because it has been met with, though rarely, in the human body. Professor Partridge took a single fluke from the gall-bladder of a patient who died in the Middlesex Hospital. I show you, from our Museum, the very gall-bladder in which the fluke was found. Both it and its cystic duct were perfectly healthy. The practical lesson to be gathered from the facts ascertained is that we should take especial care, not only that our drinking water should be pure, but that the vegetables which we sometimes eat raw—watercress, mustard and cress, celery, and the like, should be very thoroughly washed.

The urinary organs have their parasites also: of which I shall specify but one, and that chiefly on account of its strange lurking-place, and remarkable size. I allude to a species of strongle which sometimes occupies the human kidney, and which is no uncommon tenant of the same organ in various animals: the horse, the bull, the dog, the wolf, the polecat, and the otter. In the human subject its length varies between five inches and a yard, and it is sometimes half an inch in diameter. There is a specimen nearly of that size in the Hunterian Museum. It may well be called the *giant* strongle, *strongulus gigas.* Fancy a creature as big as a snake coiled up in one's kidney! It gives rise to no distinctive symptoms, although, as you may suppose, it causes much renal distress; hæmaturia, retention of urine, and great suffering in its passage out of the body, either through the natural urinary channels, or by abscess and ulceration through the back.

It was the opinion of Linnæus, and of other natural philosophers of his time, that the *intestina* worms were really terrestrial or aquatic animals which had been accidentally swallowed, either while young and small, or in the antecedent state of ova. It was even pretended that these animals had been recognised and detected out of the body, in stagnant waters. But later inquirers, especially Bremser and Rudolphi, completely disproved this notion. After dedicating twelve years of his life to the observation and study of entozoa, Bremser was satisfied that no creatures identical in structure with the intestinal worms are ever met with out of the body, except such as have come from the intestines of man, or of some other animal; and conversely, that no terrestrial or aquatic worms are ever found living within the bodies of men or of animals, unless they have been directly and plainly received from without.

But, then, is it not possible that, as Boerhaave supposed, aquatic or terrestrial reptiles, casually entering the body from without, being placed under entirely new and unnatural conditions, may have attained a monstrous growth, and undergone metamorphoses, such as we know that some of the lower animals, by change of circumstances, do undergo; as the tadpole becomes a frog, the maggot a butterfly?

That animals, or the ova of animals, having their proper life out of the human body, may by accident enter it, and having entered may grow, or hatch, is not to be doubted. But that the true intestinal entozoa have ever such an *accidental* origin is disproved by the facts that certain species of worms infest certain species of animals only: that in the same animal different species of worms occupy (as we have seen) special parts of the alimentary canal; have each its peculiar habitat: that worms and animals of external origin mostly die as soon as they are received into the digestive organs; while the true intestinal worms perish whenever they are delivered therefrom. Again, the fact that these worms not only live but breed within the human bowels is very adverse to this theory of an accidental *error loci.*

We repudiate therefore the notion of a fortuitous entrance, and are sure that intestinal worms spring from specific germs or ova introduced from without, not casually, but in accordance with a natural law: germs or ova which find in the interior of certain other living bodies the only conditions that admit of their development, the only soil in which they are capable of germinating, the only nest in which they can be hatched. Such has always been my own belief. We have something like this in that common affection of horses called the 'bots.' A species of œstrus or gadfly deposits its eggs upon the animal's hide, where they cause, I presume, some irritation, which induces the horse to lick that part with his tongue. The eggs are thus conveyed into the mouth, whence they reach the stomach. There they are converted into larvæ, and affix themselves to the parietes of the stomach. At length when they are ready to undergo their final metamorphosis, they are detached from the interior of the stomach, pass along with the food and fæces through the intestines, and are ejected from the rectum with the dung.

Why, it is asked, may not similar phenomena take place in the human body? There can be no doubt that we every day swallow, inadvertently, numerous ova of various kinds. It is supposable enough that sometimes the digestive organs may, and sometimes they may not, have the power of decomposing or expelling these ova.

It is quite certain that what are generally called *spurious* worms may have that mode of origin in the body. Thus Dr. Elliotson states that he had once a patient, an infant, who discharged from its bowels a dozen live larvæ, or maggots, of the common fly. The child had eaten part of a *high* pheasant some months before. There was, in that case, this instructive circumstance. The infant had been suffering under a chronic cough, but as soon as those larvæ were got rid of, the cough ceased. Dr. Elliotson says that he saw them in the napkin, moving about in the fæcal matter, just as they might have done if they had never been in the child's body. The same physician tells us that he has twice known, in two different patients of his, a living caterpillar to be voided from the intestines. One of the patients was a woman who had been in the habit of eating cabbage stalks while she was washing them for the pot. The moth lays its eggs on cabbages, and she no doubt had swallowed some of them, and one had hatched within her. In the ninth volume of Dr. Duncan's 'Medical Commentaries' is a precisely similar case. A boy, after a dose or two of calomel and jalap, emitted from the rectum very many caterpillars, all alive, and full of activity. He had been in the habit, when in the garden, of eating young cabbage leaves. Till this habit began he had enjoyed good health. While the animals were within his bowels he suffered severely; had locked jaw; and fell into a state resembling coma. Upon their expulsion he recovered perfectly. I have myself seen a sort of caterpillar which had been so voided. Centipedes have in like manner been vomited, and passed by the bowels. But the most wonderful instance of this kind that ever was heard of, is related by Dr. Pickells in the 'Transactions of the King and Queen's College of Physicians in Ireland.' A young woman of melancholic disposition and chlorotic appearance had been in the daily habit, from some superstitious motive, of drinking water mixed with clay taken from the graves of two priests who lived and died in the odour of sanctity. In this way she probably imbibed the ova of the insects which subsequently issued from her body. In the course of about three years and a quarter she discharged partly by vomiting, but chiefly *per anum*, upwards of 2,000 beetles, and their larvæ, most of them alive. Dr. Pickells counted more than 1,300. Larvæ, and pupæ, and perfect insects, all came forth simultaneously. Some of them ran off, as soon as they were vomited, into holes in the floor; and two large winged insects were so lively and vigorous as immediately to fly away. These strange births were preceded and attended by a complicated and distressing train of symptoms ; a gnawing, and sense of something creeping, at the pit of the stomach, vomiting of blood, amenorrhœa, hysterical convulsions, head-ache, retention of urine, and sometimes a degree of mental derangement. She was at length freed from this disgusting malady by large doses of oil of turpentine. Dr. Cobbold has collected a long list of insects, or the larvæ of insects, which have accidentally found their way into the human body, or been placed there by trickery and imposture.

These spurious worms differ from the true intestinal parasites in this—that the human alimentary canal is not their *only*, but their accidental and unusual nidus. Nevertheless, their occasional presence, alive, in that place, probably suggested the suspicion, since converted into fact, that some of the entozoa might have been originally ectozoa.

No difficulty however remains in the present day, respecting those creatures which occupy shut cavities within us, or are embedded in our solid organs : but this difficulty formed heretofore one avowed ground of the theory of *equivocal generation* : which means the spontaneous production of living creatures, independently of any germ, or egg, or parent. The vulgar suppose that dirt engenders fleas ; that maggots result from the putrefaction of flesh ; that eels arise of themselves in, and out of, mud ; that insects are bred from the dung upon which they congregate and feed. In other words, they infer the spontaneous origin of those creatures, of which they cannot or do not trace the procreation by pre-existing parents : and philosophers and men of science have done the same. They will not believe that which they cannot see or prove. The obscurity that formerly hung over the origin of some of the entozoa was not indeed the only, nor I think the chief, ground upon which the notion of spontaneous generation rested. You are doubtless aware that minute animalcules, so minute that most of them cannot be seen without a microscope, soon become abundant in water wherein vegetable or animal matters have been dissolved by infusion. Such animalcules are therefore called *Infusoria.* How do they come there? There are two suppositions open to us. One is, that they are formed by the fortuitous union of organic atoms contained in the infusion. The other is that they proceed from ova or germs existing in the liquid, or floating perhaps always in the atmosphere, and ready to quicken, whenever they light upon their proper element. That the ova of animals which are themselves visible only by the aid of a microscope, should be absolutely invisible to us, is not surprising. We may conclude that the latter supposition is the most true, if we can show that when these ova or germs are excluded, all the other conditions of the production of infusoria being present, no animacules appear. But this it is very difficult to show. The scientific world is even now at loggerheads about the validity of certain experiments devised with the view of settling that very question: in France Pasteur on the one side, Pouchet on the other; and they have their respective, very able, disciples in this country. For myself, I profess to hold the doctrine of Pasteur, long ago enounced by our great Harvey, of *omne vivum ex ovo*; or more exactly, since reproduction may take place by fissure, by gemmation, and therefore without ova—of *omne vivum e vivo.*

The wonderful tenacity of life possessed by some (at least) of these minute creatures, increases the chance of their reaching at last their appropriate habitat. Spallanzani kept certain infusorial animalcules four years in a state of complete desiccation and apparent death; but they presently recovered life and motion upon being then moistened. He dried and moistened alternately the same animalcules twelve times, with similar results; except that the number of revivers was each time less and less, and after the sixteenth moistening there was no resurrection. The *Vibrio Tritici* (a minute worm which is a parasite of wheat) having been dried by Mr. Bauer, resumed its activity, when remoistened, after the lapse of from four to seven years. Another small parasitic worm has been seen to exhibit strong contortions—evident vital movements—after having been subjected for above an hour, together with the cod-fish which it inhabited, to the temperature of boiling water. On the other hand it is stated by Rudolphi, that entozoa which infest the herrings annually sent to Berlin, hard frozen and packed in ice, do, when thawed, manifest unequivocal signs of restored vitality. 'If' (says Professor Owen, from whom I take these facts) 'the fully developed and mature entozoa can resist such powerful extraneous causes of destruction, how much more must the ova possess the faculty of enduring such without losing their latent life.'

What I have further to say will relate exclusively to the intestinal worms of the human body: their predisposing causes; the symptoms they occasion; and the methods of getting rid of them.

Of predisposing causes there is little to be said. I know of none except the endemic or the accidental prevalence of the outward germs of these worms, and the personal habits of those who are liable to their intrusions. The tænia armata has been observed to be common among pork-eating communities; the cyst-worm from which it is bred being, as I have already stated, a very frequent parasite of the omnivorous pig. By the same rule, it is said to be very rare among Jews. Again, the tænia inermis is frequent in beef-eating peoples. Wherever raw meat forms a part of the food of the people, as in Abyssinia, there we find the tape-worm prevalent. Even in this country the taste for raw meat appears to be less singular than one might have supposed. Many instances have now been published of the coincidence, in the same person, of tape-worm, with the practice of eating raw or under-done flesh.

It more concerns us to inquire into the general symptoms, through which the existence of worms in the alimentary canal may be ascertained, or suspected.

Those symptoms are very multifarious; and, for the most part, very equivocal. I know of none that can be reckoned certain or pathognomonic, except the actual appearance of one or more of the animals, or of portions of them, in the excrements of the body. Yet that they do give rise to a variety of morbid phenomena—which morbid phenomena are, however, liable to be produced by other causes also—there can be no question.

The most common of these are well known to all nurses and old women; such as colicky pains, and swelling of the belly; picking of the nose, in consequence of itching and irritation there; itching of the fundament; a foul breath; grinding of the teeth during sleep; a variable and capricious appetite, sometimes voracious and insatiable, sometimes none at all; and irregular bowels.

Worms now and then occasion strange, and even severe *nervous* symptoms, explainable upon the principle of the reflex office of the spinal cord. We conclude that the symptoms are owing to worms in such cases, because they cease when the creatures are got rid of. Some examples of this I have already noticed. Thus Dr. Elliotson's infant patient lost a chronic cough upon the expulsion of the live larvæ of the common fly.

Bremser gives a very similar case. A child of eleven, afflicted with tænia, had a troublesome dry cough. It was observed that the cough was suspended for two months, just after a very large portion of the worm had been brought away by anthelmintic medicines. This kind of coincidence happened, not once only, but three or four times; and at length, when the whole worm had come away, the cough was permanently cured. You may read an instructive instance of the same kind in Dr. Graves' Fortieth Clinical Lecture. I mentioned, some time ago, the frequent association of intestinal worms with epilepsy, which is then of the eccentric form: and I stated that a certain nobleman voided some kind of worm (a tape-worm, I think) from his bowels, and was thenceforward free from epileptic fits, under which he had long laboured. A curious circumstance, illustrating the fact that irritation of the mucous membrane of the alimentary tube may affect distant parts, is quoted by Dr. Joy from Albinus. A soldier received a wound, which led to the formation of an unnatural anus, in front of the abdomen, and in the track of the colon. Through this opening the mucous membrane of the bowel sometimes protruded; and whenever it was out, and exposed to the contact of cool air, the patient began to cough; and continued to do so till the mucous surface was warm again. Partial palsy, amaurosis, aphonia, and other nervous symptoms, occasionally depend upon the presence of worms in the intestines.

But let us examine into the symptoms which are more or less proper to particular species of intestinal entozoa; and into the treatment which they severally require.

A variety of symptoms are ascribed to the *ascaris lumbricöides.* Dr. Baillie says that the most characteristic are a tumid belly, emaciated extremities, offensive breath, and a deranged appetite. To these may be added colicky pains of the abdomen. When these animals get out of the small intestines, and ascend into the stomach or œsophagus, they may occasion pain, nausea, vomiting, even convulsions. They have caused death, as I mentioned before, by crawling into the biliary ducts, and into the chink of the glottis. Sometimes, on the other hand, they emerge, unexpectedly, from persons who had received no previous notice of their presence within.

This, the commonest parasitic tenant of the human bowels, is also a troublesome one to eject. A great variety of anthelmintics have been cried up as successful against it. Brisk purgatives, and bitter medicines in the intervals, have been much in favour. I believe that most of the patent worm remedies consist of mercury, jalap, and scammony, given in strong doses. The fœtid drugs, assafœtida, galbanum, valerian, are often used. Cowhage also, the dolichos pruriens, which is supposed to tease the skin of the parasite no less than that of the human worm; and tin-filings, which are thought to bruise and lacerate the offenders, are favourite medicines with some persons. I have never tried them. The oil of turpentine I have not found so effectual in expelling this species of ascaris as I shall presently show you that it is against the tape-worm. Probably the most eligible remedy for this kind of worm is *santonine*, a white crystalline substance obtained from a species of wormwood, the *artemisia santonica.* Its trivial English name testifies to the repute of this plant as a worm-destroyer. From two to eight grains of santonine—according as the patient is a child or an adult—may be given, at night, dissolved in a tea-spoonful of castor-oil, to be followed the next morning by a dose of senna.

It is well for you to be aware that santonine is apt to affect the vision; to cause the patient to see a yellow colour in the objects around him—perhaps by imparting that colour to the humours of the eye. It gives also an orange-red tinge to the urine. Most commonly it has no sensible ill effects; but I have known it produce feelings of general irritation and discomfort that were well-nigh unendurable.

The symptoms produced by the *ascaris vermicularis* are itching and irritation about the anus, especially in the evening, and at night. The annoyance is aggravated by the warmth of the bed, and by whatever overheats the body. I would refer you to a paper by Dr. Heberden on this subject, in the first volume of the 'Medical Transactions.' The case he describes is the more valuable, as it was related to him by a *physician* who was all his life plagued by these thread-worms. Generally, however, they infest children; and become fewer, and at length disappear, as childhood passes into youth. When they do accompany life through its several stages, although they are a source of great worry and suffering, they do not appear to shorten the duration of the patient's existence.

To introduce at one end of a twisted tube, several yards long, substances which are intended to act upon animals which live quite at its other end, would be a very roundabout course. Whether a purgative effect, or a specific destructive effect, be the object, enemata are preferable to medicines given by the mouth. *Bitters* offend and destroy these little worms. I have relieved many patients from their tormentors by prescribing simply the infusion of quassia as an injection. Tobacco clysters are praised; but the remedy is too hazardous. Dr. Darwell says of an enema composed of half an ounce of the muriated tincture of iron mixed with half a pint of water, 'there are few cases so obstinate that this will not suffice to overcome.' Lime water, injected into the rectum, forms another efficacious remedy against ascarides.

Thread-worms may be scooped out of the rectum with the finger. Old women fish for them with a piece of fat meat, or a candle, wherewith the entangled worms are drawn out of the bowel. Martinet recommends three successive injections: the first merely purgative; the second specific (common salt in solution, cold vinegar and water, lime water, some bitter infusion); the third, oleaginous and soothing. Oil often allays the itching. This teasing symptom may sometimes be quieted by applying a towel, wetted with cold water, to the fundament, while in bed.

Dr. Christison has told me of the case of a gentleman, 36 years old, whose life had been persistently disquieted by these pests, and who had come to despair of any permanent relief, but who was cured at once by a dose (24 grains) of the ætherial extract of the male shield fern, mixed with water, and swallowed in the early morning. The patient noticed that, whereas the thread-worms previously brought away by other means had been always in a living and lively state, those which were voided after taking the fern extract were dead, two only excepted, and these two died immediately on being touched with a drop of the remedy. In obstinately recurring cases I should be tempted to administer this drug at both ends of the body; for the parasites are said to wander, upon rare occasions, up the bowel as far as the cœcum.

Whatever means are employed for getting rid of the worms, appropriate measures should be combined for improving the general health.

I know of no signs by which the presence, in the human intestines, of the *tricocephalus dispar* is revealed; and I am equally ignorant of any remedies for it.

That a *tape-worm* is within, we know when joints of it are voided. Numberless symptoms have been ascribed to this huge internal parasite. The following are probably the most distinctive. Uneasy feelings in the epigastrium, which often abate or are removed by eating; the appetite generally craving, but sometimes bad; itching of the nose and of the anus; nausea; colic; giddiness; a sour breath. Less frequently loud borborigmi occur; and sometimes convulsions.

Louis has watched and recorded, with his accustomed minuteness, the symptoms of ten cases, in the wards of La Charité. Seven of the patients were males, and three were females. The youngest was a boy of twelve, the son of another of the patients; the oldest was seventy-four. Most of them were in comfortable circumstances, and had been habitually well fed. The greater number of them had for some time been passing fragments of tape-worm, with their stools, in their clothes, and in their beds. In one of the cases the articulations had been twice only detected in the stools, and each time upon the operation of a purgative.

In all the patients but two, the other symptoms commenced when the evacuation of the fragments commenced. This renders it probable that the worms begin to give annoyance when they get into the large intestine. The temporary relief that results from the expulsion of portions of the animal strengthens that supposition. In the 'Medico-Chirurgical Journal' is mentioned the case of a man who was in the habit of freeing himself from large fragments of tape-worm by introducing a stick into his rectum, and twisting the worm round till it broke.

The chief symptoms observed in Louis' cases were colicky pains of the abdomen; itching of the anus, and of the end of the nose; uneasiness in the epigastrium; and deranged digestion and appetite.

Pain in the abdomen occurred in *all* the instances: but it differed in different cases, both in degree and in kind. It was intermittent, and mostly felt towards the flanks.

There was itching at the margin of the anus in seven of the ten cases; itching of the nose in four. With one exception only, itching was present in one or the other, or in both of these situations.

The appetite was craving in one patient; unaffected in four; variable or bad in all the rest. In all, slight emaciation was observed. In all, the pupil of the eye was of its natural dimensions. This is noticed, because dilatation of the pupil has been set down as one of the symptoms of tænia.

Louis thinks that the following combination of symptoms indicates with tolerable certainty the presence of some kind of worm in the intestines. Pain in the belly; colic of various degrees of intensity, unaccompanied by diarrhœa; itching about the anus, and at the end of the nose. If pains in the limbs, lassitude, and nervous symptoms exist also, the diagnosis is strengthened.

One potent remedy for tænia—at any rate for the tape-worms of this country—is oil of turpentine, given in large doses. The anthelmintic virtues of this substance were not generally known till a paper on the subject, by Dr. Fenwick, of Durham, was published, in 1811, in the 'Medico-Chirurgical Transactions.' A sailor, plagued by this parasite, had noticed that large fragments of the worm were passed whenever he had swallowed an unusual quantity of raw gin. Thinking that a stronger spirit might have a stronger effect upon his internal enemy, he tried a glass of turpentine, which completely cured him. This practice was then taken up by some unprofessional persons, who gave turpentine, with similar success, in several cases. At last Dr. Fenwick, in conjunction I believe with my friend Dr. Southey, investigated the subject; and when they had satisfied themselves of the value of the remedy, the result of their inquiries was communicated by Dr. Fenwick to the late Dr. Baillie, in a letter which was read before the Medical and Chirurgical Society. But there is nothing new under the sun. More than half a century ago, Mr. Malden, in the 'Memoirs of the London Medical Society,' recommended the same remedy, in the same doses, for the same purpose. But his recommendation had been neglected, or forgotten.

The dose of turpentine is from half an ounce to two ounces. It may be given in combination with castor-oil; or castor-oil may be administered afterwards to assist its purgative effect. The patient should take it in the morning, fasting; and no drink should be admitted into the stomach until the medicine begins to operate, lest sickness and vomiting should be provoked. The worm generally is voided, dead, within an hour or two.

The inconveniences of turpentine as a remedy are its nasty taste, the sort of intoxication it is apt to produce, the distressing sickness it excites in some persons, and the strangury it sometimes occasions. This effect of it, however, is less common from large than from small doses of the oil. The bowels should be kept open with castor-oil, so long as the urine retains the violet smell, which indicates the presence of the turpentine in the circulation.

Upon the Continent a celebrated empyreumatic oil, called Chabert's, is in great repute. It contains turpentine, but is still more nauseous than it. One part of the empyreumatic oil of hartshorn is mixed with three parts of oil of turpentine. After the mixture has stood for three days, three-fourths of it are to be distilled over, in glass vessels, by means of a sand-bath. Chabert was a veterinary surgeon; and had used this remedy with remarkable success upon domestic animals, cows, dogs, and sheep. Bremser, after testing its safety by taking it himself, administered it, he tells us, to no less than 500 individuals troubled with tænia. Among these were two children, a year and a half old. He found it both a powerful and a permanent cure. Of the whole number treated there were but four who required a second course of the remedy. The dose is two teaspoonfuls, night and morning, until four or six ounces have been taken; a purgative being from time to time interposed. If that dose produce any confusion of head, it must be diminished.

Bremser admits that this curative process is tedious, but affirms that, on the other hand, it is safe, and but little inconvenient. When the patient has continued free from any symptoms of tape-worm for three months, he concludes that the cure is absolute. Other practitioners agree with him in attesting the efficacy of this oil; but are not so confident about its perfect safety and convenience.

Another great remedy is the bark of the pomegranate-root. This is at least as old as the time of Celsus. It has long been employed in India. Its value has only been recently appreciated in France; and in this country it is not much used, I believe, even yet. You may read a very instructive account of its effects in a paper communicated to the Medical and Chirurgical Society by Mr. Breton. He boiled two ounces of the fresh bark of the root in a pint and a half of water, till the decoction was reduced to three-quarters of a pint. Two ounces of this, cold, he gave to a patient who had tape-worm, and repeated the dose every half hour until four doses had been swallowed. About an hour after the last dose, an entire tænia was voided, alive.

The bark dried in the sun he found still more active. A stout man, forty years old, took a decoction prepared in the same way as the former, only with the dried instead of the fresh bark. Three hours after the first dose a live tape-worm came away, nineteen feet two inches long. The medicine thus prepared seemed to be too strong; the patient was sick, giddy, and trembling for several hours. He, also, had taken four doses.

To two boys, of seven and ten years of age, he began early in the morning to administer one ounce of a decoction made of half the previous strength, every half hour, for six times; and then stopped. In the middle of the day he resumed the medicine in half-ounce doses. Giddiness and faintness soon came on; and, about five o'clock, each of the patients passed a tænia of the solium kind.

A scruple of the powder was given, mixed with water, every hour, for five successive hours, to a boy of nine. Forty minutes after the last dose a living tænia was expelled.

The same quantity was given to a girl, ten years of age; beginning at eight, and repeating the dose hourly till twelve o'clock. At twenty-four minutes past one she voided a living tænia lata: and the next morning, at nine o'clock, a dead tænia solium. This curious case shows that the two species may exist in the same person at the same time.

Mr. Breton relates other examples; but those which I have cited are quite sufficient to demonstrate the power of this substance. The bark appears to act upon the worm as a poison. In tepid water tæniæ will live for several hours. When they are plunged into the aforesaid decoction, they writhe and manifest great uneasiness. Between the first dose of the medicine and the expulsion of the worm the shortest period appears to have been three hours, the longest twenty-five.

Louis' ten cases, before referred to, were all treated by a nostrum called the potion of M. Darbon. It proved successful in all. Eight or ten ounces of it were taken in the morning, before breakfast; and the cure was accomplished by that single dose. It is said to be quite safe, to have no very decided taste, and to cause no further inconvenience than a slight colicky feeling, and uneasiness in the epigastrium, less than is produced by many a purgative: and even these sensations were probably owing to the movements of the worm; for when, after the lapse of four months, the dose was repeated, it was followed by no inconvenience at all. The medicine is not strongly cathartic, and sometimes requires the aid of a lavement. In each of these cases the expelled tæniæ were apparently dead. Their *heads* were detected in the evacuations: in one instance seven heads were seen. Louis says that within a few days at furthest after the discharge of the worms, all the previous symptoms of their presence ceased; and the patients improved in respect of appetite, digestion, complexion, flesh, and strength. They all remained well four months afterwards, and then the potion was again administered; but it brought away no more worms. Some of the patients, who had previously tried other modes of cure, had never enjoyed so long an interval of freedom.

In 1850, a new vermicide drug was introduced from France into this country, consisting of the dried flowers of a tree that abounds in Abyssinia, where they have been employed as an anthelmintic for more than two centuries. Kousso, in the Amharic language, is the name both of the tape-worm and of the remedy. This has been tried by several English physicians, but first and most frequently by Professors Budd and Todd, in the King's College Hospital. In every instance, so far as I am informed, a single dose of the medicine has been followed by the expulsion of the worm, or of a great part of it. Its action is attended with but little distress, often with none. Occasionally it has caused nausea; sometimes it has proved gently aperient; and sometimes the aid of a subsequent purgative has been found requisite. It appears to be more uniformly successful than turpentine—while it is much less offensive to the palate, and less rough in its operation. Tæniæ have been dislodged by it where turpentine had failed, or had lost its previous power. A patient of my own, a young military officer, was plagued with tape-worms in India. He had been in the habit of taking turpentine, which at first brought away portions of the animal, but after a while it failed entirely. Upon returning to England he took a dose of the kousso, a quarter of an ounce; and in four hours a worm, twelve feet long, was voided alive. But the symptoms recurred, and he then swallowed a second dose, with a similar result. A third dose, taken a fortnight afterwards, simply purged him. He assured me that, irrespective of its vermifuge property, he found the kousso far less unpleasant and annoying than the turpentine had been. In but few instances has the head of the parasite been noticed in the evacuations; but portions of its narrow part, near the head, often. Neither has the kousso always achieved a radical cure. All the kinds of tænia have been expelled by it; and being certainly effectual for the time, and both speedy and safe in its operation, it will no doubt be largely used here now. At first its cost was an impediment. Thirty-five shillings an ounce was the price, and half an ounce the dose. Much smaller quantities have however been found sufficient, and an ounce of the dried flowers may now be obtained for twenty pence. The powdered flowers are steeped in half a pint of lukewarm water for a quarter of an hour, and then the whole of the mess is swallowed, powder and infusion together. Lemonade is recommended to be drunk, both before and after the dose; why, I do not know, unless it be for compensation to the palate. It is well with this, as with every other weapon against tape-worm, to take the enemy at a disadvantage by using the remedy in the morning—an aperient having been premised the day before. The worm is thus less likely to be defended against the access and contact of the drug, by food, or by the intestinal secretions.

Various other remedies have been employed, and employed with more or less success. But the best and most trustworthy of all, in my judgment, is that which I just now mentioned, as being furnished by the male shield fern. The root of this plant formed the basis of a nostrum, called Madame Nouffer's, which was so highly thought of, that in 1776 the King of France gave that lady some hundred pounds sterling for the secret of its composition. Bremser thought that it was especially powerful in expelling the *Bothrio-cephalus latus*; but against this opinion we may set the experience of Dr. Gull, who in one of the volumes of the 'Guy's Hospital Reports' gives the results of the employment of an ætherial tincture prepared from the *rhizoma* of the male fern, in 200 cases, among the patients of that hospital. He states it to be a convenient and an effectual remedy, succeeding where other anthelmintic drugs had failed; and out of fifty tape-worms expelled under its use, one only was of the Swiss species. From a drachm and a half to two drachms of the tincture was the common dose. It caused some nausea, occasionally even vomiting, and then it operated as a purgative. Dr. Willshire confirms Dr. Gull's estimate of this substance, and pronounces it to be less nauseous and irritant than turpentine, more sure than pomegranate, less bulky and less expensive than kousso. I was informed by Dr. Christison some little while ago that up to that time he had never known it fail. The British Pharmacopœia prescribes a liquid extract of the fern.

What we want is the expulsion of the *head* of the tape-worm, and this should always be carefully looked for in the *excreta*.

I do not know whether the fern extract has been tried against the round lumbricoid worm. It deserves to be so if it has not.

LECTURE LXIX.

Diseases of the Liver. Acute Inflammation. Abscess of the Liver. Causes and Treatment of Acute Hepatitis. Chronic Hepatitis. Jaundice. Its Symptoms, Causes, and Species.

From the alimentary canal I go to the other organs directly or indirectly concerned in the digestion and assimilation of our food. And first, let us look at that large gland, the liver, of which the most obvious office is the secretion of bile.

The liver is liable to various forms of disease: but it is not so frequently in fault as many would have us believe. It is often blamed most gratuitously and unjustly.

I shall follow the usual order, and consider first, *inflammation* of the liver, which may be either acute or chronic.

These are, both of them, diseases that are much more common in warm climates than here.

Of well-marked acute hepatitis the symptoms are fever, with pain, tenderness, and a sense of tension in the right hypochondrium, inability to lie on the left side, difficulty of breathing, a dry cough, vomiting, hiccup.

You will not find all these symptoms present in every case; yet they are all worth attending to.

The pain is sometimes sharp and pricking, like a pleuritic stitch: sometimes dull and tensive. In the former case the peritoneal covering of the gland is supposed to be affected; in the latter its parenchyma. The pain in the right side is aggravated, often, by the movements of the diaphragm in respiration; and this explains the embarrassment of the breathing, and the short dry cough. Why the patient cannot well lie on the left side is obvious enough: all the connections of the inflamed organ are then put upon the stretch by its weight. There are, however, some exceptions to this rule. On the under or concave part of the liver lies the pyloric extremity of the stomach; and that viscus often sympathises with the hepatic inflammation: nausea and vomiting occur; and hiccup. The thoracic symptoms on the one hand, or the stomach symptoms on the other, may be expected to predominate, according as the convex or the concave surface of the organ is chiefly the seat of the inflammation.

Permanent rigidity of the abdominal parietes, especially on the right side—and, more particularly, rigidity of the right rectus muscle—is a symptom strongly indicative, according to Mr. Twining and other surgeons in India, of deep-seated abscess of the liver. The same symptom was noticed by Dr. Budd in a case of jaundice from closure of the common duct, and in a case where a cancerous ulcer of the stomach had eaten into the adherent liver : and by Dr. Graves in a case of inflamed gall-bladder. This sympathetic affection is but one instance, among many, of that kind of protective instinct whereby a tender part is in some measure shielded against the infliction of pain by pressure.

From the earliest records of medicine down to the present day, pains about the shoulders have been spoken of as accompanying disease of the liver. I have already referred to these pains as furnishing a good example of what are called sympathetic sensations. This subject has been closely investigated quite recently by Dr. Embleton; and I give you his conclusions as corrective, in some important respects, of the explanation attempted by myself in an earlier lecture of this course.

Dr. Embleton states his belief that 'the true path of sympathy from a diseased liver to a pained shoulder lies along the lines of the *pneumogastric*, and the two divisions of the *spinal accessory* nerves.'

The pain, he remarks, occurs more frequently than is generally thought; but, being seldom severe, it is but little complained of by patients. When well marked, and *not* severe, it occupies the top of the shoulder, in the angular space between the acromial end of the spine of the scapula, and the adjacent end of the clavicle. Here is lodged the upper and outer part of the trapezius muscle, as it goes to connect itself with those two bones. When the pain is severe, it shoots downwards also, along the vertebral edge of the scapula—in other words, along the track of the external division of the spinal accessory nerve ; the only nerve lying in that direction : and the painful track is often tender under pressure. Moreover, in these cases some portions of the pneumogastric nerve of the same side are unnaturally sensitive to pressure. The links of connection which Dr. Embleton believes to exist between the two nerves principally concerned, you may carefully study in his paper 'On the Shoulder-tip Pain.'

In extent and intensity the pain varies directly as the liver disease. It is lessened when the pus of a hepatic abscess is discharged, and tension is thus removed. It depends, in Dr. Embleton's opinion, upon a 'real hyperæmia or inflammatory condition of the nerves concerned, and of their sheaths.' He holds also, what I have always believed, that these pains are felt in and about the right shoulder when the right lobe of the liver is the seat of disease, in and about the left shoulder when the left.

The situations of the pains, the cough, the short and shallow breathing, used to puzzle the older observers: and they confessed their occasional inability to determine whether the inflammation was situated in the lower lobe of the right *lung*, or in the *liver*. But now-a-days we need have no difficulty in making the diagnosis. The ear will tell us, if we employ auscultation and percussion, whether the contents of the chest or of the belly are suffering: and my own experience has taught me that sharp pain, with feverishness, occurring in the debateable ground of the right side, denotes pleuritic inflammation far more often than it denotes hepatic.

Jaundice is an *occasional*, but by no means a *necessary* effect or accompaniment of hepatitis; whether acute or chronic: and, therefore, what I have to say of that symptom I shall give under a separate head.

Acute hepatitis may terminate in resolution; or it may terminate in diffused suppuration; or, what is more usual, in the formation of a circumscribed abscess, or of abscesses, in the liver. In this climate we do not often meet with hepatic abscesses; but they are very common in hot countries: and some of the most interesting events of the disease have relation to the progress of these collections of matter. When they approach the surface of the liver, adhesions generally take place (in virtue of that conservative principle of which we so constantly discern the working) between the diseased organ and the neighbouring parts. If no such adhesion occurred, the matter would at length burst into the cavity of the peritoneum; and this *does* sometimes happen. The peritonitis which is thus, suddenly, set up, is almost always fatal. I referred, however, in a former lecture to one instance in which it was recovered from. Sometimes the adhesion is effected between the liver and the parietes of the abdomen, and the abscess points *externally*, and may be opened by the surgeon. Such a case occurred in my neighbourhood last year. Sometimes the liver glues itself to the stomach, or to the intestines; and then the abscess breaks into the alimentary tube, the matter is evacuated by vomiting, or by stool: and all goes on tolerably well again. In other instances the agglutination is to the diaphragm, which is perforated, and the pus makes its way into the sac of the pleura, or through the lung to the bronchi, and so out by the mouth. I have seen three or four examples of this myself.

It is plain that in such instances as these, there must be adhesion of the *lung* to the diaphragm also: but cases have occurred in which the matter burst into the cavity of the pleura, and presently destroyed the patient by suffocation.

One case of perforation of the lung is still *sub judice.* A gardener, forty-five years old, was attacked, four weeks before he came under my care in the hospital, with severe pain in the right loin. Just at the edge of the short ribs, and not far from the vertebral column, on the right side, I found a large elastic swelling, very tender, and of a bluish red colour. As the patient's urine was most remarkably loaded with amorphous deposits, my first suspicions turned towards the kidney. It soon, however, became evident that, although the tumour was below the diaphragm, the parts above that muscle were involved in the disease. The right half of the thorax was dull to percussion; no vesicular breathing was audible there, but some scattered crepitation and bronchophony. Four days subsequently to his admission the patient began, during an effort of coughing, to discharge almost in a stream, from his mouth, a considerable quantity of gray, pultaceous fluid, of the consistence of gruel, and horribly fœtid. Altogether the amount of matter thus expectorated was estimated at two quarts. The pain then ceased; and the swelling was observed to be less. After three days more, it was deemed proper to puncture the tumour, and matter was let out having precisely the qualities of that which he had ejected through the mouth. The patient remained eleven weeks in the hospital, the expectoration becoming less unnatural, and the discharge from the abscess gradually diminishing. He then chose to depart. Some months afterwards he applied for re-admission, and again stayed with us a week or two. His health had much improved during the interval: but the wound in his back was still open, and he still continued to expectorate somewhat; and yellow bile was frequently to be seen, both on the dressings which covered the puncture, and in the vessel into which he spat. I think it probable that this man may eventually recover, at the expense, however, of a permanently damaged lung.

A kitchenmaid at the Thatched House Tavern in St. James's Street received a violent blow at the back part of the right hypochondrium. This was followed by pain there, and by fever; and at length she began to have cough and dyspnœa, and to expectorate. She was taken into the hospital. Here she brought up an abundance of frothy mucus of a bright yellow colour; not at all resembling the rusty sputa of pneumonia, but exactly of the tint which bile would give to it. At the same time large crepitation could be traced from the bottom to the top of the right lung. I fully expected that this woman would die: but by degrees the yellow expectoration ceased, all the auscultatory signs gradually disappeared, and she recovered perfectly.

I thought, at the time, that this also might be a case of perforation of the diaphragm and of the lung, connected with the formation of an abscess in the liver. And the absence of jaundice, to which the colour of the matter expectorated might else be imputed, lends likelihood to that supposition still. But I have subsequently met with a case in which similar symptoms presented themselves, without any hepatic mischief. A gentleman, beyond the middle period of life, was attacked with pain in the right side, and with fever. He coughed also, and spat up some reddish sputa. Auscultation disclosed the existence of pneumonia in the lower lobe of the right lung. After two or three days the patient became deeply jaundiced; his skin presenting an orange-yellow hue. There was no deficiency of bile in the fæces; nor any pain, tenderness, or hardness in the hepatic region. Very tenacious mucus was expectorated, having a deep grass-green colour, with here and there patches of yellow. The inflammation extended to the upper lobes of the lung, and the patient sank. The lower lobe was found to be in a state of gray softening, or diffused suppuration, and it adhered, by a capsule of recent lymph, to all the parts around it. No appreciable change could be discovered in the liver, which contained rather less blood and bile than usual; nor in the gall-bladder and ducts. The secreted mucus of the air-passages had received its very unusual hue from the bile that circulated with the blood.

The sputa, collected in a basin, presented another appearance, such as I had never before seen. Large pyramidal bullæ projected among them, of green colour and crumpled irregular surface; looking like bells of moulded green glass. And when the summits of these large bubbles were broken through, the bubbles did not collapse; but their brittle walls remained firm, as they might have done if really vitreous.

Rigors occurring during the progress of hepatitis should make us suspect that suppuration is taking place: if the pain be thenceforward mitigated, or exchanged for a sense of weight, and if hectic fever should set in, we may be tolerably certain that pus has formed.

Of the several courses taken by a hepatic abscess, that towards the surface of the body is the most common; and it is the only one with which we are able to interfere. And the chief thing we have to look to, is not to interfere too soon. The adhesion of the inflamed organ to the wall of the abdomen is the indispensable condition, not of success only, but of safety, where the question arises of puncturing the tumour. Without such adhesion the pus will be transferred from the cyst to the cavity of the peritoneum; or if the abscess be not reached by the scalpel, that cavity will be laid open. Now it is not easy to ascertain whether there be adhesion or not. Certainly the operation ought not to be attempted until the parts above the abscess are very thin, and are verging to a point; and even then, unless there were some distinct purpose, besides that of saving a little time, some urgent distress or danger to relieve, I think such abscesses might be more prudently left to themselves. They differ materially from hydatid cysts, though these sometimes contain pus. Dr. Gregory, of Edinburgh, used to mention a case in which, as fluctuation was palpable, and the tumour pointed, it was proposed to open it; but the patient died, somewhat suddenly, before the operation could be performed; and inspection of the dead body showed that no adhesion existed. Mr. Malcolmson has published, in the 'Medico-Chirurgical Transactions,' two or three instances of a like kind. These are circumstances which teach us to be cautious about recommending the operation.

Dr. Budd discourages the opening of a hepatic abscess by puncture, deeming it generally best to allow it to open of itself. Other authors of note recommend that the matter should be let out as soon as it is known to exist. The old question here crops up—'Who shall decide when doctors disagree?' and it may properly be answered by stating that in some cases the operation is advisable, and in others it is not; and that the circumstances of each particular case will generally enable a careful observer to determine which of the two courses is the most safe, and therefore the most expedient.

Adhesion of the liver to the abdominal parietes being a prerequisite condition for puncturing the abscess, Dr. Budd has suggested the following plan for ascertaining—when the liver is large, and the abdominal walls are thin—the presence or absence of such adhesion. Feel for the edge of the liver, or for some prominent part of its surface, and mark the place with ink. If there be adhesion, the marked spot will correspond with the edge or the prominent part of the liver in all states and positions of the body. On the other hand, if there be no adhesion, the liver will slide along the wall of the belly when the patient takes a deep breath, or turns from a supine posture towards the left, and the mark will no longer correspond with the edge, or the prominence, as in the former case.

A method was proposed and practised by the late Dr. Graves for *producing* adhesion, by making an incision over the centre of the tumour, down to within a line or two of the peritoneum. The same safeguard had been previously devised by Dr. Dick in India, who made, however, the wound in the integuments by caustic, instead of by the knife.

A case is recorded of the bursting of a hepatic abscess into the pericardium; another into the vena cava. Facts of this kind constitute mere medical curiosities, and have no practical bearing.

It is clear, both from the size of the organ, and from its situation in the body, that an abscess in the liver can never be otherwise than gravely hazardous. Yet many recover from them. Much evidently depends upon the manner and direction in which the pus seeks and finds a vent. The most desirable road for its exit is one which it sometimes takes, but which I have not yet mentioned. It occasionally flows out into the duodenum through the excretory ducts of the liver, when these happen to have been laid open by the suppurating process. Next to this we may hope for its discharge through the adhering parietes of the abdomen; next by a breaking of the abscess into the alimentary canal. The escape of the matter through the air-passages is fearfully perilous; and its entrance into the shut serous sacs, or into the great blood-vessels, almost necessarily fatal.

I have spoken of abscess in the liver, as an event of acute inflammation of that organ. Such inflammation is apt to arise, in tropical countries, after exposure to cold. In any climate it may be excited by a blow, or other mechanical injury. But abscess in the liver is frequently the result of other remote causes. You will call to mind those collections of matter which form in the liver, as well as elsewhere, as consequences of pyæmia. There is, moreover, an acknowledged connection between hepatic suppuration and *dysentery*. In hot climates the two are often found coincident. It has commonly been thought that, in these cases, the affection of the liver happens first, in the order of time; that the hepatic disease, interfering with the freedom of the portal circulation, occasions congestion of the sub-mucous capillary blood-vessels, and so disposes the membrane to take on inflammation under the influence of any slight exciting cause. Dr. Budd, however, has proved, by a careful collation of a large number of authentic instances, that the relation of these two morbid conditions to each other is sometimes exactly the reverse: that the dysentery may be the primary disorder, and the hepatic abscess the secondary; the link of connection between them being the same as between phlebitis (in the sense explained in a former lecture), and the formation of pus in parts more or less remote. The blood, in its return through the veins which are tributaries of the vena portæ, carries with it some vitiating ingredient from the inflamed membrane: and this vitiating ingredient provokes inflammation in the capillary vessels of the liver.

It is not, however, upon dysentery only that suppurative inflammation in the liver is apt to supervene; but upon various other morbid conditions also of the extensive mucous surface from which the returning blood is poured into the portal vein; upon injuries, therefore (including surgical operations), to the rectum, and the parts adjoining it, and especially upon ulceration of the intestines, of the stomach, and of the gall-bladder or its ducts. Yet not every form of ulceration: for abscess in the liver does not occur with that half-sloughing, half-ulcerating state of the glands of Peyer, which is characteristic of typhoid fever; nor with that curious sort of ulceration of the duodenum, originating apparently in the glands of Brunner, which Mr. Curling has shown to be a frequent consequence of external burns; nor with the intestinal ulceration (still primarily glandular) which belongs to phthisis; nor are such abscesses often met with in connection with simple ulcer of the stomach.

When small abscesses in the liver result from pyæmic disease which does not prove fatal, it seems that they may remain there long, even for years, without apparently disturbing the general health.

Excepting hepatic abscesses, resulting from external violence, it is the pyæmic abscess alone that we are likely to meet with in this country and in persons who have never lived in tropical regions. It is certain also that, here at least, dysentery often occurs without leading to abscess of the liver. During an epidemic dysentery in the Penitentiary at Milbank, among many hundred cases, not one (according to the testimony of the late Dr. Baly) was complicated with hepatic abscess.

It is believed that in the hepatic abscesses associated with dysentery in hot climates (what Dr. Murchison calls *tropical* abscesses) the two morbid conditions are mostly, or often, the common result of one and the same cause, and not connected together as cause and effect. The abscess may occur without any dysentery; or precede the dysentery when both occur in the same person; or, sometimes, be a pyæmic consequence of the dysentery. These opinions are held by Dr. Murchison as the upshot of observations made by himself while residing in Burmah.

I witnessed, in 1862, the last few days of a distinguished nobleman who had recently returned to this country upon relinquishing the governor-generalship of India. I could not trace, from the report of his constant medical adviser in India, that he had ever any dysentery there. He was very carefully looked over, by a very competent physician, upon his return to England, and was thought to be sound: but he soon after began to droop, had severe hepatic and abdominal pains, and invincible diarrhœa. The large intestines were ragged with ulcerations, and there were two large, loosely floating abscesses in the liver.

Acute hepatitis, when it occurs—in this country it seldom does occur—requires the same treatment in the outset as I have recommended in other acute inflammations. Our object is, if possible, to prevent suppuration. Blood should therefore be freely taken from the neighbourhood of the inflamed part by leeches. I will not weary you by going over the old ground that we have so often trodden already, nor repeat observations which have been many times made respecting the methods, the amount, and the repetition of this great antiphlogistic remedy. Depletion of the portal vessels may also be indirectly obtained by purgatives; especially by such purgatives as produce copious and watery stools. The neutral salts are therefore proper in this disorder. It may be, as some suppose, that they operate beneficially, as counter-irritants, upon the duodenum; but their effect in draining the veins that feed the vena portæ, and thereby relieving the hepatic congestion, is more obvious and more intelligible. These saline medicines should be much diluted; and their action may be quickened, if that be necessary, by adding the infusion of senna.

After blood has been duly abstracted, warm fomentations should be applied to the right hypochondrium; or a large and warm linseed poultice.

When suppuration has taken place, or is unavoidable, when the patient ceases to complain of pain, but has in its stead a feeling of weight in the hypochondrium, and becomes distinctly hectic, a corresponding change must be made in the treatment. Active depletion is no longer admissible; you must sustain the strength by a more nourishing diet, and prescribe some tonic remedies; the sulphate of quina, with sulphuric acid; or the nitro-muriatic acid, which enjoys a considerable repute, greater perhaps than it merits, for the relief of liver complaints: and you must consider, as you best may, by the light of what has been already said on that subject, the propriety of letting the collected matter out by puncture.

The liver is subject to one very remarkable form of disease with which, though it is not at all common, nor controllable as yet by any known plan of treatment, but is almost always fatal, you ought to be acquainted. It has been described by Dr. Budd under the name of *softening*, and by Frerichs as *acute or yellow atrophy* of the liver. In the worst and typical cases, death may take place within twenty-four, or even within twelve, hours from the first declaration of the disease; in others, life may go on for from two to five days, but is seldom prolonged for a week.

It is not an inflammatory disease. There is, at first, no febrile heat, and the pulse is slow. The most prominent symptoms are vomiting—first of mucus, and at length of matters like coffee grounds—in other words, of altered blood; pains in the head, and delirium, frequently of a fierce and maniacal character; extreme restlessness; convulsions; tremors; stupor; dilated pupils; coma; death. With the coming on of these disturbances of the nervous functions, the pulse becomes extremely frequent; and there is jaundice withal, unequivocal, yet not generally of a very deep hue.

Meanwhile there is commonly some tenderness of the abdomen on pressure, especially in the right hypochondrium; the natural hepatic dulness on percussion narrows in its area, without any tympanitic state of the intestines to account for this; in fact, the liver lessens rapidly in size; shrinking from its left lobe towards its right; becomes soft, wrinkled on its surface, and flexible; and is pushed and flattened towards the spinal column. Of course these last-mentioned facts can be ascertained only after death. The spleen, on the other hand, undergoes a proportional enlargement.

The rapid diminution of the liver is very surprising, and would scarcely be credible, if it were not a measurable change. The gland loses one-third, one-half, or even two-thirds of its natural bulk. Dr. Bright has recorded cases in which its weight was reduced to 2 lbs., to 23 ozs., even to 19 ozs. There is no mechanical impediment in the bile-ducts. The gall-bladder is mostly empty, or contains a small quantity of gray mucus only. The secreting cells of the atrophied liver are found to be thoroughly spoiled, even undistinguishable as cells. In their place brown granules are seen, large particles of colouring matter, oil globules, and isolated bodies similar to cell nuclei, accompanied often by needle-like crystals of tyrosine, and globules of leucine. The urine deposits, on cooling, a greenish-yellow precipitate, differing to the naked eye, and still more plainly under the microscope, from all other deposits. It is found to contain large quantities of leucine, tyrosine, and extractive matter of a peculiar nature, while the urea and phosphate of lime gradually disappear.

Frerichs appears to be quite justified in his remark that the disappearance in the course of a few days of one-third, or one-half, of the natural bulk of a large gland, amply supplied with blood, without any discoverable alteration in the blood-vessels leading to it, taken together with the character of the urine already described, constitutes a state of disease to which we know of no parallel, and attests the presence of profound metamorphoses in the composition and materials of the bodily structure.

I should have told you that in several cases premonitory symptoms of the coming outbreak of disease have been noticed; a state of general malaise not differing specifically from such as is known to usher in blood diseases; among which, and not as essentially a liver disease, this disease, acute atrophy, must certainly be ranked.

So far as experience has hitherto gone, the malady is one of adult life. It has been noticed more often in women than in men; and most especially in pregnant women.

In cases which do not run so rapid a course, yet which seem to belong to the same category, active purgation has been found beneficial; and in a few rare instances the patient has recovered.

Acute inflammation of the liver is apt to degenerate into *chronic*. Chronic inflammation may also arise under the circumstances that gave birth to the acute form. Chronic hepatitis, again, is not unfrequently produced by the presence of specific disease in the liver; of carcinoma; of syphilitic deposits; of scrofulous tubercles. Melanosis and hydatids are both of them of common occurrence in the same part; and they may give rise to symptoms, or they may not: and when these morbid conditions do declare themselves by external signs, those external signs are very much the same as belong to chronic hepatitis. The precise diagnosis is sometimes exceedingly obscure; the symptoms point distinctly to the liver as the *seat* of the disorder; but as to its exact *nature*, we must often be content with probabilities alone.

The symptoms of chronic hepatitis—or of the chronic forms of disease to which I have alluded, when they show themselves by symptoms—are (I give you them in Cullen's words) 'some fulness and some sense of weight in the right hypochondrium; some shooting pains felt at times in that region; some uneasiness or pain felt on pressure in that part; some discomfort from lying upon the left side; perhaps some degree of jaundice; and sometimes a certain amount of fever combining itself with more or fewer of these symptoms.' In short, they are just the symptoms of acute hepatitis occurring in a minor degree.

Chronic affections of the liver are sometimes attended with an *increase*, and sometimes with a *diminution*, of its *size*. When it is augmented in bulk, its place and enlargement may be ascertained by palpation and percussion; nay, the outline of the magnified gland may sometimes be seen, extending beyond its proper situation in the hypochondrium, and passing far down into the abdomen. I have known the liver reach to the right groin: and when its left lobe is affected, it will sometimes stretch across towards the lower part of the left side of the belly. On the other hand, the liver may shrink into a much smaller space than it naturally occupies. These small livers are usually hard. Interfering more with the portal circulation than many enlarged livers do, they are more frequently attended with dropsy of the peritoneum.

The 'hobnail' liver, the *cirrhose* of modern French writers, is nodulated as well as hard. The irregularity of its surface may be so great as to be perceptible to the touch. I fully described this condition of the liver when I was upon the subject of passive ascites, of which it is the most common cause.

I have already told you that when a large round boss can be distinguished, projecting from the surface of the liver, you may speculate upon its being caused by a collection of hydatids; especially if the tumour has arisen without pain, or fever, or any material interference with the general health. When several smaller prominences can be felt, rendering the enlarged liver uneven, and the patient's health is broken, they are probably cancerous; and you search after tokens of cancer elsewhere. A smooth, globular, painless tumour, perceptible by the fingers near the margin of the liver, suggests the likelihood of a distended gall-bladder, especially if jaundice concur.

I mentioned some time ago, the 'fatty' liver, so frequently found associated with pulmonary consumption. The liver in this state is soft, enlarged, smooth on its surface, and of a buff or tawny colour throughout. Mr. Bowman has lately shown that these changes are owing simply to the unwonted abundance of certain small granules of fat, of which, in the healthy organ, each lobule contains a few only. If in a phthisical patient we find the liver palpably enlarged, and if we can feel no irregularity of its surface or of its edge, we may guess that it is encumbered with this interstitial fat: but there are no symptoms peculiar to the fatty liver. As to its cure, we are quite helpless: and the same may be said of the hobnail liver, as well as of nearly all those forms of disease in which the organ is loaded with specific deposits.

Until within the last six or seven years, nothing definite was known in this country about syphilitic disease of the internal organs of the body. Medical science owes a great debt to Dr. Wilks for his researches and writings on this subject.

True syphilis is a constitutional disease produced by inoculation of the venereal poison. It is characterised by the deposit of lymph, or an albuminous material, in nearly every tissue of the body. We *see* the evidence of this deposit in the amount of thickening and hardness which, after a few days, underlies the inoculated spot, or chancre; in the lymph which lies upon the inflamed iris; and we infer the existence of a similar deposit, at the same period of the disease, in those internal tissues and organs wherein at a later period, and after death, we find remains or traces of its presence. The gravest of the internal changes thus wrought occur within the cranium: the most common are met with in the *liver*. Probably, in the outset, the exudation is softer, but it is rarely seen till long after its first effusion, and then roundish nodules are found, as big as peas or marbles, firm, and shooting out fibrous rays into the surrounding hepatic tissue. These are the dried-up, contracted remains of the earlier exudation. They lie chiefly near the surface of the liver, which is puckered up by their shrinking, and has a scarred appearance. Whether in the early or in the late stages of this form of hepatic disease its presence is revealed by any distinctive symptoms, I cannot tell you. It seems to me probable that many of the cases of so-called 'lardaceous' or 'waxy' liver—and some at least of Dr. Budd's 'knotty tumours' of the liver—are of this syphilitic kind.

I must not lose this opportunity of reminding you of the remarkable influence of mercury, given till the gums are tender, in resolving and dissipating the lymph poured forth in syphilitic iritis. The surgeon witnesses the same thing in the rapid disappearance of the hardness and thickening beneath the venereal chancre. It can scarcely be doubted that a similar healing process goes on in the syphilitic exudations that befall internal organs. When this remedy has been omitted, or not long enough continued, the remaining exudations tend at length to dry up, harden, and contract in the way I have pointed out; and what is found at last must be regarded as a sequela, rather than a stage, of the constitutional syphilitic complaint.

Dark masses of extravasated blood are sometimes found interspersed through this gland, and then, by an absurd perversion of language, the patient is said to have had 'apoplexy' of the liver.

The same causes which produce acute hepatitis, acting in a less intense degree, will excite chronic inflammation of the same textures. Intemperance also, and particularly the habitual and excessive use of alcoholic liquors, certainly tends to generate chronic mischief in the liver, allied to inflammation. We see this even here, and it is still more strikingly perceptible in warm climates, as you may learn by reading the works of those persons who have had experience of the diseases of India. Dr. William Ferguson, for example, who was for some time chief of the medical staff of the Windward and Leeward Islands in the West Indies, observed that there was a regular increase and aggravation of these chronic affections of the liver among the troops after they received their monthly pay, when they drank great quantities of ardent spirits; arrack in the East Indies, and rum in the West.

What used to be called the 'nutmeggy' liver, is simply the result of congestion of its blood-vessels :—the congestion occurs under two forms, according as the branches of the hepatic vein, or those of the vena portæ, are gorged; and the former of these two conditions is very much more common than the latter. If both these sets of vessels are full, the liver is universally red. If the hepatic vein alone be the seat of the congestion, then in the centre of each lobule we see a red speck, surrounded by yellowish matter; the specks are isolated, the yellow colour is arranged in a sort of network. Whereas, if the portal system be greatly engorged, the red streaks will be continuous, and the yellow portions hemmed in by them, and isolated. I show you these distinctions in some specimens prepared by Mr. Kiernan himself.

I should have stated, when speaking of the signs of chronic hepatitis, what is singularly true of chronic liver affections in general, that they are apt to be attended with much languor and lassitude, and a remarkable depression of spirits; and with that sort of dread and apprehension of impending evil, which I mentioned as being a striking feature of hypochondriasis: the very derivation of which term marks its frequent connection with hepatic disorder. There is sallowness of the complexion also; and sometimes emaciation.

The same remedies are adapted to the chronic, as are proper for the acute inflammation of the liver: the comparative mode of their exhibition, however, differs somewhat.

When the organ can be felt, and pain is referred to it, with tenderness on pressure, topical bleeding and fomentation may be expedient; or in some cases a succession of blisters may be more advisable. And since the general symptoms of chronic hepatitis, and those produced by the presence of specific diseases of the liver—cancer, to wit, or syphilitic deposits—are often much the same, and consequently equivocal, it will be right to adopt such treatment as may prove beneficial under at least one supposable condition, yet not seriously hurtful under any. Mercury, therefore, I should give in obscure cases, especially if any other suspicion of syphilitic infection suggested itself—mercury to the extent of touching the gums. The biniodide of mercury, as recommended in a previous lecture, furnishes a very eligible form of remedy in these ambiguous cases.

In some instances gradual improvement follows the use of saline purgatives, given in small doses, and repeated for a long time together. Five grains of blue-pill every night, or every night and morning; and as much of the sulphate of magnesia as will produce one or two watery stools every day, for weeks, perhaps, in succession. Patients are not so well content to bear this discipline when it is administered from boxes and phials, *as physic*; but they have more faith in the natural mineral waters: so that a residence at Cheltenham, or some such place, is exceedingly proper to be recommended in these cases; where the daily use of the waters may keep up a continual drain on the system of the vena portæ; and where relaxation from business, the amusements that are constantly going on, with change of scene and of society, may contribute to dissipate the hypochondriacal feelings which are so apt to render the subjects of chronic hepatic disease supremely wretched.

Moderate exercise, in the open air, on horseback and on foot, should be encouraged. There is no doubt that hepatic as well as gastric derangements are fostered by sedentary habits. Tepid bathing is another expedient from which benefit may be hoped. In many instances it will be proper to make trial of Scott's nitro-muriatic bath.

Taraxacum is a drug which has been much employed in liver complaints since Dr. Pemberton's book on the diseases of the abdomen was published: and when well prepared, and taken for a long time together, I believe it often does much good. The Germans are very fond of giving the *muriate of ammonia* in small and frequent doses. They have the same belief in the virtues of this salt, in various disorders, as most English practitioners have in those of mercury; and what is curious, they attribute to it some specific influence upon the functions of the *liver.*

In the account that I have now given of the principal diseased conditions of the liver, I have not dwelt upon, nor included, all the changes of structure and appearance to which that organ is liable. There are various conditions which disclose themselves by no intelligible symptoms during life, of which the nature has not yet been determined, and of which the cure still remains to be discovered. At this advanced period of the course, and with no time to spare, I do not think it necessary or right to trouble you with the unprofitable discussion of matters that are not strictly practical.

I have mentioned *jaundice* as an occasional symptom both of acute and of chronic inflammation of the liver. But jaundice is often spoken of as constituting, itself, a distinct form of disease. If we consider it in that point of view, its diagnosis is most easy. We have only to look upon our patient in daylight to know what is the matter with him. But jaundice depends upon various and very different morbid conditions; and looking to those conditions as the true objects of diagnosis, we find that the real nature of a given case of jaundice is often involved in very great obscurity.

Let us first consider the constituent features of jaundice, whether it be regarded as a *disease*, or as a *sign of disease.* They are—yellowness of the skin and of the eyes; whitish or drab-coloured fæces; urine having the colour of saffron, and communicating a bright yellow tinge to white linen.

The characteristic yellow complexion is owing, no doubt, to the presence of bile, or at any rate of the colouring matter of the bile, in the circulating blood. And the deep tint of the urine is evidently derived from the same source. On the other hand, the paleness of the fæces is to be ascribed to the want of bile, which always exists in healthy and natural excrement. This last symptom is not, however, a constant one; there may be jaundice while bile appears in the stools. I shall explain how this is supposed to happen presently.

If you ever doubt, as you possibly may, whether your patient be really jaundiced, or only yellowish from sallowness combined with anæmia, you will look especially to the conjunctiva, and to the urine, both of which betray the yellow tint of jaundice very early and conclusively. The eye readily recognises bile in the urine; but its presence may be ascertained, in questionable cases, by an easy chemical test. If nitric acid be added in moderate quantity, the urine assumes a green colour, which passes into red upon the continual addition of more acid. The shades of colour may be well shown, by allowing sufficient nitric acid to trickle to the bottom of the urine contained in a conical glass. The lowest stratum will be colourless acid; the next, urine reddened by the acid; and above this a layer of green. The colour of the urine caused by taking santonine, or rhubarb, deepens into a blood-red when treated with the caustic alkalies; which have no such effect upon any modification of the bile-pigment.

You may ask how the bile, or its colouring matter, comes to be visibly present in the blood, or rather in many of the tissues supplied by the blood, and in several of the other fluids of the body. There can be no doubt that when the bile, after being formed in the liver, is detained there, or in the gall-bladder, in consequence of some impediment to its excretion, it is re-absorbed —both by the lymphatic vessels and by the veins—carried into the circulation, and so conveyed to the surface, and to the parts in which the change of colour is observed. In the beginning of the present century Dr. Saunders, of Guy's Hospital, made on this subject some conclusive experiments, which have since been repeated by others with similar results. The hepatic duct of a dog having been tied, and the animal killed two hours afterwards, the numerous lymphatics in the walls of the bile ducts were seen to be distended with a yellow fluid; the fluid in the thoracic duct also was yellow; and so were the intervening lymphatic glands. Again, two hours after the ligation of the hepatic duct, the serum of blood taken from the hepatic vein was found to contain much more of the colouring matter of the bile, than that of blood taken from the jugular vein in the neck. That bile is capable of being taken up by the absorbents is further apparent from the fact that when the cystic duct is permanently shut, the bile disappears gradually, but entirely, from the gall-bladder. The existence of some positive mechanical obstacle to the efflux of the secreted fluid is often ascertained; and even when none can be discovered after death from well marked jaundice, it is conceived that the ducts of the liver might have been temporarily plugged up either by inspissated bile, or by a sort of biliary sand.

But another theory to account for those cases in which no physical impediment to the passage of bile through the ducts can be discovered has been broached: first I believe in this country, by the great Dr. Erasmus Darwin. It was afterwards revived by M. Chevreul, who has been followed by Mr. Mayo, and others. These pathologists are of opinion that the bile is formed, not by the liver, but in the blood: that the office of the liver is to strain off or withdraw the bile from the circulation, constantly, as fast as it is formed: just as the perpetual elimination of urea from the blood appears to be one great purpose of the kidneys. They hold, therefore, that jaundice manifests itself whenever the due separation of the bile from the blood is suspended or imperfect. Failing of its natural vent, this peculiar substance accumulates in the blood, seeks other outlets, is deposited in various places, and, in fact, partly escapes through unaccustomed channels. They speak of jaundice as a symptom of *suppression* of bile, while others consider it as a sign of *retention*; using these words, suppression and retention, in the sense in which they are applied to the secretion of urine.

They maintain that one of the proper functions of the liver, the abstraction of bile from the blood, may be arrested by alterations of the substance and structure of that gland; or by the obliteration or obstruction of the gall-bladder or ducts, impeding or forbidding the removal of the bile already collected; or by some obscure influence of the nervous system upon these organs. They introduce the last kind of cause with the view of explaining those cases, which certainly occur, in which jaundice is the result of severe bodily pain or strong mental emotion. Nay, on their supposition, we might even suppose that the yellowness is sometimes due to a spontaneous and unwonted *abundance* of the elements of bile in the blood: in which case we need not wonder that jaundice should go along with perfect integrity of the biliary apparatus. Dr. Budd even thinks it probable that, in the majority of all cases, jaundice 'results primarily, and solely, from the secretion of bile being suppressed or deficient.'

Grave doubts, however, have been expressed as to the soundness of this theory of suppression. I pronounce no judgment upon the question in dispute. What seems certain is that jaundice accompanies many forms of disease which plainly result from blood-poisoning, when there is in the gall-ducts no physical impediment to the excretion of bile: yellow fever for example; relapsing fever often; typhus fever sometimes; pyæmia; snake-bites. In all or most of these cases analogous derangements of the urine, and of the glands which secrete it, may be noticed when looked for. Indeed, the general analogy between the functions and structure of the liver and the kidney is well worthy of attention. Of each of these glands the secreting cell is the essential part, the part wherein its vital chemistry is carried on. The kidney is purely, the liver partly, an excretory gland. The secretion of the former is more open to our scrutiny than that of the latter. Urea and uric acid are known to exist already formed in the blood: it is the chief office of the kidney (not to form but) to secrete, separate, eliminate these substances from the blood. When it fails to do so they accumulate in the blood, and destroy life. It would seem probable that the liver, in like manner, may secrete bile, or some at least of its constituent principles, from the blood; that these may pre-exist in the blood, although chemistry fails to detect their presence. Indeed, the same failure is experienced when bile in considerable quantity has been injected into the blood. Besides its natural and normal secretion, the kidney is capable of separating, without difficulty or damage, substances which reach it, with the blood, other than urea and uric acid: various organic salts, for example, and many odorous and colouring matters derived from the vegetable and animal kingdoms. But there are substances—numberless poisons—which refuse to be so eliminated, or which, in their elimination, do violence to, and gradually injure and spoil, the texture and properties of the secreting cell. We may apply the same observations with more or less of certainty and accuracy to the secreting cells of the liver. In acute atrophy of that gland we have seen that its cell structure is absolutely destroyed; and it seems reasonable to suppose that the jaundice, always associated with the disease, may be due to the retention and modification in the blood, of elements or materials which the cells, during health, would have transformed and separated as bile. But I leave the problem as being hitherto unsolved.

I pass from this digression to a somewhat closer examination of the principal circumstances noticeable in the complaint. Its technical appellation, I should observe, is *icterus*, which is the Greek name for a bird with a yellow plumage, the galbula, or golden thrush; the sight whereof, by a jaundiced person, was death (Pliny tells us) to the bird, and recovery to the patient. Various other terms have been applied to the disorder, most of them having reference, like jaundice itself (from the French *jaune*), to the unnatural colour. *Morbus arquatus*, from its exhibiting some of the bright hues of the rainbow; *aurigo*, from its resembling gold; and we hear the common people say, now-a-days, such a one is as yellow as a guinea. The Latins spoke of it also under the title of *morbus regius*: why they so called it we learn from the following curious passage in Celsus, giving an account of the pleasant regimen, fit for Royalty itself, to be adopted by those who labour under the malady. 'Per omne vero tempus utendum est exercitatione, frictione: si hiems est, balneo; si æstas, frigidis natationibus; lecto etiam et conclavi cultiore, lusu, joco, ludis, lasciviâ, per quæ mens exhilaretur: ob quæ *regius* morbus dictus videtur.'

The whiteness of the stools I have mentioned as being a very common but not a constant appearance. It clearly depends upon the absence of bile. Such stools have often a sour and very offensive smell. But sometimes bile appears in the discharged fæces, though the skin, and eyes, and urine are yellow. This probably depends upon the circumstance that some branches of the hepatic ducts are obstructed while the others are free; and thus the bile that is secreted is, in part, reabsorbed into the blood, and in part carried off into the intestines. In a former lecture I stated that one of the uses of the bile appeared to be that of stimulating the bowels to action: it is the natural purgative. Accordingly, in most cases of jaundice, the bowels are costive. But neither is *this* uniform. In some of the worst cases, wherein the jaundice depends upon hepatic disease, which is connected with disease also of the mucous coat of the intestines, there is constant diarrhœa.

In some instances the yellowness of the skin is preceded, or accompanied at first by itching, which is occasionally so intolerable as to require the employment of opiates to allay it. In most cases there is no itching at all. The bile never fails to appear in the urine, which is in itself dark, and when collected in considerable quantity in a deep vessel, even *black*; and which tinges any white substance that is dipped into it of a bright yellow. The urine which thus sometimes seems black, may be proved to derive that appearance merely from concentration of the yellowness, by pouring a little of it into a shallow white dish, or by diluting it with water; when the brilliant yellow tint will become manifest. So, too, the froth which may float on the surface of the dark urine will be visibly yellow. Bilious sweat sometimes occurs, staining the patient's linen yellow. The saliva, in some jaundiced persons, has the same yellow tinge, and a distinctly bitter taste. Perhaps this happens only under mercurial salivation, when the saliva is mixed with albumen: for the secretions furnished by the mucous membranes are in general signally exempt from this change of colour. The tongue and inside of the lips present commonly a remarkable contrast with the face of a jaundiced person. Dr. Budd observes that the mucus of the stomach and intestines is never stained with bile, except when that secretion has continued to enter the duodenum. I have already mentioned two or three instances that have fallen under my own notice, in which mucus brought up from the lungs was rendered green and yellow by bile: but this is of rare occurrence. It has been said that the milk of women who are nursing is made yellow in this disorder. Dr. Heberden, however, states that he never witnessed this; and he had known a woman with a very deep jaundice upon her, suckle her infant for six weeks together with no apparent bad effects upon its health. One man assured him that his tears were yellow. You are aware of the vulgar notion, suggested, no doubt, by the colour of the conjunctiva, that to a jaundiced eye all things appear yellow. It is an old notion, for we find it expressed by Lucretius:—‘Lurida præterea fiunt quæcunque tuentur Arquati.’ Heberden was disposed to regard this as a mere poetical fiction. But certainly it is sometimes, though very rarely indeed, a fact. Two women, whom he considered however to be of little credit, told Heberden that objects appeared yellow to them. I have been assured of the same thing by a medical man who experienced it in his own person. If I do not mistake, Dr. Mason Good saw all things yellow when he was jaundiced. Dr. Elliotson had some very interesting cases of this phenomenon. One of his icteric patients declared that objects seemed yellow when looked at with one eye, but not with the other; and in the eye that perceived the yellow tint he observed two large red vessels running towards the cornea. And in one or two instances, which he met with afterwards, of yellow vision with both eyes in jaundiced patients, he found inflammation, or distended blood-vessels, in both eyes. This very morning I saw in the hospital a patient of Dr. Wilson's, a middle-aged woman, affected with jaundice. She affirms that to her all objects look yellow. In both of her eyes there are several varicose and singularly tortuous vessels, proceeding across the sclerotica towards the cornea, and some of them reaching its margin. It seems probable, therefore, that the ophthalmic vessels, in their natural state, do not permit the colouring matter of the bile to pass through them; but that when they become enlarged by disease, so as to admit the colouring particles of the blood, they may also give a passage to the yellow colouring matter, which tinges the humours of the eye: and in that case the objects seen through the yellow fluids would appear like those viewed through a piece of yellow stained glass. This is a point which is worth your attention in future.

The shades of yellowness are different in different patients. The young, and those who are pale and fair, present a bright lemon colour. In those who are florid, or whose cheeks and skin are flushed with fever, the tint will more resemble that of the Seville orange. Again, if the patient be naturally swarthy, or if his visage be livid or dusky through imperfect arterialisation of his blood, the superaddition of jaundice will give him a greenish or olive hue. In old age the colour is, *cæteris paribus*, less vivid. These differences result from natural or acquired differences of complexion, antecedent to the icterus. But sometimes the bile that is reabsorbed is vitiated and dark; and we may have, for that reason, as Dr. Baillie has pointed out, cases of green or black jaundice. You will remark that, from whichever cause the green or dark colour proceeds, whether from a mingling of the yellowness of the bile with the blueness of lividity, or from the circulation of green-coloured bile, such cases are especially unpromising cases.

Icterus depends, as I have said, upon various and different internal causes: and frequently we cannot determine at all, until death affords us the means of inspecting the parts concerned in its production, what the precise exciting cause may be; even when it is simply mechanical. Any kind of pressure made upon the excretory ducts of the liver may produce it: and such pressure may be exercised by tumours seated in the liver itself; or by a scirrhous pylorus; or by specific disease situated in the head of the pancreas, of which I have seen several examples; or by a diseased condition of the duodenum: and these possible causes of a detention of the bile in its receptacle should always be borne in mind when we are investigating an obscure case of jaundice.

The impediment, in the cases just supposed, is external to the ducts; but they may be obstructed within, plugged up by mucus, by inspissated bile, or by a biliary calculus. This forms another of Cullen's species of icterus—the *icterus calculosus.* The concretion is most commonly situated, I believe, in the ductus communis choledochus. Occlusion of the cystic duct by a gall-stone does not cause jaundice. The pain that attends the passage of a gall-stone through these ducts is often dreadful. Perhaps there is no pain to which the body is subject that is more severe. You will not wonder at this, when you reflect that through a tube, of which the natural size scarcely exceeds that of a goose-quill, there sometimes passes a stone as big as a walnut. The common duct has been found so dilated as readily to admit one's finger. Cullen's definition of this species is 'Icterus, cum dolore in regione epigastricâ, acuto, post pastum aucto, et cum dejectione concretionum biliosarum.' Now the last of these circumstances, the voiding of biliary calculi by stool, may happen over and over again without its being noticed, and it does not help us at all to judge of the nature of the complaint at its commencement, while the gall-stone is still within the ducts. With the pain, which is not constant, but comes and goes, there is commonly much nausea and vomiting; and sometimes hiccup; and the matters vomited are usually very sour. The patient is flatulent and dyspeptic;

languid and gloomy. At length the concretion passes into the intestines; the pain suddenly ceases, and all is soon well again. Attacks of this kind, having happened once, are very apt to be repeated. And when jaundice, associated with the peculiar pain, has once occurred, repetitions of the pain may fairly be attributed to gall-stones, although there may be no repetition of the jaundice: and conversely, many repetitions of jaundice justify the probable diagnosis of gall-stones.

Now this pain you might readily mistake for the pain of inflammation, were it not marked by these two circumstances —the absence of tenderness, and the absence of fever. Pressure, instead of augmenting, usually mitigates it. The patient keeps his hand firmly applied to his epigastrium; or rests, perhaps, the weight of his body upon some hard substance placed beneath his stomach. I speak now of the beginning of the attack, before there has been much retching; for a degree of tenderness of the abdominal muscles is often produced by repeated straining and vomiting. The pulse is unaffected, or I should rather say it is not accelerated, during the pain: occasionally it is even slower than natural, and the skin cold. Though there be no inflammation, rigors may occur; just as they sometimes happen when a solid substance—a bougie to wit—is passing through, and distending the *urethra.*

Nevertheless, inflammation does sometimes arise, and then the pulse becomes frequent, and the skin hot, and thirst and headache are complained of, and the epigastrium is tender; and if blood be drawn it exhibits the buffy coat. Sometimes the gall-stone makes its way, by ulceration, through the contiguous structures, and so is discharged outwardly or into the bowels. In such cases there must have been inflammation.

As jaundice often occurs without any pain, so a gall-stone may enter and pass through the ducts, and produce pain, when there is no jaundice. The cystic duct alone may be blocked up, and that portion only of the bile be prevented from escaping which is accumulated in the gall-bladder. It is probable that re-absorption of the contents of that cistern is not very active. Or a calculus of an angular shape may stick in the common duct, and thus impede, without entirely stopping, the egress of the bile. Dr. Heberden thought that gastrodynia was not unfrequently owing to biliary concretions: founding his opinion upon the fact that many persons suffer, for months or years, under occasional attacks of epigastric pain, which is at last associated with jaundice. But after all, this might happen from progressive disease in the stomach itself; and it is a pity that Dr. Heberden's views were not fortified by dissections.

When once a large calculus has forced its way through the natural channels of the bile, they remain permanently dilated; and smaller stones may be afterwards voided without pain or other notice of their passage. There are persons who get rid of scores of them in this way, during the course of their lives.

Sometimes a large concretion, after its extrication from the biliary passages, lodges in the more capacious intestines, and gives rise to serious obstruction there. I mentioned, recently, one case of this kind which had fallen under my own notice. But in general the concretions are presently voided with the stools: and they should always be looked for. The patient is much gratified by *seeing* that his enemy has been expelled; and also by the proof he thus obtains of the sagacity and judgment of his physician. Now, as most gall-stones before they are dried sink in water, although when dry they may probably float to its surface, it is not enough merely to mix the fæces with water when we are searching for these calculi. The whole of each alvine evacuation should be carefully passed through some kind of fine sieve. I formerly told you that I had never but once succeeded in catching a concretion in the evacuations of a patient, whose symptoms had led me to seek for it. Since that statement was made, three other patients of mine, taught how to search, have detected among the alvine discharges this palpable source and explanation of their previous sufferings. One of the three collected, in this way, fifty-five small facetted biliary calculi, which he voided within the space of five weeks. He was jaundiced; and he began to pass them, four or five at a time, with paroxysms of severe pain, just after having had the hepatic region diligently shampooed and kneaded as he lay on his left side in a warm bath; so that they seemed to have been mechanically pressed out of the gall-bladder, and through its ducts.

When concretions pass which are small and angular, having several flat surfaces, we are to expect that more will follow them. If a single stone come, large, smooth, and roundish, we may hope that it has left none behind it.

On more than one occasion I have known jaundice to terminate upon the passage through the bowels of concrete bile, in the shape of a black gritty powder, very like powdered cinders or coal-dust.

We often find gall-stones, even in vast numbers, in the gall-bladders of persons who during their life-time had never been known to suffer pain about the liver, or to have jaundice, or to exhibit any token of the presence of such concretions. We infer from this that, while they remain in the reservoir of the bile, they are harmless; and that the suffering and the hazard they occasion are mechanical consequences of their transit through the gall-ducts. I have heard of an instance in which upwards of 1,300 gall-stones were taken from a human gall-bladder after death.

On the first day of June, 1854, a remarkably strong and healthy man, between 60 and 70 years old, with whom I had for many years been on terms of friendship, was engaged in inflating an air-bed by blowing with his mouth through a tube. The process required a long-sustained straining effort, and he suddenly felt that he was hurt in the right side of the epigastrium, near the edge of the false ribs. After a while the pain ceased, but it recurred with severity in the night, and the next day his skin was slightly yellow. Bating some trifling and early variations in its tint, the yellow colour became deeper and deeper, and he remained intensely jaundiced till the day of his death, which was the 29th of the following August.

During his illness, his liver gradually became very large and prominent. His stools were devoid of bile, and his urine was charged with it. The pains continued to return from time to time. He had been subject to similar pains in his youth, and had been taught to ascribe them to ‘spasm of the diaphragm.’

There was a good deal of fat about the abdomen. He did not die of inanition. The liver was enormously enlarged, and full of bile; and the gall-bladder, which was much thickened, was filled with numerous calculi, and moulded, as it were, upon their irregular form and outlines. One large gall-stone, something like a horse's hoof in shape, completely plugged up the opening of the common duct into the duodenum. Branches of the hepatic ducts in the liver were so distended as to present little reservoirs of bile and mucus. It seems probable that some of these calculi were of very old date. Here life was extinguished in thirteen weeks, by the mere occlusion of the biliary ducts, and the complete barring up of bile in the liver. Dr. Budd relates a case in which the body was tolerably well nourished for more than twelve months, although the common gall-duct was closed all that while by a gall-stone; and another in which a woman, suffering under a similar obstruction, lived more than eight months in a state of deep jaundice, and five months after the occurrence of the jaundice gave birth to a child, which she was able to suckle up to the time of her death.

These gall-stones are not, as you might suppose, mere lumps of inspissated bile. There are, I believe, concretions of that kind, but they are very rarely met with in the human subject. The ordinary calculi consist, in a great measure, of a peculiar substance, *cholesterine*, which exists in a state of solution in healthy bile, but which in some morbid conditions of that fluid, being released from its solvent, assumes its proper crystalline form. Very little is known respecting the circumstances under which the change takes place. Cholesterine, Dr. Prout tells us, is the product of some modification of the oleaginous principle. Biliary concretions seldom form in children. They are much more common in women than in men. They occur most frequently in persons who are corpulent, lead sedentary lives, use generous fare, sleep much, and neglect their bowels: all which things foster or denote a torpid and congested state of the hepatic system. Cattle are said to be subject to biliary calculi when shut up in stalls during the winter, and to lose the complaint when they are again turned out into the pastures in the spring. Hence the absurd notion, countenanced even by Van Swieten, that *grass* is a good remedy for jaundice.

One of Cullen's varieties of jaundice was supposed by him to depend upon mere spasm of the gall-ducts. '*Icterus spasmodicus*, sine dolore, post morbos spasmodicos, et pathemata mentis.'

Certainly the 'pathemata mentis' somehow play their assigned part: fits of anger, of fear, of alarm, have been presently followed by jaundice: and it has also been produced by great bodily suffering, by a severe surgical operation, or, perhaps by the dread which attended it. Mr. North witnessed a case in which an unmarried woman, on its being accidentally disclosed that she had borne children, became in a very short time yellow. A young medical friend of mine had a severe attack of intense jaundice, which could be traced to nothing else than his great and needless anxiety about an approaching examination before the Censor's Board at the College of Physicians. There are scores of instances on record to the same effect: and *this* is observable of such cases, that they are often fatal, with head symptoms: convulsions, delirium, or coma, supervening upon the jaundice. We may believe that in these graver cases, just as in acute atrophy of the liver, some profound changes have been worked in the blood and other fluids of the body. We have, however, no proof of the occurrence of mere spasm of the gall-ducts; and if we had, we could scarcely conceive of any spasm sufficiently abiding to produce jaundice.

Jaundice may also occur, as I stated before, as a symptom of acute or chronic inflammation of the liver; and then its treatment will merge in that of the primitive disease which occasioned it.

A high atmospheric temperature, long continued, appears to have a decided influence in producing certain forms of this disorder. I was struck with the frequency of a mild and manageable kind of jaundice, which affected young persons, chiefly females between ten and sixteen years old, in this town, in the autumn of 1846, just after the prevalence of extremely hot weather.

Icterus occasionally comes on during pregnancy; and disappears after child-birth. The pressure of the gravid uterus may thrust other organs, a loaded colon for example, against the liver, and so permanently impede the passage of the bile. The little exercise that pregnant women are apt to take, and the costiveness that frequently attends their condition, may have some influence in causing the *icterus gravidarum.*

Almost all systematic writers follow Cullen in making jaundice a common disorder among newly-born children. The *icterus neonatorum* occurs, they say, a few days after birth; is not attended with any suffering, or obvious disturbance of the bodily functions; and soon disappears. Now there seems reason to believe that this is not icterus at all; and has no relation to the biliary organs. The surface of the infant, at its birth, is frequently of a deep red, from hyperæmia or congestion of blood; presenting a condition which falls little short of a mild but universal bruise. By degrees the redness fades, as bruises fade, through shades of *yellow* into the genuine flesh-colour. Such, I am assured by those who are more conversant with these matters than myself, is the pathology of the icterus infantum. Of course true jaundice may, as well as most other complaints, befall the earliest period of life; but I conceive that it seldom does.

The prognosis in jaundice is generally favourable; except when it depends upon structural disease of the liver, or supervenes suddenly upon some great mental or bodily shock. In both these cases the prognosis is bad, or doubtful. Intense yellowness of the skin and eyes is often more hopeful than a fainter tinge of yellow. It is better, in the variety connected with hepatic disease, if that disease proceed from some known cause, by which a low degree of inflammation has been produced; and the cause be such as can be avoided for the future. Just, indeed, as in chronic hepatitis, of which the icterus is simply an occasional symptom. The prognosis is worst of all in old persons, when the constitution is impaired, and there is no obvious cause for the disease; and particularly when the colour of the skin is greenish, or approaching to black.

LECTURE LXX.

Treatment of the various Species of Jaundice. Diseases of the Gall-bladder; of the Spleen; of the Pancreas. Diseases of the Kidneys. Nephritis and Nephralgia. Phenomena constituting a 'fit of the Gravel.' Different kinds of Gravel. Diseased states of the Urine. Description and remedies of the Lithic, Phosphatic, and Oxalic Diatheses.

In the last lecture, after describing the symptoms, causes, and treatment of acute and chronic inflammation of the liver, and after pointing out various other forms of chronic disease to which that organ is obnoxious, I spoke of *jaundice.* I offered you some comments upon its phenomena; and I indicated several different internal conditions upon which it may, in different cases, depend; and the lecture was closed with some brief hints respecting the *prognosis* of icterus. I have yet to consider the plans of treatment best adapted to the several varieties of the complaint.

Some kinds of jaundice are absolutely and obviously irremediable. In these take care not to *harm* your patient by senseless routine formalities. From others the patients recover, whatever treatment may be adopted, or without any treatment at all. Hence, as is customary in such circumstances, remedies the most worthless and absurd are extolled for their efficacy against jaundice. The patient gets well, and the drug last tried is held to have cured him. *Post hoc, ergo propter hoc,* is an argument more often applied I believe to the variations of disease than to any other class of events.

In that species of icterus which occurs, sometimes, in connection with acute or chronic inflammation of the liver, the treatment must be such as I yesterday recommended for acute and chronic hepatitis.

With respect to the icterus calculosus, our object must be to get rid of the mechanical impediment to the excretion of bile; or, at any rate, if that cannot be accomplished, to ease the acute sufferings of the patient. Should fever attend the passage of a gall-stone, or should the epigastric pain become epigastric tenderness, leeches may be applied over the painful spot. The withdrawal of blood may prevent any thickening of the distended gall-ducts: or it may perhaps relax their spasmodic closure upon the calculus, provoked by its presence. But, in general, bleeding is not requisite nor of service in this variety of jaundice. Our great resource for relieving the pain, and for loosening the presumed spasm, is opium, given in full doses: and I can add but little, with any advantage, to the directions laid down, on this head, by Dr. Heberden. 'This pain (says he) can only be assuaged by giving and repeating opium and its preparations, as often as the continuance of the pain requires them; and because this pain is very apt to return, the patient should always be advised to keep by him, as long as the distemper lasts, pills of pure opium, each weighing one grain—or what is equivalent to them—that no time may be lost in quieting a sensation which it is so difficult to endure. One of these pills may be taken as soon as the pain comes on, and it may be repeated once or twice in the course of two hours, if the pain require it; and I have often found it both safe and necessary to give much more.'

This plan of giving opium in the form of *pills*, is the more judicious, because, from their small bulk, they are more likely to be retained than draughts would be. Sometimes the stomach is so irritable as to reject even a pill. I would add, therefore, to Dr. Heberden's recommendations, that of throwing an opiate injection into the rectum; half a drachm or a drachm of laudanum, mixed with a small quantity of warm gruel; or, better still, that of implanting a commensurate quantity of morphia in the subcutaneous areolar tissue. Another very useful expedient is the warm bath. If this cannot be readily procured, hot fomentations to the epigastrium, the mustard-poultice, the turpentine stupe, are valuable substitutes for it. Dr. Prout states that he has seen more alleviation afforded by large draughts of hot water containing the carbonate of soda in solution (one or two drachms to a pint), than by any other means. 'The alkali counteracts the distressing symptoms produced by the acidity of the stomach; while the hot water acts like a fomentation to the seat of the pain. The first portions of water are commonly rejected almost immediately; but others may be repeatedly taken; and after some time it will usually be found that the pain becomes less, and the water is retained. Another advantage of this plan of treatment is, that the water abates the severity of the retching, which is usually most severe and dangerous when there is nothing present upon which the stomach can react. This plan does not supersede the use of opium, which may be given in any way deemed most desirable; and in some instances a few drops of laudanum may be advantageously conjoined with the alkaline solution, after it has been once or twice rejected.' The pain having been quelled, the bowels should be swept out by a brisk purgative.

When jaundice appears to have been suddenly engendered by moral causes, the rationale of its production is obscure; and the treatment has a corresponding uncertainty. Certain elements of the bile, failing to undergo their natural metamorphoses in the blood, are supposed to operate somehow as a poison upon the nervous system. But the mental state which precedes and seems to occasion the malady may possibly be itself the cause of the nervous symptoms that follow. In other forms of jaundice patients remain intensely yellow, often for a long time together, without becoming comatose, delirious, or convulsed. Not that this is conclusive. We know that a given poison may influence different persons very differently. The same dose of opium that will put one man to sleep, will stimulate a second to madness, and will have no sensible effect upon a third. In the very complaint before us, one patient is tormented with a universal itching, which we attribute to bile in his blood; and ten others remain free from that disagreeable feeling. The lesson which experience has furnished amounts to no more than this: that active purging is sometimes followed by evident amendment, and ultimate recovery. In all the varieties of what, from its intensity and rapid accession, I may call acute jaundice, purging is strongly indicated: and we sometimes succeed in rectifying the whole morbid condition by thus applying a sudden *wrench* (so to speak) to the biliary organs; by giving, for instance, half a scruple or a scruple of calomel, and, a few hours afterwards, half an ounce of castor oil, with half an ounce of spirit of turpentine.

When *green* jaundice arises from hepatic disease, we can only palliate. Mild laxatives and anodynes, with occasional warm baths to promote perspiration, comprise all that such a state admits of. For the icterus *gravidarum*, delivery is the natural cure: it may sometimes be removed by the careful employment of aperients.

The *gall-bladder* has its own diseases, which I do not stop to investigate, for they seldom become the objects of specific treatment. Sometimes it is found shrivelled up, and nearly empty; sometimes enormously distended; sometimes ulcerated; sometimes ruptured. Of these conditions, the distention of the gall-bladder is the only one that we can ever expect to recognise in the living body. The bag then projects beyond the edge of the liver, and is palpable externally, forming an elastic tumour in the right side. Authors lay down marks for distinguishing a distended gall-bladder from abscess of the liver, and from a hydatid cyst; but they are not much to be trusted in; nor is the precise diagnosis of any great moment. The practical rule seems to be that, when the swelling is adherent to the parietes of the abdomen, we may puncture it, whatever be its nature: but, excepting the case of a hydatid cyst, under no other circumstances.

I have already, incidentally, described most of the morbid states of the *spleen* which are susceptible of relief from medicine: especially of the enlargement of that body constituting the *ague-cake* of the fens, and occurring in connection with intermittent fever; and that other kind of enlargement which sometimes goes along with hæmatemesis and melæna. The spleen is liable to tubercles also; to deposits of other specific tumours, and of bone; and to softening of its substance. Sprinkled through it may not unfrequently be seen a number of yellowish or buff-coloured wedge-like spots, which are believed, with good reason, to be little masses of fibrin, detached by the circulating blood from the interior of the heart, or of some large blood-vessel, and arrested in the capillaries of this, as of some other organs, and especially of the kidneys. I mentioned these spots in a previous lecture.

The spleen is sometimes enlarged by the lodgment within it of a whitish substance, albuminous in character, but called by some lardaceous, from its resemblance to hard bacon; by others waxy. The Malpighian bodies are first affected. This condition is the more interesting because it is apt, as I have shown you, to occur in the liver also, and in the kidneys. Being frequently found in more than one, or in all, of those organs at the same time, we infer its origin from some cause which affects the constitution at large; there can be little doubt that it belongs often to the category of syphilitic disorders; and it has often been met with also in persons labouring under strumous caries of the bones.

Going along with certain other enlargements of the spleen (or of other glandular bodies belonging to the lymphatic system) there has been observed in the blood a greatly increased ratio of the white or colourless to the red corpuscles. Professor Hughes Bennett, of Edinburgh, who was the first to draw attention to this remarkable state of the blood, has given to it, or to the disorder which it constitutes, the name of *Leucocythemia*, or white-cell blood. It is met with in cases which present other indications, hitherto ill-defined, of infirm or broken health. Not unfrequently ascites, or anasarca, is associated with the enlargement of the spleen. As yet this disorder possesses more of physiological than of practical interest; and I must limit myself to this passing notice of it: commending to your attention one practical point only. Doubt may arise whether a tumour in the left side of the abdomen is splenic or ovarian. Dr. Hughes Bennett mentions an instance in which such a doubt was solved at once by submitting a single drop of the patient's blood to scrutiny under the microscope. It was found to be crowded with colourless cells.

The best remedy for the ague-cake is the remedy for intermittent fever, *quina*. Purgatives also have the effect of reducing hypertrophy of that curious organ. One caution enforced by Dr. Abercrombie is that, in splenic disease, mercury should be sedulously avoided, or rather such an employment of mercury as would risk tenderness of the gums. The late Dr. Robert Williams, of St. Thomas's Hospital, having made many trials of the *bromide of potassium* as a remedy in various disorders, satisfied himself of its utility only in cases of diseased spleen. Of this effect of that salt I know nothing. But I have already told you of some other of its curative virtues.

Again, it may seem a slight to the *pancreas* to pass it over without noticing the diseases to which it is subject. But really those diseases appear to be but few; and they do not signify their existence by any plain or intelligible signs. I have, nine or ten times perhaps in my life, met with carcinomatous deposits in the pancreas. In every instance the head of the gland, that extremity which lies next to the bowel, has been the exclusive or the principal seat of the disease. I have known this change in the pancreas to cause jaundice, by obstructing the bile-ducts; I have known it in the same way to occasion very great enlargement of the liver itself; and I have known it to produce enormous and slowly fatal distention of the stomach by compressing the duodenum, and so preventing the free passage of the aliment through that gut.

I have already directed your attention to the fact, and to the presumable explanation of the fact, that destructive disease of the pancreas is often associated with discharges of fat from the bowels. As to remedies for pancreatic diseases or disorders, I do not know of any.

Diseases of the *kidneys*—and disorders of their function—and alterations in the fluid they secrete—and diseases of the reservoir of that fluid, the bladder—require more consideration. I proceed at once to the subject of *inflammation* of the kidney; to *nephritis*; and it will be practically convenient to take *nephralgia*, or pain of the kidney, into account at the same time. Nephralgia is commonly, but not always, produced by the transit of a urinary calculus from the pelvis of the kidney, through the ureter, towards the bladder. This constitutes what is called, in common parlance, *a fit of the gravel*. The symptoms are these:—pain, sometimes dull, but more frequently very severe, in the loins, usually on one side, and descending often along the track of the ureter of the same side; numbness of the corresponding thigh; in the male, retraction, and perhaps pain, of the testicle; a frequent desire to make water, which is generally high-coloured; nausea and vomiting.

If to these symptoms there be added pyrexia, we learn the important fact that inflammation is present: we have the symptoms of *acute nephritis*. The passage of gravel from the kidney sometimes does, and sometimes does not provoke inflammation of the gland. Nephritis is very seldom idiopathic. It may sometimes arise under the influence of cold; more frequently it is excited by calculous matter lodged in the kidney; by a blow or fall upon the loins; by scrofulous or pyæmic disease; by the internal administration of cantharides, or of turpentine. It is to the presence of fever that we look, to establish the inflammatory character of the renal affection.

Nephralgic pains require to be distinguished on the one hand from rheumatic, and on the other from colic pains. In lumbago there is pain in the back, and it may or may not be attended with fever; but the pain usually affects both sides, and is aggravated by such movements of the body as call the muscles of the loins into action, particularly by stooping. It originates, frequently, in some strain or effort of which the patient is made painfully conscious at the time. It is seldom accompanied by any notable trouble of the urinary functions.

When rheumatic pain extends from the back into the thigh, it mostly follows the course of the great sciatic nerve, and is felt down the outer part of the limb; whereas the pain that accompanies nephritis or nephralgia shoots rather along the track of the anterior crural nerve. Lastly, lumbar pain, depending upon rheumatism, is not attended with nausea and vomiting.

The pain of colic *is* often associated with sickness and retching: and it may occupy those parts of the abdomen which correspond to the place of the ureters. The urinary functions are undisturbed; and this is a capital point of distinction. The numbness of the thigh, and drawing up of the testicle, are sufficiently characteristic, when they happen; but frequently they are altogether absent.

Some years ago I was sent for by an exceedingly intelligent surgeon, who had been one of the house-surgeons at the Middlesex Hospital. I found him in bed. He told me he had pain in the abdomen. It had begun in the morning in the situation of the right kidney, and soon extended round to the right side of the abdomen and to the groin. Two days before, he had experienced a similar attack of pain in the renal region, stretching round into the hypogastrium. When I saw him he described the pain as lying more round the umbilicus than elsewhere; and he expressed a strong persuasion, from the feelings which attended it, that it would be removed by free action of the bowels. But he felt nausea; and had vomited some medicine which he had taken. He had no fever, no retraction of the testicle or numbness of the thigh, and the pain was not increased by pressure. Neither had there been any marked irritation of the bladder. He said, indeed, when I questioned him on that point, that he *thought* he had made water *rather* more frequently than usual the day before. I mention this case to show you the occasional obscurity of the symptoms. Here a well-instructed medical man believed that nephralgia, existing in his own person, was colic. To my judgment, however, it seemed most probable that a small calculus had been passing from his kidney towards, and perhaps into, his bladder. I may as well tell you the event of the case, which interested me a good deal; for it exhibits the train of symptoms that are apt to ensue after nephritic attacks; although in this instance they were but slightly pronounced. His bowels were well acted on by a purgative, and the next day he was free from pain, and apparently well.

Two days after this, he had more frequent calls to void urine than were usual with him, and having done so on one occasion, he presently felt the want again, and then passed a little blood. The urine had been of a clear amber colour throughout. At the expiration of two or three days more he called upon me to say that after making water he had perceived in the vessel a small crystallised mass, which he took out, supposing it to be (what it very much resembled) a fragment of sugar-candy. In fact he had been eating sugar-candy, and thought some portions of it had fallen down between his waistcoat and shirt, and afterwards into the chamber-pot. He had the curiosity, he said (some misgiving he must have had too, for I had told him my own opinion of the nature of his attack), to put a small crystal from this fragment into his mouth; and as it neither tasted sweet nor dissolved, he suspected it might be a urinary concretion, and brought it to me. And sure enough it was so; a piece of very pure oxalate of lime, which he had been fortunate enough thus to get rid of. It was a quarter of an inch in length, and less than one-eighth of an inch broad, consisting of an aggregation of small crystals. It was exactly similar in appearance and colour to a piece of brown sugar-candy of the same size. It would pass, longways, into a large crow-quill.

That it was oxalate of lime was proved in this manner. A separate little crystal was heated to redness on a piece of platinum foil, by means of a spirit-lamp and blowpipe. By these means the oxalic acid was converted into carbonic acid, which was driven off by the strong heat; and quick-lime was left. This residue, moistened, and pressed into a powder on a piece of turmeric paper, gave the characteristic brown colour.

You see, then, that a nephritic affection may be mistaken for an attack of colic. In reference to practice, it would indeed be a mistake of no great importance, since the remedies that are proper in the one case are generally proper, or not improper, in the other. If the pain be attended with fever, antiphlogistic measures are alike indicated in each of the two diseases.

The numbness of the thigh, and the drawing up of the testicle, are phenomena analogous to the shoulder-tip pain which affects the shoulders in hepatic disorders. Irritation of one extremity of a nerve, situate internally, and belonging to an organ which is not endowed with a high degree of sensibility, causes sympathetic sensations in the sentient extremities of other branches of the same nerve, or of communicating nerves.

And this sympathetic affection of distant parts is sometimes attended (as I formerly observed) not merely with pain, but with some degree of inflammation also. The testicle occasionally swells, and becomes tender, during a nephritic attack. On the other hand, as the nerves which communicate with those of the testicle or thigh may or may not be implicated in the renal disorder, so we see how it happens that these curious symptoms, so instructive when they do occur, may frequently be wanting; as they were in the example I just now detailed to you.

When the symptoms I specified in the outset are attended with fever, we conclude that we have to deal with nephritis; and when inflammation of the kidney, however produced, lasts for a certain period, without abatement, *suppuration* is to be dreaded. Such suppuration is marked, sometimes, by the supervention of rigors, by throbbing perhaps, and it may be by a remission of the pain: but I believe it may take place without throwing out any such signals. Nay, I think it probable that inflammation, confined to the parenchymatous substance of the kidney, may arise, and run through all its stages, without denoting its presence or progress by any noticeable local signs; and that the sharp and peculiar symptoms ascribed by authors to acute nephritis, manifest themselves only when the investing membrane of the gland, or its pelvis and excretory tubes, are involved in the inflammatory process. However this may be, suppuration leads to ulceration, to the formation of renal fistulæ, to the establishment of a purulent discharge, and hectic fever: and finally, in most cases, to a fatal event; whether the inflammation was at first idiopathic, or dependent on a calculus.

I may illustrate these remarks, by stating the heads of a case which occurred to me some time since. I admitted Caroline Barnard, a married woman, forty years old, into the hospital, on October 18. Among other things she complained of pain in the situation of the right kidney. She had been ill six weeks, and at the commencement of her illness her urine had been very turbid, as indeed it still was: and she had experienced much pain and difficulty in passing it, and after it had passed. From that time she had frequent nausea and retching, and occasional numbness of the right thigh. She had been losing flesh fast; and her pulse was frequent. There was some tenderness discoverable in the right renal region; and after a time a manifest fulness there, and hardness; and at length œdema of the integuments and *extreme* tenderness. She suffered also well-marked hectic fever, and had severe and repeated rigors. On November 4, after a careful examination of the right loin, we satisfied ourselves of the presence of matter. I got Mr. Arnott, therefore, to see her, and to put a lancet into the abscess; and a large quantity of faint-smelling pus came out. She was greatly relieved by the operation; and a purulent discharge, mixed with shreds of cellular membrane, came away in abundance for some days: but in time the discharge ceased, the swelling subsided, and the opening healed. We began to hope that it had been merely an abscess in the *neighbourhood* of the kidney, irritating it and affecting its functions. But in three weeks after the abscess was punctured, the swelling was found to have recurred; and she again began to suffer much. The tumour was again opened, and pus, of a more offensive character than before, let out. In the early part of December she sank.

We found the right kidney small, collapsed, and hollow; in some parts a mere flabby bag. On its posterior surface there was an opening, which formed a communication between the interior of the kidney and the abscess in the areolar tissue, which had pointed externally. The pelvis of the kidney was much dilated: and the substance of the gland destroyed to a considerable extent, by suppuration and ulceration. The ureter, where it left the pelvis of the kidney, was found to be impervious.

The other kidney was much enlarged; but of quite healthy and sound structure. That kind of compensation had occurred which I formerly mentioned as not unusual when, of double organs, one has been rendered incapable of its natural functions, and the other takes up its duty, and performs a two-fold amount of work. The organ of which the function is thus increased becomes hypertrophied. This woman did not die because there was not urine enough secreted; but she sank under the wasting purulent drain, the irritation and pain she suffered, and the protracted hectic fever. In this instance the inflammation and suppuration occurred independently of the formation of calculous matter.

Sometimes the pus finds its way out of the body through the natural passages, and appears in the urine. This woman's urine was thought, by some of the pupils, to contain pus. It was quite thick, and of a yellowish colour. But heat rendered it transparent. You must not judge by a cursory look at the water. The effect of heat proved that the yellow material was not pus; the impervious condition of the ureter showed afterwards that it could not have been.

When calculi exist in the kidney, they often betray their presence there, by causing *bloody* urine. But bloody urine may proceed from various causes; and in conformity with my usual custom, I shall by and by offer you some general remarks on hæmaturia, as one of the hæmorrhages.

I showed you, at our last meeting, that gall-stones might inhabit the gall-bladder in considerable numbers, and be quite harmless, unless they attempted to escape from their prison, through the very narrow channel of egress from it; and I intimated that the same observation was often applicable to urinary concretions. Renal calculi do indeed, in many cases, produce abiding uneasiness, or frequently recurring pain, in the situation of the affected kidney, bloody urine, and gastric disturbance; especially when the concretions are shaken or displaced by sudden jolts or jarring movements of the body; or when the system is deranged by intemperate habits. But in many other instances these calculi cause no pain or annoyance, so long as they remain in the kidney: although they inflict horrible suffering, in general, while, for the first time, they are forcing their way along the narrow ureter. A concretion cannot be formed in a moment; yet the attack of pain often comes on in a moment, without any previous warning. After a while it remits, perhaps as suddenly; the calculus having passed (it may be presumed) from the ureter into the bladder; and then indications, more or less palpable, usually begin to declare themselves of its presence in that receptacle. Moreover it is not uncommon to find calculi in the kidney after death, of the existence of which there had been no symptom manifested during life.

The *treatment* of nephritis—or of the nephralgia calculosa, when accompanied by fever, or occurring in young, strong, and plethoric persons—is just such as would be proper in cases of severe colic, or enteritis: and therefore it is that any mistake between these disorders at the outset is not of so much practical consequence. The objects of treatment are, to arrest the inflammatory process; to quiet existing irritation; and to obviate any fresh causes of irritation. It will mostly be advisable to take away blood from the neighbourhood of the suffering part by cupping. Warm fomentations; the warm bath; the injection of warm water into the bowel; these are all expedients of which practical men acknowledge the value. The warm enemata not only clear out from the large intestines any irritating matters they might contain, but, from the proximity of the colon to the kidney, they have perhaps the effect of an internal fomentation. It is desirable also to get the bowels well acted upon by purgative medicines as soon as possible: the relief that follows free alvine discharges is often very marked. There is sometimes a difficulty, from the irritability of the stomach, in administering purgatives by the mouth. Calomel, however, will often be retained, when other substances are rejected. It is generally considered of importance to give those purgatives only which are not likely, after being absorbed into the blood, to irritate the urinary passages. On this account the *saline* purgatives are to be avoided. Nothing is so good as castor oil, if the stomach will bear it; or infusion of senna, with manna, may be used; or, if the stomach be very queasy, *pills*, composed of cathartic extract and calomel.

When there is no fever, i.e. when the case is one of nephralgia, and a calculus is passing, after the intestinal canal has been cleared by a purgative, it will be necessary to give opium in full doses to allay the pain: and the opiate may either be administered in the form of pill through the stomach; or introduced into the rectum, or under the skin.

When a person suffers what is called a fit of the gravel, the pain, I say, is at length very suddenly relieved, in general, in consequence of the calculus having emerged from the ureter and entered the bladder. We judge that this has taken place, first, by the cessation of the pain; and secondly, by the supervention, sooner or later, of symptoms indicative of stone in the bladder: viz. a more than usually frequent inclination to make water; pain, referred to the extremity of the urethra, especially just after passing urine; and stoppages and renewals of the stream of water while the patient is endeavouring to void it.

The time which a calculus takes in travelling from the kidney to the bladder varies a good deal. The painful journey may be over in a few hours; or it may last two or three days. More rarely the symptoms continue, with irregular intervals of comparative quiet, for weeks. And sometimes, notwithstanding the peculiar pain, which amounts to torture, all morbid symptoms cease, and yet no calculus has passed, apparently, into the bladder: none, i.e. of the symptoms of stone, ensue; no calculus is voided by the urethra; and none is found in the bladder when the patient at length dies.

What is the explanation of these circumstances? Why, as calculi have been discovered in such cases in the *kidney*, it has been supposed that a concretion may get into the very beginning of the ureter, where it is a little larger than elsewhere, and give rise to the peculiar symptoms, yet never pass fairly into that narrow tube; but at length fall back again into the pelvis of the kidney: when the symptoms cease.

But similar symptoms undoubtedly occur, occasionally, when there is no calculus at all. Sir B. Brodie has referred to this form of complaint. In people who live intemperate and luxurious lives, and in gouty persons, pain is apt to seize upon one renal region, and to extend round and downwards into the groin; and these symptoms will be followed by frequent, difficult, and painful micturition, the urine being unusually acid, high-coloured, and sometimes turbid. The whole irritation appears to be produced by this unhealthy urine: at least the complaint vanishes after cupping the loins, purging, warm baths, and frequent effervescing draughts of citrate of potash. It is highly probable that small colourless particles of oxalate of lime give rise to these symptoms.

Sometimes the little stone becomes immoveably wedged in the canal of the ureter. When it completely shuts the tube, the urine accumulates behind it, and that portion of the ureter dilates. The obstruction usually proves fatal, by its influence upon the functions of the kidney, and thereby upon the whole economy. But if the urine find a passage by the side of the impacted concretion, this danger is averted or postponed.

One occasional consequence of the impaction of a stone in the ureter is a wasting of the corresponding kidney, and its conversion into a membranous bag. I show you a specimen of this from our Museum. Both kidneys are here preserved, and you perceive that the sound one had grown to twice the usual size.

When we have reason to believe, from the nature and course of the symptoms, that a calculus has come down from the kidney and lodged in the bladder, then it becomes an object of deep interest to the practitioner, and of fearful importance to the patient, to try all means to bring about its expulsion before it grows too large to be voided. For grow it almost surely will, by the continual accretion of earthy matter upon its surface, if it remain long in the bladder. We know that it *may*, at first, be voided, provided the urethra be in a healthy and natural state; that whatever has passed through the ureter, may pass through the urethra also.

The objects to be kept in view are these: first, to procure a plentiful secretion of bland urine, wherewith the bladder may become filled; secondly, by lulling the sensibility of the parts concerned, to prevent or lessen that spasmodic effort of the sphincter of the bladder, which the presence of the calculus is apt to provoke; and, thirdly, to ascertain that the channel of the urethra is open and unimpeded.

To effect the first of these purposes, the patient should be instructed to drink copiously of diluent liquors; such as barley-water, or linseed tea, in which may be mixed a small quantity of the sweet spirits of nitre. To fulfil the second, he should take a full dose of opium at bed-time. By these means the pain and irritation which may have been produced by the calculus will be soothed; and the bladder will gradually fill. He should then make water, having first placed himself in such a position that the outlet of the bladder shall be at the lowest part of that bag. He may stand up, and lean forwards; or it may be well to make water while kneeling, in a warm bath. If these expedients are not presently successful, the urethra may be cautiously expanded, and habituated to the contact of a solid body, by the daily introduction of a full-sized bougie. Sometimes the calculus will follow the bougie, as it is withdrawn, through the urethra. In this way the patient will have a fair chance of getting rid of the stone. In this way a very near friend of my own, a physician formerly practising in this town, did expel a formidable, though not very large, piece of rough oxalate of lime several weeks after its entrance into the bladder. Out it came, at last, with a smart clink, which was music to his ear, against the chamber-pot. A gentleman was sent up to me from Kent, by a former pupil of this College, with the following history. About a month before, he had been suddenly attacked with acute pain in the loins, extending forwards into the left flank and pelvis; and with nausea and vomiting. For nearly ten days these symptoms continued to occur at intervals; then they ceased; and then he began to be troubled by a frequent and very urgent inclination to make water, and by pain after voiding it, just above the arch of the pubes. I gave him directions, in accordance with the plan just now mentioned; and wished him to allow some surgeon to explore the contents of his bladder. To this he would not as yet, he said, consent. I saw him on the 2nd of August. He returned into Kent the next day. On the 5th, while taking a walk, he was seized with a most imperative want to make water, but found that he could part with none. Concluding that a calculus had entered and stopped up the urethra, he was proceeding homeward, but was soon constrained again to try to empty his bladder: and then he had the satisfaction of feeling, and seeing, a stone fly out with great force: but, as he had turned towards a hedge, he could not find it. From that moment he was quite easy.

When a calculus of a certain size has once traversed the tubes that lead respectively to and from the bladder, others sometimes follow it with more ease. I show you here a large concretion which was passed by a patient of mine without his knowing it. He was subject to epilepsy, which was probably eccentric, and excited by renal disease. He was closely and anxiously watched by his wife. One day she noticed that the urine he had just voided was slightly tinged with blood, and she then found in the vessel this oblong stone, as big as a small bean, and composed of lithic acid.

If the renal calculus, after it has reached the bladder, cannot be got rid of by the expedients I have been recommending, the question arises, whether medicine can do any further good, or whether the patient is to be delivered over to the surgeon.

Most of these small concretions admit of being mechanically crushed into smaller fragments, which are then readily washed out by the stream of urine. Larger stones are extracted entire, through incisions of the bladder. Yet there are many cases in which, for various reasons, surgery declines to attempt the removal of vesical calculi. Medicine still offers to these unfortunate patients the means of mitigating, at least, their sufferings. But it often can do more than this. It is very important for you to know that judicious medical treatment may retard or prevent, and that injudicious medical treatment may promote and hasten the enlargement of such calculi. Let us briefly consider the principles by which our judgment and our practice, in this serious matter, must be guided.

I have described a fit of the *gravel*. We say that a patient has the gravel when he passes concrete matter with his urine, whether in the form of powder, of grit or sand, or of more massive calculi. We do not apply that term to the cases in which the urine is clear when recently voided, and still warm; but throws down a powdery sediment as it cools, which sediment redissolves if the urine be again artificially heated. Now besides the different forms which the gravel assumes, of powder, sand, and little stones, there are (as you may have guessed from certain terms that I have been obliged to employ) several *kinds* of gravel; differing, I mean, in their chemical composition. The main signs—the pain, the sickness, the affection of the testicle, the subsequent bladder symptoms—are much the same, whatever be the nature of the solid matter that descends from the kidney, and lingers in the bladder. But other circumstances differ widely. The qualities of the water previous to the formation, and to the discharge, of the sabulous matter; the state of the system at large. And it is quite impossible to treat cases of calculus in the kidney, or of stone in the bladder, or of inflammation of the bladder, with propriety or safety, without constant reference to the condition of the urine. The morbid states of that secretion are of the greatest interest. I cannot undertake to enter upon the subject in much detail. Yet some outline of it I must attempt, especially where it touches upon points of practice.

The office of the kidneys is simply excretory. Through them, and with the urine, are drained away many of the impurities, habitual or accidental, of the circulating blood; and any excess of its aqueous ingredient. It does not fall within my province to go into the chemical composition of the urine. That is fully taught in other lectures. It is enough to say that it is complex, and perpetually shifting. Everybody knows by his own observation that, compatibly with the most perfect health, the urine may vary considerably in its sensible qualities; in quantity for example, in colour, in its specific gravity. The average diurnal quantity is from thirty to fifty ounces. If much liquid be drunk, more urine is secreted. If much water pass off by the skin, or through the bowels, less passes by the kidneys; and contrariwise. Its natural colour resembles that of wheat-straw, of amber, or of pale sherry. Its ordinary specific gravity lies between 1015 and 1025; that of distilled water being represented by 1000.

You know, probably, that the urine voided by a person in health always exhibits *acid properties*, always turns litmus-paper red. Not that healthy urine contains a free acid; but only that certain of the alkaline and earthy bases are not exactly neutralised, but exist in the state of supersalts. Of these, most probably the acid phosphate of soda is the one which usually gives the acid reaction to the urine. You ought also to be aware of certain variations which take place, with much regularity, in the acidity of the urine. Want of attention to this point—or rather the want of knowledge on this point—has been a fertile source of mistakes. Dr. Bence Jones has ascertained that the urine is most strongly acid just before a meal, and that it gradually becomes less and less acid while digestion is going on. The acidity is least about three hours after breakfast, and five or six hours after dinner. His explanation of these changes is this. During digestion, soda is set free in the stomach and finds its way into the blood, and so into the urine. The acidity of the gastric fluids, and the acidity of the urine, are in inverse proportion. Dr. Roberts suggests a different explanation, namely, that as a meal implies a dose of alkali, it must add, for a time, to the alkalescence of the blood, and that the urine presently reflects that addition.

Modern chemistry teaches (I repeat) that the acid reaction of healthy urine is commonly due to the acid phosphate of soda. Dr. Prout ascribed it to a super-lithate of ammonia; but it seems doubtful whether a super-lithate of ammonia ever exists. Neutral lithate of ammonia, however, is very readily soluble in the saline urine. But, whether out of the body or within it, the lithate of ammonia will, of course, be decomposed if any free acid be present in the urine, for which ammonia has a stronger affinity than it has for the lithic acid: and the latter, being insoluble, will be slowly thrown down, in the form of a red or yellow sand: little crystals, in point of fact, they are; to the naked eye very often like, in shape, size, and colour, to particles of Cayenne pepper. Under the microscope the lithic acid is seen of various forms, according to the nature of the urine from which it is deposited. Of these forms the most marked are here exhibited (Plate I.). I show you also some of this red sand, collected by one of my out-patients at the hospital. He must have passed a peck of it while under my observation: and I am sorry (having lately lost sight of him) that I did not procure a larger quantity for the Museum.

Now this lithic (or uric) acid, or red sand, or gravel, is liable to *form* in the *kidney* (not in the bladder) * and to concrete into calculi; and a calculus once formed, or, indeed, any solid substance, will constitute a nucleus, upon and around which a further and repeated incrustation of a similar nature is almost sure to take place. You will at once perceive the importance of doing nothing to aggravate this disposition to deposit lithic acid; but of trying to prevent or stop it. If there be symptoms of stone in the kidney, or in the bladder, and we have reason to believe that it consists of lithic acid, there are medicines which would tend to make matters worse, and there are others of which the effect would be to correct the lithic acid *diathesis*, as it is called. But how are we to know whether the presumed calculus be of that kind or not? or, rather, how are we to know that the lithic diathesis exists? Why, we learn that it exists by noticing the habitual qualities of the urine, and the habitual state of the patient's general health.

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* Among 59 renal calculi in the Hunterian Museum, the late Professor Brand found that 52 consisted of lithic acid, 6 of oxalate of lime, 1 of cystine.

The urine of persons who have the lithic diathesis is bright, of a dark golden or coppery colour, like brown sherry. Sometimes it feels slightly pungent in the urethra as it is passing. It is more acid than the urine of health, and gives to blue litmus-paper a deeper shade of red. Commonly it contains more than the usual amount of urea (Plate II.), and has a high specific gravity. It is apt, too, to fall below the average quantity.

The lithic acid is not often thrown down before the urine is voided. When it is, it appears in separate crystals, in the shape of fine sand; or in coarser roundish grains, which are in fact minute concretions of crystals.

You must not confound this crystallised lithic acid gravel with those amorphous powdery deposits which are much more common and more copious, and which consist of lithic acid in combination with ammonia, lime, magnesia, or soda. They are generally spoken of as lithate (or urate) of *ammonia* (Plate I.), but are chiefly composed, I believe, of lithate of soda. The colour of these lithates is sometimes pale and almost white; more often of a yellowish brown; or red, like brick-dust; or occasionally of a deep purple or almost crimson tint. They are never deposited till the urine has cooled. People are liable to be frightened by their appearance; apprehending that they may harden into a stone in the bladder. But you may always relieve their anxiety by stating that these sediments are never substantially present in urine at the temperature of the body. You may show that they presently dissolve and vanish, as the urine is again warmed. The lithic sand does not so disappear. The lithates are apt to stain the surface of the vessel, and they render the whole of the urine turbid when it is shaken: whereas the lithic sand rolls over at the bottom when the vessel is slowly tilted, and does not trouble the general transparency of the stirred water.

These, and all other urinary deposits, are in most instances discriminated more easily, more quickly, and more surely, by means of the microscope, than in any other way: and you may now carry in your waistcoat pocket a microscope which is practically sufficient for this purpose, and perfectly simple in its use. A single glance at a drop of urine containing the sediment will reveal its character with more accuracy than could be attained by a long and laborious chemical analysis. The lithate of soda appears in molecular granules, which are often arranged in little tufts, and look like fragments of moss. True lithate of ammonia sometimes presents itself in a rounded form, with one or more little projecting spikes—resembling a minute thorn apple. (Plate I.)

Now the lithates of which I have been speaking do sometimes—do not seldom indeed—show themselves in urine which deposits the lithic acid crystals also; and you must then warm the urine, and disperse the lithates, before you can obtain a clear view of those crystals. But in such cases, the nature of the disorder, and its proper treatment, are both determined by the presence of the lithic acid; and the concomitant lithates are of secondary importance. I shall have something further to say of them, however, presently.

The presence of the so-called lithic diathesis is likewise accompanied, and so far denoted, by a tendency to feverish and inflammatory complaints. The patients are troubled with transient twinging pains in their limbs, and many of them are subject to gout or rheumatism. They are mostly also indolent and luxurious, or intemperate in their mode of life. Adults are peculiarly obnoxious to this condition of the system after the age of forty. But children, up to the period of puberty, are very liable to have lithic acid gravel: and in this early period of life such deposits indicate, according to Dr. Owen Rees, a tendency to grave disease. The frequency of uric acid sand in the urine of children is, perhaps, referable to their milk diet, and consequent acidity from lactic acid.

Whenever a paroxysm of nephritic pain befals a person whose time of life, whose habits, the character of whose health, and the habitual qualities of whose urine, are such as I have been describing, you may conclude that the concretion which has occasioned the symptoms is of the lithic acid kind: and you may expect that such attacks will recur: for it is observed of these lithic acid renal calculi, that they are generally numerous in the same individual. I speak of the *habitual*—or of the *frequently recurring*—qualities of the urine: for a deposit of lithic acid gravel, as well as of superabundant lithates, may occur to the healthiest individual, under accidental and transient disturbing causes. Many persons will tell you that their water presents a red sand whenever they have a cold. Febrile and inflammatory ailments may produce the sediment: even too full a meal: or exercise taken immediately after a full meal. In all such cases it seems probable that the customary evolution of free acid through the skin is somehow prevented: in consequence either of a check given to the perspiration, or of imperfect assimilation of the food. The free acid thus diverted from its natural emunctory—or some acid introduced from without, or generated within the system—is determined to the urine, and precipitates the lithic.

Dr. Bence Jones, in a paper which you may read in the 'Philosophical Transactions' for 1845, has pointed out one way in which an excess of free acid sometimes comes to the urine. From disorder of the stomach an excessive quantity of free hydrochloric acid is there secreted, and remains there during the whole process of digestion. If urine be at that time passed, it may be found alkaline from fixed alkali; and so it continues until the contents of the stomach are absorbed, when the free acid which was in the stomach passes off in the urine, making it intensely acid, and precipitating lithic acid, or the lithates, according to the quantity of free acid, and the length of time during which the urine (after being secreted by the kidneys) is submitted to its action.

Now the formation of lithic acid in the urine attests its over-acid condition, and both the one and the other may be controlled by the exhibition of alkaline remedies. You will find that free livers use alkalies to neutralise the excess of acid which results from their intemperate habits; the carbonate of soda, or of potass. They do this, without any reference to the appearance of their urine, to prevent or appease the uneasy feelings produced by a debauch. But it is of importance to be aware that one of these alkalies is preferable, for the purpose of obviating the lithic acid deposits, to the other. Soda will sometimes combine with the lithic acid, and form an insoluble salt, as hard, and as pernicious, when deposited around a nucleus, as the lithic acid itself. With potass there is no such danger. If it should combine with the lithic acid, the resulting salt is perfectly soluble, and will pass away dissolved, in the urine. Magnesia is also a good medicine in such cases; but it has this disadvantage, as I showed you indeed before, that it is apt, when taken habitually, to cause *intestinal* concretions; and these may be as dangerous as the urinary ones. One of the best modes of giving the bicarbonate of potass is in the common effervescing saline draught. The salts of vegetable acids are converted, *in transitu*, into carbonates. The change appears to take place, not in the stomach, but in the blood; and to be caused by the action of oxygen. The remedial properties of the bicarbonate, thus administered, are the same with those of the pure alkali, while it is much less likely to derange or disagree with the stomach. The phosphate of soda is a powerful solvent of lithic acid; and this salt has been suggested by Dr. Golding Bird as a suitable drug in these cases. It tastes, in a dilute solution, somewhat like common salt; and it may conveniently be taken in scruple or half-drachm doses, dissolved in broth or gruel. The Vichy water, which contains a large quantity of carbonate of soda, and is a very fashionable drink among us, is not so safe a beverage, under these circumstances, as potash water, for the reason just now assigned. Of course the mode of living ought to be changed when the lithic diathesis prevails; the patients should dine moderately and plainly, eating of one dish, and avoiding acids and all articles of diet likely to generate acid in the stomach; saccharine substances therefore, starch in all its forms, and fermented liquors. But, as I remarked in a former lecture, they will not, if they can help it, give up their accustomed indulgences: and they attempt, and we attempt, but the attempt is often made in vain, to *remedy* disorders which might with ease and certainty have been *prevented*.

According to Sir Henry Thompson's experience, about three-fifths of all urinary calculi consist of lithic acid or lithates.

You must take care not to give these alkaline remedies too long; nor in too great quantity. You must not push them to such an extent as entirely to destroy the acidity of the urine: for if you do, your patient is exposed to the same danger as before, but from an opposite cause. A *white* sand or gravel will be apt to form in the alkaline or neutral urine: and this will collect itself, by the force of aggregation, around any existing calculus, or foreign substance. The white deposits consist mainly of the triple phosphate of ammonia and magnesia, mixed with amorphous phosphate of lime; and if you examine collections of urinary calculi, you will find that they are not seldom made up of concentric layers; and one layer may be composed of lithic acid, and the next of the mixed phosphates; and so on, as the condition of the urine has alternated. You must test the urine therefore, and see that it still reddens litmus, though perhaps faintly. Indeed it may do so without containing acid enough to dissolve all the earthy phosphates, if they are present in excess; so that urine which only slightly reddens litmus paper may nevertheless be capable of sometimes depositing the earthy phosphates. The saline draught has always a tendency to make the urine alkaline; and thus it is, probably, that it proves of use in febrile disorders; but it may become a poison to those whose urine is already alkaline. Colchicum has a similar tendency to diminish the acid reaction of the urine. So has mercury. And I may tell you—speaking generally of morbid states of the urine—that it is much more easy to correct too great acidity than to rectify the opposite condition. We can almost always make acid urine neutral or alkaline: but to render alkaline urine acid is often beyond our power.

In truth, the administration of alkaline drugs, which, by clearing the urine of superfluous acid, staves off a present danger, does nothing towards redressing that state of the system from which the excess of acid, and the danger, proceed. Alkalies operate upon an effect, but leave its cause untouched. A long continuance of them may even tend, apart from their immediate effects upon the urine, to lower the general tone of the body, to render the muscles flabby and weak, and the complexion pale. To cure the morbid disposition the patient's regimen must be prescribed. His diet must be regulated, as I have explained already: and it is scarcely less important to attend to the functions of the *skin*, in persons having the lithic acid diathesis, than to the functions of the stomach. The warm bath is often an excellent adjuvant in their treatment: or, where it can be borne, the daily use of the cold or tepid sponging bath, with subsequent friction by the flesh brush, or hair-glove. In cold weather warm clothing must be enjoined; and the avoidance, in all weathers, of such exposure to cold as might suppress or materially lessen the amount of healthy perspiration.

Active exercise in the open air, furthering the removal of acids through the skin, and of carbonic acid through the lungs, is also of great importance: and it may be requisite to promote the healthy action of the liver and bowels by mild aperients containing a small proportion of mercury.

A word or two more about the deposit of *lithates* in the urine. I have already remarked upon the variety in their colour. When this does not result from the presence in the water of some accidental colouring matter, useful inferences may occasionally be drawn from the peculiar tint of the sediment. Those deposits which have a tawny or reddish yellow, or what Dr. Owen Rees describes as a nut-brown hue, are the most innocent. They are frequently the effect of mere indigestion, of a common cold, or of some other slight and transient disturbing influence. The white lithates are of more equivocal import. They seem to precede sometimes, or to accompany, the excretion of a small amount of sugar through the kidneys. They should suggest vigilant care and inquiry. Those which present a pink or brick-dust colour are mostly associated with febrile states of the body, and are common in acute rheumatism. When such sediments are habitual, and without fever, they are often connected with organic visceral mischief. The purplish or crimson deposits were believed by Dr. Golding Bird to be 'almost pathognomonic of disease in the organs in which portal blood circulates.'

The quantity of lithates contained in healthy urine varies continually and considerably: it is generally the greatest a few hours after a meal. The precipitation of these natural constituents of the urine depends upon several distinct causes.

Do not fall into the common and not unnatural mistake of supposing that urine which throws down the lithates, is therefore and necessarily over-acid urine. It may be so: but very often it is not so. The less acid the urine is, the more of the lithates is it capable of holding in solution, and so concealing. If the urine be very full of them, and at the same time be feebly acid or neutral, or alkalescent, there may be no precipitate. Again, the urine may be very acid, yet if it contain but a scanty amount of lithates, none of those lithates may be rendered visible. The most favourable condition for their sinking down is when there is present a slight excess of their average quantity, and also a slight excess of acid in the urine.

Two other circumstances must always be taken into account: viz., the absolute quantity of the urine itself, and its temperature.

The amount of lithates being the same, they will be less readily retained in solution as the quantity of their aqueous menstruum diminishes: and the colder that menstruum becomes, the less of the lithates will it be able to hold dissolved.

Hence we see how a slight cold, which implies generally some check to the perspiration, and a scantier secretion of urine, is apt to be accompanied with a deposit of the lithates. The appearance will be augmented, if the temperature of the atmosphere be low. It occurs in frosty weather often, when there is no appreciable derangement of the health whatever. It is clear that these are not cases for active alkaline remedies. All that they require is warmth to the surface, diaphoretics perhaps, to preserve the balance between the skin and the kidneys, or gentle diuretics, a few grains of nitre for example, to increase the quantity of urine excreted.

Bear in mind, then, that the two conditions, of increased acidity of the urine, and of an excess of the lithates or of lithic acid, may concur; but they require to be distinguished. There is no necessity for their coincidence. They are constantly met with separate and distinct the one from the other. In gout, in indigestion, and in some other disorders, there appears to be an absolute increase in the amount of uric acid or of urate of soda; and to this state of the system the term *lithic diathesis* ought, in strict propriety, to be confined.

You will have gathered, from what I have already said, that there is a morbid condition of the body, the opposite of that which is characterised by a prevalent deposit of the lithic acid: a state in which a readiness is manifested to throw down *white gravel*, and to which, by high authority, the title of the *phosphatic diathesis* has been annexed. But this phrase also will be apt to mislead you, unless you are made aware of what it means, and of what it does not mean. It does not imply, then, any excess of the phosphates in the urine: while it does signify their frequent appearance in that secretion under a substantial and visible form; in one word, their deposit. And as the deposit of the lithates depends often upon a superabundance of acid in the urine, so that of the phosphates is determined by the opposite condition, by a deficiency of acid, by alkalescence of the urine. And even with respect to this alkalescence, some further distinction is needed. There are two kinds of alkalescence:—alkalescence from the presence of a fixed alkali,—the carbonate of potass, or the carbonate of soda, or the alkaline phosphate of soda; and alkalescence from the presence of the volatile alkali,—the carbonate of ammonia. Urine alkaline from this last cause,—well known as ammoniacal urine,—especially indicates the phosphatic diathesis of Dr. Prout.

Earthy phosphates are very insoluble in alkaline fluids, and very soluble in dilute acids. The minutest trace of these phosphates will be made visible if the urine become anyhow alkaline; and a very great excess of them will be hidden from the eye in urine that is healthily acid, by their ready solution therein.

Here then, as before, the old rule, 'de non apparentibus et de non existentibus,' would be fallacious. The non-appearance of the phosphatic deposits has been mistaken for the absence of phosphatic salts from the urine; and their appearance has been wrongly assumed to denote the presence of those salts in excess. All this has been clearly laid down by Dr. Bence Jones.

The white gravel which is deposited in that condition of the system to which Dr. Prout has applied the term phosphatic diathesis, but which is better characterised by the prevalence of ammoniacal urine, is composed of minute shining prismatic crystals of a triple salt, the phosphate of ammonia and magnesia. (Plate I.) The way in which this is formed, according to Dr. Prout, is as follows. Healthy urine contains the phosphate of magnesia, which is very soluble, and therefore is dissolved, in that fluid. But, under certain circumstances, the urea of the urine becomes decomposed in the kidneys, or in the bladder, and ammonia is extricated, which combines with the phosphate of magnesia, and forms a triple salt, insoluble in the slightly alkaline urine. Almost always with the triple phosphate just mentioned there is also an admixture of phosphate of lime in the shape of an exceedingly fine amorphous precipitate. Thus is formed what has been called the *fusible calculus*, which is very common. Phosphatic urinary calculi constitute chiefly about two-fifths of the whole number; with the exception of about three or four per cent. of oxalate of lime calculus, to be presently described. Phosphatic calculi are sometimes formed in the kidney, but usually in the bladder.

The tendency to deposit the mixed phosphates, with a predominance of the triple phosphate, is accompanied frequently with local disease in some part of the urinary apparatus, especially in the bladder and prostate gland. It is no unusual consequence of injuries of the back,—or rather of some morbid state of the spinal cord. The immediate link in the chain of connexion between the cord and the urine, in these cases, seems commonly to be a chronic inflammatory condition of the mucous membrane of the bladder; the decomposition of urea being effected by the altered mucus. The urine is sometimes pale, sometimes of an orange or copper colour. It always contains crystals of phosphate of ammonia and magnesia, and in many or most cases pus globules (Plate II.) also are made visible by the microscope. Upon its surface a sort of film is often formed, exhibiting, as you look at it in different lights, the various colours of the rainbow; an iridescent pellicle. This has been found to consist of the triple phosphate, and sometimes of the phosphate of lime. If you skim the pellicle off, by placing a bit of paper under it, and then suffer the paper to dry, you may distinctly see the little crystals. Urine of this kind speedily grows putrid and highly offensive, and has a strong ammoniacal smell. It turns reddened litmus paper blue, but as the paper dries the red colour reappears. The alkalescence is from the carbonate of ammonia.

This, then, is the most usual form of the phosphatic deposit: the urine becoming alkaline after it is secreted, and precipitating the earthy phosphates. But I have stated that the urine may be alkalescent from a fixed alkali; and then, no ammonia being present, the triple phosphate of ammonia and magnesia is not thrown down, but the phosphate of lime alone falls as a fine white powder, or forms a scum of the most iridescent appearance on the surface. In these cases the urine is *secreted* alkaline, and frequently an over-acid state alternates with this alkalescence. The urine itself is pale, copious, slightly turbid or opaline, of a low specific gravity, and it does not smell like healthy urine: occasionally it has somewhat the faint odour of weak broth. The white sand is deposited as the water cools, and sometimes even while it is yet warm, and in the bladder; so that the last portion of the issuing stream looks milky. By such urine, reddened litmus paper is made permanently blue. No prismatic crystals of the triple phosphate can be seen in it, nor any pus globules detected; although mucus, and oxalate of lime octohedra (Plate I.), are often present, mixed with an amorphous, or more rarely a crystalline deposit of phosphate of lime.

There *are* conditions of health in which the absolute amount of earthy and alkaline phosphates together is increased, and to these conditions the term phosphatic diathesis is appropriate. The amount of earthy phosphate alone depends so much upon the quantity of lime or magnesia present in the urine, that the excess of that salt only might rather be considered to denote an *earthy* diathesis: while the epithet 'phosphatic' might be limited to that in which there is an increase of the phosphoric acid (combined always with alkaline or earthy matter) in the urine. The total amount of the phosphates in the urine never displays itself to the eye, for the phosphates of soda, which are the most abundant of them, are held in solution whether the urine be acid or alkaline.

Bear in mind that an excess of the lithates, and an excess of the phosphates, are perhaps both of them rare; while variations in the acidity and alkalescence of the urine are certainly exceedingly common. Even during perfect health the urine not unfrequently becomes alkaline during the process of digestion; the alkalescence being then always due to fixed alkali. Pink litmus paper dipped into it becomes blue, and remains so when dried.

That the prevalence of *ammoniacal urine*,—associated as it commonly is with disease of the mucous membrane of the urinary passages, which disease is often itself the result of some profounder fault in the nervous system,—that such urine, when constant or frequent, denotes a very unsafe and unpromising condition of the body, you will readily believe.

Alkalescence of the urine from the presence of *fixed alkali* has not this morbid or threatening character. According to Dr. Roberts, the amorphous phosphate of lime shows no disposition towards accretion into calculi; and is unirritating to the mucous membrane of the bladder. The urine may remain or be kept alkaline without risk of any deposit of phosphates upon an existing stone, provided it be not alkaline from the presence of ammonia. Dr. Roberts kept the urine alkaline for three months in a lad, who was then cut, and had extracted a considerable mulberry calculus, without any trace of phosphatic deposit.

Persistent alkalescence, however, of the urine, from whatever cause, signifies that the health is below its natural standard. You refrain, in such cases, from all drugs or measures that are calculated to lower the vital powers; from mercury and colchicum; from bleeding; and even from active purgation; or you may add to the patient's dangerous weakness; may promote, in certain cases, the more abundant deposit of the mixed phosphates.* But you may do more than abstain from what is hurtful: you may counteract the alkalescent tendency by a generous diet and by the exhibition of tonic medicines; bark, wine, and acids: the muriatic acid, or the nitric, or both together, may be given in such cases before meals with vast advantage sometimes. Opium is also a remedy to be employed in this form of disease. No single drug probably has so much power in rendering alkaline urine acid, as opium. And it is indicated for other reasons; it composes the nervous anxiety to which these patients are mostly a prey. Mental relaxation—freedom from care—the relinquishment of all exhausting habits and pursuits—exercise in fresh air—these too are points of vast importance, whenever they are attainable.

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* Dr. Owen Rees believes that in cases of alkaline urine, associated with a morbid condition of the mucous membrane of the bladder, the urine, as secreted by the kidney, is over-acid, and tends to keep up, or to aggravate that

There is yet another diathesis sufficiently common and important to claim your best attention. I mean the *oxalic*: in which there is a tendency to the formation, in the *kidney*, of the oxalate of lime, or *mulberry* calculus; an epithet derived from the occasional resemblance of the concretion to that fruit, in respect of colour and inequality of surface. This diathesis is not so obvious as the other two, but it is no less real. By the use of the microscope we find that it is nearly as frequent as that in which the lithic acid, or the lithates, are precipitated, and far more common than that which is marked by deposits of the phosphates.

The urine differs much in its sensible qualities from that of both the preceding varieties. Unlike the urine of the phosphatic character, it is often bright and clear; unlike that of the lithic, it is remarkably free from sediment. The mulberry calculus is solitary also; or recurs at long intervals; and is chiefly met with during the prime of life. In both these particulars the contrast with the habitudes of lithic acid concretions is striking.

The persons who manifest this disposition are usually dyspeptic; sometimes very much so; sometimes very slightly. They are uneasy during the assimilation of their meals;

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morbid condition; which, in its turn, renders the urine contained in the bladder alkaline. He counsels, therefore, an alkaline treatment in such cases, and indeed in all inflamed states of the urinary mucous surfaces. In this way the urine, as first secreted, may be made alkaline, and then the inflamed mucous surfaces, no longer irritated by an acid fluid, recover themselves and cease to pour out their alkaline liquor; till at length the healthy and acid secretion from the kidney is voided through the urethra.

The soundness of the theory can be tested only by the success of the practice. I can say nothing on the subject from my own experience.

suffer flatulence when the stomach is empty; prefer vegetable diet to animal; are fond of sweets, especially of sugar. They are liable to boils and carbuncles, and to scaly cutaneous eruptions. According to their original temperament, they are nervous and irritable, or dejected and desponding in mind. A nephritic attack relieves them from all this discomfort for years perhaps. When the oxalic diathesis is strongly marked, the skin, Dr. Prout says, 'is apt to assume an unnatural appearance difficult to describe, but the colour of which may be said to vary from dull greenish yellow in the sanguine, to dark olive or livid in the melancholic temperament.' Young and old appear to be equally liable to this diathesis. In scrofulous children it is constantly to be observed. In fact, most of the residents in large towns who suffer even the slightest dyspeptic symptoms will be found to pass more or less oxalate of lime in their urine.

The formation of the oxalate of lime within the body depends, according to Dr. Prout, either upon the non-assimilation of oxalic acid taken with the food, or upon the mal-assimilation of saccharine aliments. Hence, as a general rule, both curative and prophylactic, *sugar* and other saccharine substances should be rigidly excluded from the diet of these patients. They should avoid, also, all kinds of fermented liquor. The young stalks of the *rhubarb*-plant, which of late years have come into such general use in this country for tarts in the spring; and *sorrel*, of which our neighbours, the French, consume a good deal in salads, and in other ways; both contain oxalic acid; and *hard water* contains lime. Dyspeptic persons who drink such water, and eat such articles of food, and are thus daily introducing, without suspecting it, the constituent ingredients of the mulberry calculus, are very likely indeed to incur the pain, and the exceeding peril, of a renal concretion of that kind. You must see, therefore, the great importance of detecting the oxalic diathesis; and of forbidding, to those who have it, all such viands as contain the oxalic acid, and of recommending them to use pure water, even distilled water, for drinking. Animal food, and the stronger farinaceous matters, and weak brandy-and-water rather than beer or wine, are best for them.

Professor Liebig first pointed out the close relation of uric acid to urea and oxalate of lime. He showed how these last two may be formed from uric acid in the laboratory: and some later experiments on animals by Woehler, have proved that they actually are so formed, within the body.

Agreeing with Dr. Prout, that the mulberry *calculus* is not of very frequent occurrence—Dr. Golding Bird first made us acquainted with the fact that small octahedral crystals of the oxalate of lime—looking under the microscope like little folded envelopes (Plate I.), or sometimes like minute dumb-bells—are extremely common: although from their transparency, and from their having nearly the same refractive power, and nearly the same specific gravity with the urine in which they exist, they do not frequently disclose themselves to the naked eye, nor sink down in manifest deposit. They are made plainly visible by the microscope: though sometimes its highest powers may be required, and it may be necessary to leave the urine for twelve hours after it has been voided, in order to allow the oxalate to crystallise out. From the quantity of epithelium (Plate II.) which usually accompanies the crystals, a degree of cloudiness of the urine is frequently perceptible.

The same writer states also that the persons whose urine is thus charged with crystals of oxalate of lime are, for the most part, exceedingly sensitive and irritable, hypochondriacally apprehensive of impending evil, full of gloomy fears concerning their bodily and mental powers, dyspeptic, weak, and usually emaciated. But this description applies to extreme cases only. In both adults and children, slight cases present no symptom whatever; and it is only through the revelation of the microscope that they can be recognised.

With respect to direct remedies for this diathesis, Dr. Prout tells us that he has seen more benefit derived from the mineral acids, alone or combined with tonics, than from any other. And certainly I have myself seen, in numberless instances, and at first not without marvelling, a vast improvement in the condition and feelings of such patients follow speedily upon the administering of the nitro-muriatic acid, in moderate doses, about half-an-hour before their several meals. Preparations of iron also are advisable if the patient be anæmic: and besides the prescription of these drugs, the avoidance of any excessive addiction to study, or to business and its cares, and (as far as may be) of mental anxiety and worry, should be at the same time enjoined. The effects of the acids require to be watched: and when they begin to produce a deposit of the lithates, or of lithic acid, their use must be suspended. Dr. Prout was in the habit of recommending for patients who happened to live at a distance, the muriatic, or nitro-muriatic acid, till the lithates, or the lithic acid, began to appear in the urine; or for a *month*. 'By adopting,' he says, 'such a course of acids three or four times in the year, and by a carefully-regulated diet, I have seen the diathesis gradually subdued, and at length removed altogether.' Dr. Bird also testifies to the efficacy of similar measures.

There is another diathesis still, named after the *cystic oxide* (Plate I.), a substance which is chiefly remarkable for the quantity of sulphur which it contains. This is but rarely met with; yet it is worth mentioning.

These observations will serve, I hope, in some degree, as landmarks to guide your treatment of patients labouring under renal or vesical calculi, or presenting symptoms such as warrant the apprehension that disorders so fearful may occur. It is impossible for me to do full justice to this important subject in these lectures; and I must refer you, for more minute and the most recent information respecting it, to Dr. Golding Bird's work on 'Urinary Deposits'; to the published 'Lectures' of Dr. Owen Rees; to various papers in the 'Lancet,' and in the 'Philosophical Transactions,'—full of original research and interest,—by Dr. Bence Jones; to Dr. Beale's volume on 'Kidney Disease and Urinary Deposits'; to Dr. Roberts' instructive book on 'Urinary and Renal Diseases'; and to Sir Henry Thompson's 'Clinical Lectures on Diseases of the Urinary Organs.'

Intimately connected with urinary calculous disorders, sometimes as their cause, sometimes as their consequence, is inflammation of the mucous lining of the bladder. Whether in an acute or in a chronic form, *cystitis* is seldom an idiopathic malady. Acute cystitis, which is not very common, often owes its existence to misplaced or maladroit curative efforts of our craft: to mechanical irritation and injury in instrumental explorations of the bladder, or in the operations of lithotrity and lithotomy; to cantharides applied in blistering the skin; or to the same drug, or to turpentine, swallowed as a remedy.

The inflammation provoked by mechanical violence, or by the friction of calculi, occupies probably the whole of the membrane, is attended with fever, with pain and tenderness above the pubes, with very frequent and very urgent desire to pass and great pain in passing water, which may soon become foul, purulent, bloody, and mixed with shreds. This dreadful condition often ends in early death. It falls within the domain of the surgeon. So does that more limited vesical inflammation which is simply an extension from gonorrhœal inflammation of the urethra.

When the inflammation has been excited by the poison of cantharides or of turpentine, the danger is generally little, while the distress is great. Urine is passed frequently and in small quantities with much pain, and sometimes it is mixed with blood. The symptoms are those of the acute form in a mitigated degree, and the complaint is usually spoken of as strangury. It is soon recovered from; and the remedies are absolute rest, copious diluent drinks containing small quantities of citrate of potash to diminish the acidity of the urine, and henbane in full doses.

Chronic cystitis is a frequent sequela of the acuter disease. It originates also very often in the habitual detention of a portion of the urine in the bladder, which is unable to expel the whole of its contents in consequence of some mechanical impediment to their exit, such as an enlarged prostate; or in consequence of feebleness of its own muscular fibres: and this feebleness may be of a paralytic character, and depend upon spinal disease; or it may result from some previous overstretching of the bladder by undue and long delay of their expulsion: or it may be simply an incident in the general weakness and decay which creep upon us in old age.

In a bladder which never gets itself thoroughly emptied, the residual urine is apt to undergo decomposition, to become alkaline and irritating to the surface with which it remains in constant contact. What is voided is found to contain a muciform, tenacious, and stringy substance, mixed with more or fewer pus globules.

Sir Henry Thompson suggests a very necessary caution in respect of the indications furnished in these cases by the urine. The first portion that issues should be kept by itself in a wine-glass, while the remainder may be passed into a larger receptacle. If the morbid products just spoken of are found in the wine-glass *only*, they bear evidence of an inflammatory condition of the urethra or prostate. The other portion tells us of the state of the bladder.

It is not always that the persistent retention in the bladder of a certain quantity of urine leads to these consequences. Sir Henry Thompson remarks, that although the bladder has not been completely emptied for months, or even for a year, the urine may still remain clear and healthy.

The results of his large experience help us to decide whether, in a given case, the partial retention is attributable to enlargement of the prostate. He has never known such enlargement to occur before the age of fifty-four. It usually takes place, if at all, from the fifty-seventh to the sixtieth year. The man who, up to sixty-five, has escaped it, will be likely to escape it altogether.

The most indispensable part of the treatment of chronic cystitis resulting from the abiding retention of a portion of the secreted urine, is the removal of its cause. In the first place, the subject of the infirmity should take pains to empty his bladder completely; not by violent straining efforts, but by patiently waiting for a renewal of the stream after its first stop, till by some recovery of its spent contractile power a further portion, and perhaps yet another, is slowly emitted from what has quaintly been spoken of as the stammering bladder. The ultimate discharges may also be helped by drawing forwards the urethra, whereby the bladder is, as it were, milked of its remaining contents. Should these means fail, the residue of the urine should be drawn off by means of a catheter: and it will be well worth the patient's while to learn to introduce the instrument for himself. Facility in doing this is easily acquired; and he will thus secure a safeguard against the mischief which the stagnant dregs of the decomposing urine tend to produce, or to increase. Dilution of the urine by bland drinks, containing, when the urine is acid, medicines such as the citrate of potash, is calculated to diminish without extinguishing its acidity. Other drugs, in the more chronic forms of the complaint, unattended with fever, sometimes do good by their astringent, tonic, or balsamic qualities. Among these may be counted the tincture of the sesquichloride of iron, uvæ ursi, triticum repens, buchu, tannin, benzoic acid, and benzoates: and very striking improvement, and sometimes complete recovery, has followed the administration of copaiba and of cubebs, in these cases. Acids are occasionally useful, and among them fresh lemon-juice, taken before meals, is perhaps the best.

LECTURE LXXI.

Suppression of Urine. Diabetes; Qualities of the Urine; Symptoms; Anatomical Appearances; General Pathology of the Disease. Treatment. Diuresis.

Systematic writers have adopted the term *Ischuria*, to express that condition in which no urine is voided. It includes, therefore, those cases in which no urine is secreted; and those in which, although secreted, it is not discharged from the body. Now these two conditions are exceedingly different from each other in most respects; and I shall prefer making use of the two plain English names, *suppression* of urine, and *retention* of urine. Even these terms are sometimes confounded with each other. In *suppression*, the secretion is suspended: in *retention* it may be as active as ever. Retention of urine is a surgical complaint; involving points of great practical interest. Suppression belongs to the physician; and the technical term for it is Ischuria *renalis*. It is sometimes spoken of as paralysis of the kidney; a phrase to which I object, because I think palsy is a word which ought to be restricted to a loss of power over the muscular fibre.

Absolute suppression of urine, though a very grave, and for the most part a fatal affection, is probably in all cases a mere symptom rather than a substantive disease. You will remember that it occurs as a symptom in cholera. It is a consequence also of certain renal changes which I have yet to consider. For this reason I shall defer its further consideration until we have had those renal changes before us; and I turn to the opposite condition of the kidney, in respect of its peculiar function—that, I mean, in which its secretion is largely and morbidly *augmented*—a condition which is sometimes as surely though not so rapidly fatal. When the amount of urine secreted and passed is permanently too great, when it is constantly running off, as it were, from the system, the patient is commonly said to have *diabetes*: from *διαβαινω*, to pass through.

But it is not every case of an excessive flow of urine that deserves to be called diabetes. Great quantities of aqueous urine are passed by hysterical and nervous patients. We all make more water in cold weather than in warm; the functions of the skin and of the kidney compensating each the occasional defect of the other. Certain drugs and articles of diet are also well known to cause a temporary excess in the amount of urine secreted.

In fact, although the quantity of urine voided is the most obvious and striking symptom of diabetes, its definite and characteristic symptom is a most remarkable change in the *quality* of that liquid: in its becoming loaded with sugar. You will find, indeed, two species of diabetes mentioned by many authors —the diabetes *insipidus*, and the diabetes *mellitus*. The former term ought in my opinion to be abolished. If it refer merely to an unnatural abundance of urine, not otherwise differing in its composition from healthy urine than in containing a large proportion of water--by calling such a state diabetes we link together in the same genus two essentially different conditions. It is said that there is no human urine that does not contain some trace of sugar. This alleged trace we may practically ignore. In true diabetes sugar may always be detected in the urine by ordinary tests: in small quantity it may not reveal itself to that coarse test, the *taste*; but modern observers almost all agree in rejecting any species of diabetes in which the urine is not demonstrably saccharine. This condition of the urine was first described in 1684, by our learned countryman, Dr. Willis.

The sensible qualities of diabetic urine differ strikingly, in many particulars, from those of the urine in health. Its chemical quality differs strikingly too, as I have already told you; but it is in one particular only. Fortunately no extraordinary skill is required to recognise the morbid secretion.

Diabetic urine is commonly light coloured, and transparent; of a pale straw, or greenish tint. Being so copious, it rarely exhibits any visible lithates. Its odour is peculiar. According to Dr. Prout the scent somewhat resembles that of sweet hay, or that of milk; but to my nose it is more like the faint smell of certain apples, or rather of an apple chamber. Its taste is, more or less decidedly, sweet. Notwithstanding its limpid and aqueous appearance, diabetic urine is remarkably heavy. When boiled in a test tube with an equal quantity of liquor potassæ it assumes a claret colour of a greater or less depth, according to the quantity of sugar present. This is called *Moore's* test. Its delicacy may be increased, Dr. Garrod says, by *first* adding a drop or two of liquor potassæ to the urine, so as to make it slightly alkaline, and then decolorising it by the further addition of animal charcoal, and by filtration through paper.

Another familiar test is that of *Trommer.* In a large test tube mix with some of the suspected urine just enough of a solution of sulphate of copper to give it a faint blue tint. Then add liquor potassæ in considerable excess. If sugar be present, a precipitate of hydrated oxide of copper first falls, which is redissolved in the excess of alkali, forming a dark blue solution. If this be gently heated to ebullition, a dense deposit of red sub-oxide of copper takes place.

Dr. Pavy recommends an extemporaneous cupro-potassic test, formed by pounding together five grains of sulphate of copper and ten grains of neutral tartrate of potash, and dissolving in two drachms of liquor potassæ. A clear deep blue liquid results, which answers quite as well for the detection of sugar as any other form of copper solution.

Yeast, again, furnishes a ready and an easy test of saccharine urine. Invert a test tube filled with urine to which a small quantity of yeast has been added, into a saucer also containing urine. In a warm room, fermentation, if sugar be present, will soon commence, and carbonic acid rising in the tube will depress the upper surface of the urine.

What are called *torulæ*, minute vegetable confervoid growths, very soon appear in diabetic urine when it is freely exposed to the air in a warm place. This sugar fungus, as it is also named, has in fact been ascertained to be identical with the yeast plant; and during its growth (and probably in consequence of its growth) the urine undergoes the alcoholic fermentation, bubbles of carbonic acid gas are evolved, and a vinous odour arises. The presence of torulæ in the urine has therefore been regarded as good evidence of the presence of sugar also. But this is not a safe inference. Other confervoid forms, requiring an instructed eye to distinguish them from the sugar fungus, do often accompany it: and these are constantly developed in non-saccharine urine also, provided that it be acid, and that it contain albumen, or some other animal matter.

These other confervoid forms constitute stages in the growth of the fungus called *penicilium glaucum*, which is the same that so often gives to decaying animal or vegetable substances a mildewed or mouldy appearance. It has been recently asserted that the yeast plant, the *torula cerevisiæ*, is really identical with this penicilium glaucum. Dr. Arthur Hassall, however, in a very interesting paper on the subject, contained in the 'Medico-

Chirurgical Transactions,' affirms that these two fungi are readily distinguishable, the one from the other, under the microscope, and he gives drawings of both, in the various stages of their development up to their perfect fructification.

However this may be, if you observe torulæ in the urine of a patient, do not, unless you are expert microscopists, conclude at once that he is labouring under diabetes; but taking the hint which they furnish, employ other tests to determine whether his urine be really saccharine or not.

It was long believed that the quantity of urea in diabetic urine was reduced much below the natural standard; and that the sugar was somehow formed at the expense of the urea. Dr. Prout, in his earlier researches, always detected a little, and but a little, of this peculiar principle. Later observations have shown, however, that the urea is not so scanty; nay, that it is generally as abundant as in the urine of health, and sometimes even more so. The presence of the sugar conceals the urea; interferes with the action of the ordinary tests of that substance. By certain modes of procedure, which I need not stop to describe, the urea may readily be discovered: and it is often found, I say, to be rather excessive than deficient. The usual saline matters belonging to healthy urine are present also in that of diabetic persons; and in the same *relative* proportions; but, as might be expected, their *absolute* amount, in a given quantity of the liquid, is very much diminished. In short, the only essential deviation from the standard chemical constitution of the urine is, that it holds in solution a quantity of sugar. This explains its peculiar odour, its sweetness, and perhaps its excessive quantity. It accounts also for another very characteristic property of diabetic urine; I mean its high specific gravity. In general, you know, the specific gravity of the urine is inversely proportional to the quantity secreted in a given time; the more copious and dilute it is, the lighter it is. But in diabetes, so strong is the saccharine impregnation that the specific gravity more than keeps pace with the increased quantity of the liquid secreted. The specific gravity of diabetic urine is always much higher than that of healthy urine.

The *quantity* of urine secreted and voided is sometimes enormous; far more than could be supplied by the quantity of fluid taken as drink, although that, as I shall presently explain, is excessive too. A healthy person passes from one to three or four pints of urine in the twenty-four hours. The quantity, as you well know, is liable to considerable variation: perhaps the average may be safely laid at about fifty ounces. But patients in diabetes will void 50 *pints* in the same time. I have myself known 26; 13 or 14 are not uncommon; and cases are recorded by writers of credit and veracity, in which 70 pints were passed daily. Nay, one Italian author declares that 200 pints have been discharged in that time.

The saccharine matter thus held in solution may be obtained in its solid form by evaporating the urine. I have seen large flat cakes of beautifully crystallised diabetic sugar. It differs somewhat from common sugar, the produce of the sugar-cane; and approaches more nearly to the sugar of grapes. This kind of sugar, which may also be produced artificially from starch, chemists have named *glucose.* By rapid evaporation of the water a thick syrup is procured, resembling treacle; but Dr. MacIntyre, who has presented to our hospital-museum some very perfect specimens of this sugar, prepared by Mr. Blandford, informs me that to get it well crystallised, the evaporation in a steam bath should be stopped while the urine is still of thin consistence. It may quickly be reduced to one-half, perhaps, of its original quantity. Then it should be set aside, in shallow plates; and in the course of ten days or a fortnight the sugar will be deposited in an irregularly crystalline mass.

The sugar is sometimes so abundant, that it undergoes a rude crystallisation as the urine dries, wherever it happens to fall. A girl who was in St. Bartholomew's Hospital while I was a student there, observed that if her water were accidentally sprinkled upon her black stuff shoes, every drop left a white powdery spot behind it. So also an aged patient under Dr. MacIntyre's care expressed to him her alarm at finding that her black worsted stockings were sticky and covered with a white dust, from the same cause. A man recently under my charge in the Hospital, complained that two pairs of his black cloth trousers had been spoiled in a similar manner. I remember hearing from a diabetic patient in the Edinburgh Infirmary, that his attention was first drawn to his urine by the number of flies and wasps which its sweetness attracted to the chamber-pot. In India the red ants have been observed to swarm in the same way about a vessel containing diabetic urine.

This daily production of sugar from the laboratory of the human body, is surely a very singular and surprising phenomenon. Sugar is not a constituent of healthy urine. Dr. Prout (whose experience on this subject was very large) says in his book that he had never known saccharine matter to occur in the urine of any other animal than man. I once had a coach-horse which I supposed might have diabetes. He was a greedy feeder, and drank eagerly, yet he grew thinner and thinner; and at whatever door I had occasion to stop, there he invariably began to stale: so that I became thoroughly ashamed of his leaking. Dr. Prout was good enough to examine his urine for me. It contained no sugar, but its healthy properties were much changed: it had less than the natural quantity of hippuric acid, and more of earthy matters. The disease, he told me, is known at the Veterinary College; whence specimens of such urine had been sent to him for inspection: but it is not true diabetes.

The unnaturally high specific gravity of diabetic urine is a constant quality; and you must attend to this, for it is almost always a faithful index, not only of the presence, but of the severity of the disorder. Dr. Prout places the specific gravity of healthy urine between 1015 and 1025, that of distilled water being represented by 1000. Different authors vary somewhat in their estimate of the natural standard; but we may be content to follow Dr. Prout. He says that the specific gravity of diabetic urine has been stated to vary from 1020 to 1050: that he has many times seen it higher than this, but very seldom so low. In fact it ranges generally between 1030 and 1060; and the average may be taken at 1040.

So much with reference to the quantity and qualities of the urine discharged in this complaint. It is attended, however, in well marked cases, with other and important symptoms, as you may suppose.

So much fluid being evacuated from the body through this channel, it might be expected that the *other* channels for the excretion of liquid matters would be comparatively dry: and so they are. The skin is arid, harsh, and unperspirable. The patients tell you that they never sweat: that they cannot get into a perspiration. This is a very general symptom: yet in some few patients, especially as the fatal period of the complaint draws near, the surface readily becomes humid. And a friend and patient of mine, whose urine was very copious, and contained a notable amount of sugar for several years together, without any perceptible abatement, however, of his general good health, perspired profusely every night. Again, the bowels are mostly costive, and the fæces remarkably solid and free from moisture. The tongue is dry, parched, and sticky; sometimes unnaturally red and clean: and the waste of watery particles from the system seems to be felt and expressed by the inordinate thirst which the patients suffer. Their drought is often insatiable. I remember one girl's telling me that when she was debarred from an excess of water to drink, she would get up if she heard it raining in the night, and catch some of the descending drops to satisfy the tormenting sensation of thirst. And another patient, a very sensible fellow, informed me that, believing it could not be good for him to drink so much, and feeling no confidence in his own resolution to refrain, he was in the habit of betaking himself in the summer time to the fields and dry pastures, where no water was at hand to quench his strong desire for it. The appetite for food is often, but not always, equally keen: and the patients, especially those in the lower ranks of society, are apt to think, while wondering at their loss of strength, that there cannot be much the matter with them, since they continue to eat and drink so famously.

Again, the enormous daily drain upon the system may be expected to cause various symptoms and sensations which may all be referred to weakness and defective nutrition. A table has been published by Dr. Henry, showing the quantity of solid extract in every wine pint of urine of different specific gravities from 1020 to 1050. Taking 1040 as the average specific gravity, and ten pints as the average quantity, of the urine discharged daily, the patient would in this manner lose, every twenty-four hours, 15 ounces 7 drachms—or more than a pound and a quarter—of solid materials. Dr. Garrod, after analysing the urine in several cases of diabetes, found the daily quantity of sugar excreted to vary from half a pound to a pound and three-quarters. It is most abundant three or four hours after a full meal, and least abundant when the secretion takes place at the time most remote from the influence of food.

We need not be surprised, then, at the hunger, the wasting, the hectic fever, the feeling of emptiness and sinking at the stomach, the debility, the chilly state of the body and especially of the extremities, the aching and sense of weariness in the loins and legs, the aversion to exercise, the loss of virility; all of which symptoms are generally present. I may add, to complete the picture, some others, enumerated by Dr. Watt, and confirmed by Dr. Prout, and consistent with my own experience of the disease. They are, uneasiness in the stomach after meals, flatulence and acid eructations, dimness of vision, redness of the whole interior of the mouth, sponginess of the gums, looseness of the teeth, and some degree of irritation and inflammatory redness about the external orifice of the urethra: these last are symptoms noticed in persons dying of inanition. Again, listlessness and depression of spirits, weakness and peevishness of temper: 'the once vigorous mind becomes feeble, oblivious, and vacillating—the once amiable temper, fretful, suspicious, and intolerant.' With all this there is a peculiar faint and unpleasant odour of the breath and person; an odour which Dr. Prout says is hay-like, which some call melleous, but which reminds me, as I said before respecting the urine, of the smell of a room in which apples have been kept. I have often recognised the complaint, upon first entering the sick chamber, by this peculiar scent.

Diabetes is generally a chronic disorder, creeping on at first insidiously, and spreading itself, under judicious management, over many years. Yet it is sometimes fairly entitled to be called an acute disease; for it occasionally breaks out suddenly, is attended with much febrile disturbance, and runs a short course, uncontrolled by any treatment. Two such instances I have seen, and others I know of. Much more frequently it proves fatal through the supervention of some organic mischief, such as debility is calculated to foster and develope. It often becomes associated, in its progress, with pulmonary disease, especially with tubercular phthisis. So common is this, that some persons have thought it universal. But it is not so. I have myself witnessed more than one or two dissections of persons dead of diabetes, whose lungs did not contain a single tubercle. Sometimes the disease terminates in incurable dropsy: and sometimes the patient is cut off suddenly, either by apoplexy, or by some peculiar disorder of the stomach.

There is some kind of connexion between diabetes and certain affections of the skin, and of the subjacent reticular membrane. Dr. Prout remarks that it usually *follows* cutaneous complaints, but *accompanies* or *precedes* those which involve the areolar tissue. Persons have been known to lose chronic eruptions, upon the supervention of diabetes. On the other hand, carbuncles and malignant boils are frequently the companions of that disorder; and are thought to be more apt to arise when the quantity of urine voided is much diminished. But carbuncles and boils are not necessarily accompanied (as Dr. Prout suspected they were) with saccharine urine. Itching of the skin is often present. Cataract is another not uncommon accompaniment of diabetes.

Dr. Garrod has found *gangrene* to be of frequent occurrence in diabetic persons. There can be no doubt that their vital power is always greatly lowered by the disease. They are apt to sink rapidly under any sudden mental shock, under bodily injuries, under surgical operations, and even under unusual fatigue or anxiety.

The same author remarks that upon the supervention of these secondary diseases the sugar frequently disappears from the urine; and cautions us that this disappearance, far from being an index of improvement in the patient's condition, is then really a warning of danger, and too often the harbinger of approaching dissolution.

The examination of the dead body throws little or no illustration upon the pathology of diabetes. We naturally look with interest to the kidneys: and we find there consequences indeed, but nothing to throw light upon the cause of the disease. Sugar, as Dr. Johnson tells us in one of his published lectures, is a substance which the kidneys are not able to eliminate from the blood in large quantity and for some time together without injury to their secreting cells, which become gradually less fitted for the secretion of healthy urine. According to his experience the epithelium of the kidney grows opaque, and sometimes contains a considerable amount of granular material and oil. Occasionally the urine becomes albuminous, and shows casts of the uriniferous tubes. In one instance, after sudden symptoms of gastritis, which followed an incautious potation of strong ale, I found the mucous membrane of the stomach distinctly inflamed, in its cardiac portion. I have seen also the mesenteric glands diseased, converted almost entirely into bone. But neither of these changes is constant. They were purely accidental in those particular cases. Suppression of urine is sometimes the immediate cause of death.

What, then, is the origin and source of this strange complaint? whereabouts in the body is the sugar formed? is it made, by the kidneys, from the blood? That was one of the earliest conjectures. It was naturally thought that, if the sugar pre-existed in the blood, and was only withdrawn from it by the kidneys, it would be discoverable in the blood. Yet able chemists sought for it there in vain. Hence it was inferred, that by some new combination of its elements, saccharine matter was actually formed in the kidneys. The chemistry and the reasoning were both faulty. Sugar has, now, been detected both in venous and in arterial diabetic blood. It is detected with some difficulty, partly perhaps because its presence is masked by the albumen of the serum, but partly because its quantity is small; and its quantity is small because it is continually decanted out of the blood, as fast as it enters, and with it a profusion of water also, through the kidneys. In this respect the sugar and the urea are alike. They are both excretions which the blood is in haste to cast forth. It would seem also as if the sugar necessarily carried with it a large quantity of aqueous fluid from the blood, and was simply diuretic. When the amount of sugar eliminated is diminished, as by certain remedies it may be, the quantity of urine diminishes too.

Traces of sugar had, I believe, been found in the blood by some previous inquirers: but it is to Mr. M'Gregor, of Glasgow, that we are indebted for the full exposition of this interesting fact. His researches on this subject were published in the year 1837, in the 'Medical Gazette.'

By a peculiar process, he did, I say, that which many preceding chemists had failed to accomplish: he detected sugar in the serum of the blood of diabetic patients. The serum had a milky appearance, he says; and I have seen that myself: its specific gravity was above the healthy standard. Having coagulated the serum by heat, he carefully dried it; then he cut the dried mass in very small pieces, and boiled them in distilled water; and lastly, he evaporated the decoction to a certain point. To the liquid thus concentrated he added a portion of yeast, and the presence of sugar was manifested by the fermentation which ensued, and which lasted for several hours. Yeast, I should have told you, is a very *delicate* test of sugar, and will readily detect half a grain in two ounces of liquid. Sugar in a crystalline state has since been obtained from the blood, and from various other fluids of the body; from the sweat, from the tears.

Mr. M'Gregor went a step further back. He obtained, by means of an emetic, the digested food from the stomachs of two men who had dined two or three hours before. One man was in health; the other had diabetes. In each case the food had been of the ordinary kind. Applying, after due preparation, the test of yeast, he found that the vomited matters fermented strongly; especially those from the diabetic patient.

Then he varied the experiment. Thinking that the sugar, in these cases, might have been introduced in the vegetable portion of the food, he adopted precautions to exclude that possible source of fallacy. He administered to a healthy man, and to a diabetic man, a vomit and a purge; to clear out the alimentary canal. Next he fed them upon roast beef and water, and nothing else, for three days. Then, three or four hours after a meal, the contents of their stomachs were procured by the operation of the sulphate of zinc, as an emetic; and treated as in the former case. What the healthy man vomited did not ferment at all. What came from the diabetic patient fermented 'pretty briskly.'

The inference from these facts seemed direct and inevitable that the fault lay in the *digestive* organs; that instead of perfect and nutritive chyle, saccharine matter was formed by the stomach, and entered the circulation unaltered. In healthy digestion all amylaceous food is first converted, the chemists tell us, into glucose, which then undergoes further changes. In diabetes these further changes were somehow prevented. The food which should have been transmuted into muscle, and fat, and bone, and nerve, and membrane, was hurried out of the system, as sugar, with the urine.

This plausible theory was disturbed by the surprising announcement made by M. Claude Bernard, that within all healthy persons, whatever may be their food—nay, within all animals, whether herbivorous, carnivorous, or omnivorous—a manufacture (so to speak) of sugar was constantly going on. According to his statements the apparatus for this manufacture is planted in the liver, in the tissue of which sugar may always be readily detected, while every other organ of the body is destitute of it. No sugar can be found in the blood of the portal vein: plenty of it in that of the hepatic veins, even when the food (as in experiments made upon dogs, and young owls) has been exclusively animal for months beforehand. The sugar is traceable onwards in that part of the vena cava inferior which lies between the entrance of the hepatic veins and the right auricle, in the right chambers of the heart, and in the pulmonary arteries. The rest of the blood, during the intervals between successive periods of digestion, is devoid of sugar. While digestion is in full process, the blood which has passed through the lungs contains a slight impregnation of saccharine matter, not enough, however, to reach, in any appreciable amount, the urine: so that the sugar formed in the liver, disappears, according to M. Bernard, in the lungs.

Pushing his inquiries still further, M. Bernard satisfied himself that, by a vital process, a substance analogous to vegetable starch is formed in the livers of all animals; and that, by a chemical process, this substance is convertible into dextrine and sugar. He found also that sugar continued to be produced in the livers of healthy animals, for some little time after their sudden death.

Another most singular discovery of Dr. Bernard's, connected also with the subject before us, is, that *artificial* diabetes may be produced at will in an animal by irritating (by means of a puncture, or of a slight galvanic shock) a certain limited part of the medulla oblongata: namely, a spot in the floor of the fourth ventricle of the brain, close to the origin of the pneumogastric nerves. Within an hour, or in even less time, the urine of the subject of the experiment becomes strongly saccharine; and it continues saccharine for a whole day, or more; until (probably) the slight injury done to the nervous substance is repaired.

The whole subject has since been minutely reinvestigated by Dr. Pavy, and with very remarkable and unexpected results; a summary of which is all that I can attempt to bring before you.

Dr. Pavy verified the facts reported by M. Bernard as having been observed in his experiments; but found reason to dissent from his conclusions.

He satisfied himself in the first place that there is *not*, 'as a natural process of life, that flow of sugar from the liver into the circulating blood' which M. Bernard thought he had demonstrated. Immediately after sudden death indeed, while organic life alone lingers in the animal frame—and in certain unnatural states during life—there is a large escape of sugar from the liver. But in life and health a trace only of sugar is detectible in the blood between the liver and the lungs; just as much as, and no more than, in blood taken from any other part of the circulating apparatus. Further, during life and health a mere trace alone exists of sugar in the substance of the liver itself. Dr. Pavy recognises the presence in the cells of the liver of the amyloid substance called by M. Bernard glucogen.

The current theory of certain forms of diabetes seems to be this. There is formed in the liver an amyloid substance, allied to dextrine, and exceedingly prone to undergo metamorphosis into sugar. This chemical tendency is held in check during life and health by some nervous influence, exercised probably upon and through the blood. When that nervous influence is withdrawn by disease or injury of the nervous tissues, or by mental emotions, or by sudden death—or intercepted by certain diseased conditions of the liver—the amyloid substance is rapidly converted into sugar, which enters the blood, and (life going on) begins at once to be eliminated through the kidneys.

Certain forms, then, of diabetes may plausibly be attributed to disorder or disease within the cranium. And clinical experience tells in favour of this explanation. Some striking instances have been published by Dr. Goolden in which head symptoms were accompanied by saccharine urine, and in which the diabetic symptoms were checked or removed by remedies addressed to the head affection; by blisters especially, and by purgatives. Dr. Pavy records a case in which 'an intense degree of diabetes immediately followed a violent blow upon the head.' The sufferer was a cadet at Sandhurst about 20 years of age. Bernard mentions a quarry-man who became diabetic after a fall upon his head. The sugar disappeared from his urine as recovery from the injury to the head took place. And this is what, if the theory be sound, we might expect; and must govern the prognosis in any given case. The same may be said of the occurrence of saccharine urine during the progress of certain blood-diseases; and after the inhalation of chloroform or of æther.

Again, sugar, and sugar-forming matters enter largely into our natural food. The sugar passes readily from the alimentary canal by mere imbibition into the blood vessels, and so, like other foods, as fats, and albumen, is expended ultimately, by transformation and assimilation, in the growth, development, and maintenance of the animal tissues.

But the scope of the assimilation of sugar is not unlimited. Swallowed by a healthy person in excess of a certain quantity, it reappears in the urine. And the same thing happens when sugar is not taken in excess, but in consequence of disease, or the decay of age, or in some other way, the ordinary and healthy power of assimilating sugar becomes impaired, or diminished. And thus we have a class of cases in which diabetes may originate, as Mr. M'Gregor supposed it to originate, in some fault of the digestive organs.

We know but little about the outward *causes* of diabetes. It is not a very common disorder; and in those who become afflicted with it, there probably has existed a predisposition to it. Dr. Prout remarks that the complaint runs occasionally in families, and is inherited. I had under my own observation, for some time, three children, two brothers and their sister, all affected with diabetes. The mother, a maternal uncle, and a sister of a friend of mine, all died of this malady. The daughter of another friend, himself the grandson of an eminent physician, died of diabetes. He became a widower, married again, and at length incurred and fell a victim himself to the disease; and one of his daughters from the second marriage is at present diabetic. The same author mentions among the predisposing causes, long-continued intemperance, and especially the immoderate use of spirits, severe evacuations, excessive labour joined with a poor acescent diet. Distress and anxiety of mind are held also, and justly I think, to be among the predisposing causes. It sometimes seems to be produced, at once, by the operation of some exciting cause, such as exposure of the body to cold; or the drinking of large draughts of cold fluid while the drinker was hot and perspiring. Sir James Bardsley states that, in twelve instances of the disease which had fallen under his own notice, the patients attributed their ailment to one or the other of these two causes. Now these are common causes of disease; and that a predisposition does exist is probable from the fact, that where the exciting cause has acted on several individuals at the same time, one alone has become affected with diabetes. There is a narrative illustrating this, by Sir Henry Marsh, in the third volume of the 'Dublin Hospital Reports.' A patient of his traced the apparent origin of his diabetes to exposure to wet, cold, and privation, at sea, while in imminent danger of shipwreck. Another of the crew fell ill of ague. Others escaped entirely, or had only common colds.

Diabetes is a malady which justly alarms those who are the subjects of it. But though too often a fatal malady, it is not necessarily so: and the older I grow, the less despondence do I feel upon first ascertaining that a patient is voiding saccharine urine. Whether it be that the disease is really more common, and at the same time milder and more tractable than it formerly was—or whether (what seems more likely) it is more carefully looked for now-a-days, and more easily recognised, certain it is that I both see, and hear of in the practice of others, many more instances of diabetes than I did some years ago. Of the well-marked cases, a few—not many—have recovered perfectly, and so far as I know permanently, even to the return of the urine to its natural specific gravity. Others have gone on, by slow or by rapid steps, to a fatal termination. In not a few, the main symptom of the complaint, I mean a considerable impregnation of the urine with sugar, has continued for months and years, without material deterioration of the general health, until the patient has been cut off by some other illness. It would seem that when the digestive organs are capable of properly assimilating a sufficient quantity of food to sustain the bodily fabric, other portions of the aliment may run off in the form of sugar with comparative impunity to the health. But even this, the best state in which a person who remains diabetic can be, is a perilous state.

The modern views of which I have been telling you, respecting the pathology of diabetes, tend to explain these differences. No age is exempt from liability to the disorder. In 1860, I saw, in consultation with Dr. Garrod, the youngest diabetic patient that I have myself met with; a boy between 3 and 4 years old. His disease ran its fatal course in about 5 months. The younger the patient is, the worse, according to my experience, is the prognosis. If the complaint be traceable to a temporary cause, we may naturally hope and expect, as I have already said, that it also will be temporary. Generally the older the patient, the less anxiety need be felt about the issue of the disease. In most of such cases the source of the sugar in the urine is to be found in deficient power of the digestive organs to assimilate that substance.

I must caution you against a premature conclusion that your patient is well. Apparent recoveries—nay, apparent cures—are not very uncommon. And this it is of great importance to know. Remedies are not useless because they fall short of their full scope. It is better to keep a man on the edge of a precipice, if you cannot pluck him away from it, than to let him fall over. And many diabetic patients are kept in this predicament of dangerous safety.

There are certain remedies that exercise a strong controlling influence over some of the most prominent and troublesome of the symptoms; and that sometimes even restore the patient to a state which he mistakes for health; and which a medical man, unwarned of its fallacious character, might also mistake. The urine may recede within its natural limits. There may remain *one* morbid circumstance only, and that of a nature easily overlooked: indeed it is sure to escape observation if it be not especially searched after. I allude to the unnaturally high *specific gravity* of the urine. So long as the density of the urine continues permanently and decidedly above the healthy standard, there is no real security. The smallest disturbing cause—exposure to cold, an intemperate meal, unusual exertion and fatigue, sudden or strong mental emotion —may bring back all the symptoms in their former severity.

If these and similar hurtful agencies can be averted, life may sometimes be prolonged, in much comfort, for many years.

In an interesting paper, which you may see in the 36th volume of the 'Medico-Chirurgical Transactions,' Dr. Bence Jones calls attention to cases of *intermitting* diabetes, of which he relates several. The peculiarity of such cases is, that without obvious cause the sugar disappears from the urine at intervals; and sometimes just before these intervals occur, the urine is found to be full of free uric acid, and of crystals of oxalate of lime. He notices also, and partly confirms the observation of Deschambre, that sugar is frequently present in the urine of old people without producing very urgent symptoms: sometimes, indeed, with none of those symptoms which would naturally suggest a search for it.

If in a diabetic patient we can trace any local condition upon which the diabetes may possibly depend, such as disease or injury of the brain or of the liver, we must attempt, by such means as may seem appropriate, the redress of that morbid condition. The main object of treatment, however, must always be the prevention or the restriction of the discharge of sugar through the kidneys.

About the beginning of the present century, Dr. Rollo discovered and taught that a diet composed exclusively of animal matters had a signal effect in reducing the quantity and in diminishing the sweetness of diabetic urine. And the reason of this is obvious. Animal food furnishes but scantily the materials for the formation of sugar. 'The saccharine alimentary principles are chiefly derived (says Dr. Prout) from the vegetable kingdom, and indeed constitute what may be called, by way of distinction, *vegetable aliments*.' If, then, we exclude aliments of this kind, and confine the patient to animal food alone, we thus cut off at least one supply of the *materies morbi*; and, without indeed curing the disorder, suspend its worst effects. But unluckily very few persons can long endure this mode of living. So far as they can endure it, they are comparatively safe. We are obliged to relax a little, sometimes, the rigour of our rule; and it is curious to observe how suddenly and decidedly the saccharine properties, and the quantity, of the urine are augmented, when, by stealth, or by permission, the patient adds to his meal the smallest portion of vegetable food—even a biscuit or two.

I quite agree with Dr. Prout and Dr. Garrod in believing that the regulation of the diet constitutes by far the most important part of the treatment. If care be taken to exclude all articles of food that contain saccharine matter, or that are readily convertible into saccharine matter, the condition of the patient amends at once. His thirst abates, his appetite becomes more natural, the state of his tongue and of his skin improves, and his strength and weight augment. In one example, recorded by Dr. Garrod, the daily amount of urine was reduced within a few days, by regimen alone, from 354 to 100 fluid ounces, and the daily quantity of sugar voided with it from 26 ounces avoirdupois to rather less than 4.

In another instance the quantity of urine, under the ordinary diet of the hospital, averaged 267 fluid ounces daily. The patient being then put upon meat diet, the bran-loaf, cod's liver oil, and laudanum, the urine sank in quantity, in five days, to 91½ fluid ounces. And it rose again to 216 ounces upon the resumption by the patient of the ordinary diet.

In both these cases the general symptoms underwent a proportional amendment, as the amount of urine became less.

We must contrive then to vary the animal diet as much as we can; encouraging the patient by a free license to choose among the different kinds of meat, game, poultry, fish, and eggs, in their diversified modes of preparation. A refinement of accuracy, which can scarcely ever be needful, would except from the allowed meat dietary the *livers* of all animals. Of edible vegetables, those which under the influence of sunlight have become wholly or in part *green*, and in so becoming have lost all or much of the saccharine matter they previously contained, may contribute with safety to the bill of fare of most diabetic patients; the oleracea therefore, spinach, cabbage, water-cress, lettuce, asparagus, and the like. Should, however, the absolute exclusion of sugar be rigidly demanded, the whiter portions of these green vegetables must be eschewed. So, of course, must all those vegetables and vegetable matters which contain a notable proportion of sugar or of starch; potatoes therefore especially, rice, chestnuts, all farinaceous food, such as arrow-root, sago, tapioca, macaroni, vermicelli; carrots again, parsnips, turnips, beet-root, onions, peas, beans, rhubarb. Some of these, however, such as turnips, broccoli, Brussels-sprouts, and so forth, may, in the milder cases of the disorder, be permitted; only they should be first well boiled in a large quantity of water, whereby much of the sugar they contain is removed. All kinds of fruit must be forbidden. You will seldom be able to debar your patients entirely from bread: none should be allowed but such as is well fermented, and somewhat stale, and thoroughly toasted; and even that as sparingly as may be.

Various attempts have been made to provide some substitute for ordinary bread. The bran-loaf just mentioned is one; or still better bran-*cakes*. But they require to be very carefully made. You may find Mr. Camplin's directions for making them in the 38th volume of the 'Medico-Chirurgical Transactions,' in a paper by that gentleman upon the juvantia and lædentia in diabetes, suggested by experience of the disorder in his own person. *Gluten*-bread, invented by M. Bouchardat, is another of these substitutes. Dr. Pavy has suggested the use of *almond* flour, when divested of its slight amount of sugar and gluten, as a material for making loaves and cakes, and for other culinary purposes. It is best when some of its oil also is removed by pressure: otherwise, for ordinary stomachs, it forms too rich a food to be eaten instead of bread. It is well to know where, in this town, and for the present, these articles of diet may be obtained. Bouchardat's gluten bread, in the forms of slices and rolls, is imported by Van Abbott, of Princes Street, Cavendish Square. Bouthron, in Regent Street, also makes gluten biscuits and bread, well spoken of by Dr. Pavy. Dr. Camplin's bran-food, and almond biscuits also, may be procured at Blatchford's, in Oxford Street.

The strange proposal has been made, by a foreign physician, of giving sugar and honey, in large quantities, to patients suffering under diabetes, with the avowed purpose of supplying to the system the unnatural loss of sugar through the kidneys. This plan has been tried, and has utterly failed, in this country. As might have been foreseen, the quantity of sugar eliminated with the urine is greatly increased. Now the true object of treatment is to keep down the amount of sugar passing through the kidneys, and tending, besides being hurtful in other ways, to spoil their secreting cells.

It is of some importance to admonish the patient, whose appetite is often ravenous, against eating too large a quantity, even of animal food, at any one time. Not only is the digestion still further weakened and oppressed by an intemperate meal, but the patient's life may be put in peril by every such act of unwise indulgence. Of this I have witnessed one example, and have heard of several more.

Dr. Pavy is doubtless right in advising that no limit should be imposed upon a diabetic patient in respect of the quantity of his drink. The thirst is often, as I have said, most painfully intense, and no good, but most probably harm, would ensue from forbidding the relief of appeasing it. The water that is lost with the urine requires to be, if possible, replaced. Only take care that no element of sugar is contained in the liquids permitted. Milk, though it is not without some saccharine principle, may generally be allowed in moderate quantity. The same may be said of unadulterated Bass's Ale. If some stimulus be desirable, weak brandy and water, or whisky and water, may be taken. Claret, as it contains no sugar, is an excellent and agreeable beverage for diabetic persons. They need not abstain from unsweetened tea or coffee. The water of the Bristol Hotwell, containing carbonate of lime in solution, is praised by Dr. Prout for its efficacy in quenching the excessive thirst. I have found (acting upon a suggestion of the elder Dr. Latham's) a like good effect from distilled water acidulated with phosphoric acid. Dr. Pavy speaks well of the alkaline carbonates; from ten to twenty grains of the bicarbonate of potash, with aromatic spirit of ammonia.

Numerous indeed are the remedies that have been tried, and that have been recommended, for this disorder; but very few of them have obtained a lasting reputation.

There is one remedial measure which certainly, in some cases at least, has a most beneficial influence on the condition and feelings of the patient; I mean forced perspiration—perspiration induced by the hot-air bath. Of this I have seen some striking examples. A very well-marked case of diabetes came under my care in the Middlesex Hospital several years ago. A vapour, or hot-air, bath had just then been constructed in the hospital, and I thought it a good opportunity for trying whether the suspended functions of the skin might not be restored, and the extravagant action of the kidneys perhaps corrected, by that powerful mode of exciting perspiration. I should tell you that other plans of treatment had already been put in force, with but partial advantage. It would occupy more time than I can now spare to enter upon the details of this case; but I will read to you the man's own statement, which he wrote down before he left the hospital, in evidence of the benefit he derived from the *sudatorium*.

'The urine' (these are his words) 'is reduced more than one-half, and does not contain much sweetness, but sometimes tastes salt, with a mixture of bitter. My stools, which were dry, and like balls packed together, are now quite natural. The pains in my limbs are entirely removed. My spirits, which were very much depressed, are now revived and cheerful. The unpleasant aching of my kidneys, of which I spoke little lest I should be cupped in the loins, is now removed, only I feel weak there. I am cured of the pain in my stomach, and the circuitous working of the wind in my bowels, which formed lumps in my belly as it passed, resembling those formed by the cramp. I have likewise got rid of the palpitation at my breast, which was accompanied with a sort of dread. My breathing is much improved; perspiration in a great measure restored; and my skin, which was dry, is now become moist. I sleep well at night, whereas I *could* not sleep more than two or three hours out of the twenty-four. My thirst, which was excessive, has ceased to be troublesome.'

This man, who, in the statement I have just read, has so graphically described his own morbid sensations, and the relief from them which he had obtained, left the hospital thinking himself well: *but* the specific gravity of his urine remained above 1030.

In about half a year afterwards, he went one evening to Hyde Park to see some fireworks, got wet feet, and began to cough. The diabetic symptoms returned more severely than ever; and he soon died. I found his lungs stuffed with tubercles.

In furtherance of the principle upon which the use of the hot bath is recommended, the other well-known methods of promoting the natural functions of the skin should be followed; friction, and more especially warm clothing.

Opium is a treasure to us in this disorder. It quiets the nervous irritability of the patient, allays many of his most distressing sensations, and restrains in a remarkable degree the excessive profluvium from the kidneys. But you must not suppose, from observing these favourable changes, that you are curing the disease by it. It seems to control the diuretic influence of the sugar in the blood, but it does not banish the sugar itself. Such at least is the witness of my own experience. Dr. Pavy has however expressed of late his belief that opium exerts a direct remedial effect upon the actual disease. Diabetic patients bear large doses of opium uncommonly well. I had the opportunity of seeing one of Dr. Pavy's patients who was cured, apparently, by opium alone; while she was using, without any restraint, her ordinary diet. The opium was given in gradually increasing doses till she took nine grains daily. At length the sugar disappeared permanently from her urine.

She belonged, however, to the class in which the disease is mostly found to be the least serious, and the most tractable—for she was sixty years old. In younger persons, whose cases are generally more formidable, he recommends—and I think the recommendation a good one—that the sugar should first be reduced as much as possible by a regulated system of diet, and that opium should then be given with the hope of removing it altogether.

Steel is sometimes singularly beneficial in repairing the strength, and enlivening the spirits; as indeed it is well known to be in other forms of disease attended with a copious and permanent drain upon the system, and with a diminution of red blood. Of course it may be combined with opium or with any other medicine which the circumstances of the patient may render needful.

I scarcely need say that the bowels require attention. Not that active purgation is advisable, but simply their regulation. Castor-oil, rhubarb, aloes, lenitive electuary, are more appropriate in these cases than the purgative salts, which are apt to be diuretic also.

For many years of my professional life I had not met with an instance of what has been termed diabetes *insipidus*: from which I infer the rarity of that disorder. I shall use the term *chronic diuresis* to express this affection.

At length a marked instance of such chronic diuresis presented itself in the hospital. A boy, eleven years old, not unhealthy looking, but lean, was admitted there under my care. He was much troubled by thirst; and by frequent micturition, which even in the night disturbed him many times. His bowels were costive, he had a capricious appetite, and his skin was dry. He voided during the twenty-four hours several pints, seldom less than nine or ten, of simply dilute urine, of a faintly yellowish hue, and having the specific gravity of 1002. Sometimes, indeed, it was found to be scarcely heavier than distilled water. In other respects he appeared tolerably well. It was supposed that he had been affected in this way for about twelve months; his desire for drink having been the first symptom noticed.

During his residence, of many weeks, in the hospital, under my observation, I made trial of every plan and drug that I could think of, for repressing the unnatural flux of urine. Nothing did him any good: some things, I fear, by disturbing his stomach and bowels, did him some temporary harm. He went out much as he came in.

In December 1865 I saw, for the only time, in consultation, a child 4½ years old, about whom I learned the following particulars. In the preceding July it was noticed that her health was failing; in August extreme thirst came on; in September her cervical glands and a gland in her left axilla began to swell. Her mouth was dry and sticky, the end of her nose was red and itching, there was no moisture on her skin, her bowels were confined, and she had but little appetite. She drank largely and eagerly of water, and passed sometimes even more than a proportionate quantity of urine, in which no trace of sugar or of albumen could be detected, and of which the specific gravity was generally about 1003. Her drink averaged about six pints daily, and I was assured that on some days she voided a pint more than she had swallowed. She was much emaciated. A brother and sister older than herself enjoyed perfect health; and there was no trace of consumptive disease in her parents' family on either side.

Some of these cases appear to depend upon excessive thirst, arising from an unhealthy state of the mucous membrane of the pharynx, and are apt to end in phthisis. M. Becquerel has applied to the disorder the title of *polydipsia*. I have mentioned my hospital example because it afforded me a proof that liquid may be absorbed into the body from the atmosphere; either by the external skin, or by the pulmonary mucous membrane, or by both these surfaces. Dr. Prout, to whom I showed both the urine and the patient, advised that, for a time, his supply of drink should be limited. Accordingly, very much to the poor boy's sorrow, he was put upon a daily allowance of a pint and half. I have no doubt that my injunctions were strictly observed, both by himself and by the nurses. Nevertheless, without losing flesh or weight, he passed, during the corresponding twenty-four hours, ten pints and a half of urine.

I got evidence of the same fact in another way also; namely, by weighing the boy at short intervals: although the experiment was not repeated so often as I wish it had been. I give you the results of one of these trials.

Immediately after he had emptied his bladder, he was found to weigh 3st. 8lb. 0oz. 3dr. Three hours subsequently, having taken nothing in the interim, he weighed 3st. 9lb. 0oz. 2dr. Then he voided 16oz. of urine: after which his weight was again 3st. 8lb. 0oz. 3dr. So that he must have imbibed about a pound of liquid in that brief space of time.*

In this instance there was merely an excess in the aqueous ingredient of the urine: the solid matters were apparently there, in their due proportion to each other, but in a very small ratio to the water.

But the aqueous ingredient may be in excess, while the absolute quantity of *urea* is deficient. On the other hand, with an excess of the watery material, there may be an excess also in the quantity of urea it contains.

Dr. Willis has distinguished these three varieties of chronic diuresis by the terms *hydruria*, *anazoturia*, and *azoturia* respectively. Although I am no friend to the multiplication of technical names, I must tell you that the distinctions expressed by these terms are real, and of some importance. Anazoturia we shall find to be often symptomatic of a peculiar organic disease of the kidney, which I hope to describe in the next lecture.

Azoturia, which is accompanied by an unnaturally high specific gravity of the urine, is apt, on that account chiefly, to be mistaken for diabetes. As recoveries from it are not uncommon, it may be suspected that some of the boasted cures of diabetes were cures of this less serious disorder. In the one case, the appropriate tests detect the presence of sugar in the superabundant urine; in the other case they find none.

For the most complete account that I have seen of this chronic diuresis, or *diabetes insipidus*, I must refer you to Dr. Roberts' book on 'Urinary and Renal Diseases.'

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* This boy lived more than two years afterwards, and continued to void an excessive quantity of pale, neutral urine, scarcely heavier than distilled water. After death, scrofulous tubercles were found in his brain, and in his lungs. His kidneys were gorged with venous blood, but of healthy structure. There was nothing wrong, apparently, in his organs of digestion.

LECTURE LXXII.

Albuminous Urine. Means of detecting the Albumen. What it imports. Anatomical characters of Bright's Kidney, Acute and Chronic. Symptoms. Nature of the disease.

Another morbid condition of the urine, imperatively demanding your attention, is that in which it is habitually impregnated with *albumen.* This albuminous condition is much more common, and in general not less serious, than the saccharine condition which I described yesterday.

There is no albumen in healthy urine. Neither can we recognise its presence, in any urine, by mere inspection. We detect it by certain tests: and I will tell you, in the first place, what these are; and how to use them.

Albumen—of which we have so familiar an example in the 'white' of eggs—begins to pass from the fluid to the solid state at the temperature of 160° Fahrenheit. When diluted it may require for its complete coagulation the heat of 212°. Hence one simple and easy test of its presence. We discover that albumen is contained in the urine, by heating that fluid to the boiling point. This is most conveniently done in a small glass tube, by the flame of a spirit-lamp. It is seldom that any preparation of the suspected urine is requisite. It may, perhaps, be hazy in consequence of its containing *mucus*; and if its transparency be much troubled, it will be well to filter the fluid before testing it. When, as sometimes happens, albuminous urine is already turbid from the presence of the lithates, these dissolve as the heat is applied, and the urine first becomes clear; and then, as the temperature continues to rise, the albuminous opacity begins to be visible.

The phenomena observable in the heated urine vary in different cases, chiefly by reason of the variable amount of albumen. The whole is sometimes converted into one gelatinous mass; but this is uncommon. Usually the albumen first appears in the form of a whitish cloud, of which the constituent particles multiply, and collect, in proportion as the quantity is considerable, into small curdy fragments or flakes. These soon subside to the lower part of the tube, leaving the supernatant liquor clear. The amount of albumen is of course to be estimated by the height to which, after such subsidence, the tube is filled by it.

Now this test, by heat, is not always conclusive, nor sufficient. There are circumstances that may impede or prevent its effect in coagulating albumen, which nevertheless is present. On the other hand it may, under other circumstances, produce a fallacious appearance of albumen where none exists.

Albuminous urine has often a less acid reaction with litmus paper than healthy urine. The reason of this I will explain presently. When recently discharged from the bladder the urine may be neutral or even alkaline; or it may become neutral or alkaline, by spontaneous decomposition after it has left the bladder. In any case, the urine thus alkaline or neutral will not coagulate when heated, even though it may be full of albumen. The alkalescence must be owing to the presence of ammonia, or of soda; but the compounds formed by albumen with these alkalies are neither of them coagulable by heat.

Again, although there may be no albumen, heat may cause a flaky precipitate, consisting of the earthy phosphates: or, less often, of the earthy carbonates. If of the latter, then effervescence will follow the addition of nitric acid.

We avoid, or we correct, these sources of fallacy, by testing the suspected urine with nitric acid also, which has the property of precipitating albumen in a flaky or pulpy form. It will thus detect albumen when the tested urine is alkaline. It has likewise the effect of redissolving the spurious precipitates which may be thrown down by the application of heat, and consequently of showing that they *are* spurious.

Nitric acid alone, however, is not, any more than heat alone, an unequivocal touchstone of the presence or absence of albumen: for if the lithates, or even if urea, be present in excess, a buff-coloured amorphous precipitate of lithic acid may be thrown down—or crystalline nitrate of urea may be formed—upon the addition of nitric acid, when there is no albumen. But this defect is compensated by the complementary criterion of heat; these precipitates being redissolved by raising the temperature of the urine, while any coagulated albumen remains insoluble.

And always this test, by nitric acid, requires to be applied with a certain degree of care and delicacy. The risk is, of being misled through using too little of the acid, or too much.

The compound resulting from the union of nitric acid with albumen—what may be roughly spoken of as the nitrate of albumen—is soluble in water, and is not coagulable by heat. If, therefore, just so much nitric acid (and no more) be mixed with the urine, as combines with all the albumen that it contains, no precipitate will take place. But when more is added, the nitrate of albumen, being insoluble in dilute nitric acid, becomes at once apparent in the fluid. Again, any great excess of nitric acid may redissolve the albumen.

Hence, in testing suspected urine, it is better to operate on a small quantity of it, a fluid drachm for instance, filtering it first if it be cloudy, then boiling it, and then dropping into it two drops of nitric acid: instead of nearly filling the test tube with urine, and adding a drop or two only of the acid, as is often done—or of adding as much acid as there is urine, which is perhaps a less common mistake.

A neater way of escaping the possible fallacy is this. Fill a test tube with the urine to the depth of an inch or thereabouts. Then incline the tube, and let nitric acid trickle down along its side to the bottom, and form a stratum about a quarter of an inch deep. Very little mingling of the two fluids will thus happen. If albumen be present three distinct strata will soon become apparent; the undermost consisting of colourless nitric acid, the uppermost of unaltered urine, and between them an opalescent layer of coagulated albumen.

Should the urine contain visible lithates which disappear under heat, and are succeeded by the white cloud, it is surely an albuminous cloud, and the nitric acid is not required. Under these circumstances, an iron spoon and a lighted candle comprise all the necessary test materials. When there are no lithates to be seen, the domestic vinegar cruet must also be appealed to, to ensure the acidity of the urine.

Take care always that your test tubes are *clean*; and do not contain, as they may do if merely just emptied, any trace of acid, or of alkali.

Furthermore, albuminous urine of excessive acidity, from its containing a free acid, may fail to exhibit albumen when heated. Either the acetic, or the hydrochloric, may be the preventing acid: in other words, the acetate and the hydrochlorate of albumen are both of them soluble in water, and uncoagulable by heat.

This source of error is also to be obviated by the addition of a small excess of nitric acid, after heating the urine: so that by observing the directions just given, you may avoid all risk of mistake.

Other chemical tests there are, frequently spoken of, and sometimes recommended: particularly the ferrocyanide of potassium, corrosive sublimate, oxalic acid, and creosote. They are unnecessary, in addition to heat and nitric acid; and they are liable to fallacies from which these last, when combined, are free. Unless you are expert chemists, you had better avoid them.

Dr. Bence Jones has suggested the following simple and ready trial, when chemical materials happen not to be at hand. Evaporate a drop of the suspected urine on a slip of glass over a water bath. When the evaporation of the water is completed, any albumen which may be present will adhere to the glass so firmly, that it will be no easy matter to clean it. The evaporation may be effected in a few seconds, in a watch glass, over a spirit lamp, care being taken to hinder the urine from boiling, by holding it far from the flame.

Now it is quite certain that the presence of albumen in the urine does often accompany and bespeak a very serious organic disease of the kidney. For this kind of disease we have no appropriate name. I wish we had. Some have called it *granular degeneration* of the kidney, but the epithet granular is not always applicable. It is most familiarly known, both here and abroad, as *Bright's kidney*, or *Bright's disease*; after the eminent physician who, in 1837, first described it, and showed its great pathological importance. These are odd-sounding and awkward terms; but in the lack of better I must employ them. *Albuminuria* is a common, and, with certain limitations, an admissible synonyme.

Since 1837 this subject of albuminuria has exercised the intellects of many able pathologists, who have found it not a little complex and difficult. All that I here undertake is to set before you certain broad facts which have now been fairly ascertained, and by the knowledge of which your diagnosis and your treatment of the disease may hereafter be guided. I should tell you that among the conflicting views which have been, and which are, entertained concerning the minute anatomical conditions of 'Bright's kidney,' I fully adopt those propounded and taught in this college, especially by Dr. George Johnson.

In the first place, then, the so-called Bright's disease has, by common consent, been resolved into two, if not more, distinct forms of disease, plainly and readily distinguishable each from the other, as well by the state of the kidneys visible after death, as by the symptoms observable during life. But before I describe these two morbid conditions, which are essentially chronic, it will be well to consider for a moment another renal condition, to which of late years the term 'acute Bright's disease' has been sometimes applied. It is a state of extreme sanguine congestion, next door to, or even constituting, acute nephritis; and it is apt to occur after exposure to cold and wet, or during the course of certain febrile disorders, and most particularly in scarlet fever. When death, under these circumstances, takes place early, as it may do, the kidney is found to be gorged with blood, which sometimes drips freely from it when it is cut open. The kidney is in general large, somewhat flabby, of a deep dark red, or sometimes of a bright red colour nearly uniformly diffused, except that the cut surface is usually diversified by still darker tuft-like spots, which have been ascertained to be Malpighian bodies, and convoluted tubes, turgid with blood. This change from the natural appearance of the kidney is evidently of a recent kind; and the symptoms that have been observed to belong to it are these:—Fever, preceded often by rigors; uneasiness or dull pain in the loins; nausea and vomiting; a very scanty secretion of urine, which is sometimes tinged with blood, has a high specific gravity, and is always albuminous; occasionally complete suppression of urine. To these symptoms there is presently added, in most cases, sudden and general anarsaca—what is commonly called inflammatory, active, or febrile dropsy. If the secretion of urine be entirely suspended, death soon ensues in the way of coma; and these I believe to have been the cases which were described by authors before the time of Bright, as cases of *suppression of urine.* Should there not be such suppression, the disorder frequently proves fatal through the supervention of some acute internal inflammation; pleurisy, or pericarditis, or pneumonia, or peritonitis. If the urine in this disease be examined through a microscope, it is found to contain a number of little hair-like threads or cylinders, which are, in fact, very slender fibrinous coagula, moulded in and discharged from the urinary tubules of the kidney. These cylinders or casts denote, even more distinctly than any amount of albumen in the urine denotes, extreme congestion, which may well be deemed inflammatory, in the affected kidney. They are commonly studded with minute epithelial cells, which have been detached from the surface of the tubules. Sometimes they contain blood disks also, and even specks of lithic or oxalic acid. Similar casts may be seen still remaining in many of the tubules. Other tubules appear to be filled with epithelial cells which have been shed from their walls. And similar cells are scattered separately through the urine.

These are the anatomical characters of what Dr. Johnson calls *acute desquamative nephritis.*

Such, then, is found to be the condition of the kidneys in fatal cases of this kind; and such we may safely infer, from observation of the symptoms and of the urine, to be their early condition, when life is not at once destroyed. And I must tell you that death in this form or stage of the renal disease is a comparatively rare event. In 20 instances only out of 292 cases observed or collected by Frerichs, were the kidneys in this first stage—the stage of inflammatory congestion. Most persons recover completely. Many seem to recover, but bear about with them the germs or beginnings of more chronic and latent changes which constitute one form of 'Bright's kidney.'

Cupping over the loins, hot baths, sudorific medicines, purgatives, and large warm enemata, are the kind of remedies to be employed in the treatment of this very formidable yet for the most part curable condition.

The coarse anatomical characters of the kidneys in the two more chronic forms of Bright's disease to which I have alluded, are in striking contrast with each other. They differ remarkably in size and in colour, and are spoken of accordingly as the *large white* and the *small red* kidney. The average weight of the adult human kidney is between four and five ounces. In Bright's disease some have been met with weighing twelve ounces, others weighing scarcely two. Both the increment and the decrement of the natural bulk belong principally, if not altogether, to the outer *secreting* portion of the gland. If a longitudinal section of the exaggerated kidney be made, its cortical part is seen to be unduly broad; and the same part is disproportionally narrow when the whole organ is smaller than natural. For this reason, in the latter case, the radiating medullary portions, or pyramids, approach nearer to the surface than they are observed to do in the healthy gland.

The large white kidney is generally of soft consistence, and has a smooth surface, which is apt to become indented by linear depressions, and so to assume, in its enlargement, a lobular shape. Its proper investing membrane is easily stripped off. The small contracted kidney, on the contrary, is hard and red; its surface is rough, as if strewed with prominent grains, and has sometimes a scarred appearance, and its outer membrane comes off with difficulty.

The cortical substance being the main seat of the morbid changes, the alteration of *colour* is most conspicuous in the cut surface of the large kidney, which has lost its red tint and its orderly aspect, and presents a pale, nearly homogeneous appearance; not unlike, in some cases, the section of a parsnip. Its natural striæ are confused or obliterated. The incised surface gives one the notion of some deposit whereby the original texture of the part is obscured. The blood-vessels, many or most of them, seem to have been emptied by compression or to be blocked up by yellowish solid matters; while the healthier, pink, pyramidal masses belonging to the medullary portion of the kidney appear to be displaced, or pushed aside, or encroached upon by the same yellowish matter, which sometimes interposes itself between and opens out their radiating tubules. Together with these changes of appearance, I have several times found the veins that emerge from the kidney firmly plugged up by coagula of blood.

The small red kidney is apt to be studded, both on its surface and throughout its interior, with numerous small cysts or cells, containing a thin transparent liquid. These cysts have been inaccurately termed hydatids. It is not at all uncommon to meet with one or two larger cysts of the same kind in this state of the diseased gland.

The symptoms that accompany these two conditions of the kidney are also very different. In both the urine is more or less deeply, and more or less permanently, charged with albumen. That which belongs to the contracted granular kidney is, as a rule, copious as to quantity, and in the advanced stages pale, of very low specific gravity; and it contains, as the microscope shows us, granular casts thrown off from its secreting tubes. The amount of albumen which it carries is comparatively small, and in some cases it is temporarily free from albumen; and the disease is seldom attended with dropsy, except, perhaps, to a slight extent, when the end draws near. On the other hand, the urine of the large white kidney is fuller of albumen, and scarcely ever free from it entirely; is apt to be scanty, has a higher specific gravity than the other, and usually contains clear, fibrinous, wax-like, and sometimes oily casts, with occasionally a little blood; while life is seldom or never destroyed without the previous supervention of general dropsy.

The urine in this form of the disease may retain for a while its transparency and its natural sherry colour, but in the advanced stages it becomes pale, and often more or less opalescent. It froths also more than usual. If you blow into it through a tube you raise bubbles like those which may be formed on soapy water, and the bubbles remain long unbroken. Very rarely does such urine deposit the lithates.

When the acute disease is abating, the urine has, even to the naked eye, an unnatural appearance. It is dark, dingy, obscurely turbid, like muddy beer: *smoky* is an epithet frequently applied to it; that is, it looks sometimes as if a very minute quantity of soot had given it a tinge. This hue depends upon the presence of a little of the colouring matter of the blood darkened by the acid properties of the urine. This smoky urine allows of a more favourable prognosis than the pale and opalescent, for it denotes a stage in the acuter form of the disease; which form, as I have said, not seldom ends in recovery.

Bearing in mind what I have now been saying, and reversing the order in which I have said it, you will be able to look through the symptoms, and to see behind them, in the living body, what kind of kidney they specially denote.

And we may push our researches a little further, and inquire what the microscope has to tell us of the minute pathological conditions of the affected kidney, in each case.

It tells us, then, that in the disease which issues in the contracted granular kidney the cells that line the uriniferous tubes undergo a disintegrating process, become detached from the basement membrane, and appear in the urine, as I have said, in the shape of granular tube-casts, such as are figured in the diagram before you, or such as you may see depicted in most of the text-books on urinary diseases. The tubes are thus gradually denuded; or are lined with small transparent cells, quite unlike the natural glandular epithelium. I believe it to be a mistake to suppose that hypertrophy of the fibrous tissue connecting the tubules is an essential feature of the disease. It is even questionable whether there is any such matrix, as it is called, or fibrous connecting and supporting tissue, distinct from the walls of the tubes and capillaries. Dr. Beale, a master of the microscope, has never been able to demonstrate more than a mere trace of such tissue. The very colour of the contracted granular kidney testifies to the absence of compression of blood-vessels by the alleged fibrous thickening. A corresponding error is prevalent, as I showed you not long ago, respecting what is called cirrhosis of the liver. The existence of a certain constitutional fibrous diathesis is, I conceive, a figment imported into this country from Germany, and unwarily adopted here. What has been deemed thickened fibrous tissue, I believe, with Dr. Johnson, to be mainly the residue of atrophied tubes and shrivelled capillaries. The cysts which I have told you are frequently seen in the small red kidney, are simply dilatations of portions of its denuded tubules.

Again, I adopt his doctrine that 'in the case of the large white kidney the gland cells do not become disintegrated, detached, and washed out, so as to leave the tubes denuded,' but remain for the most part adherent to the basement membrane, and undergo degenerative, sometimes fatty, changes; while the tubes become filled and clogged with an albuminous or fibrous material; the product, sometimes at least, of bygone nephritis.

Furthermore, I have to say of this large white kidney, that it appears to be thus the sequel sometimes of what I have described as 'acute Bright's disease,' or nephritis; sometimes of disease chronic and insidious from its very beginning. Sudden and alarming symptoms occasionally supervene upon this chronic and latent condition—when, I mean, the contents of the urine indicate an advanced stage of the disease; yet, there having been no previous occurrence of *dropsy*, the origin of the renal change cannot have been an attack of acute Bright's disease.

On the other hand, there is reason to believe that the small red kidney is never the result of acute disease, but always, and from the first, of gradual degeneration. It is one of the changes to which gouty persons are very liable, and it not seldom ends with cerebral hæmorrhage, or uræmic convulsions and coma. But this last is true of both forms.

Occasionally you may find the larger anæmic kidney hard, instead of being soft; hard from an admixture of waxy or lardaceous material. Occasionally its outer surface and its cut surface show a number of yellowish specks, which mark, in fact, a degree of fatty degeneration within the tubules. Similar yellow specks are much less frequently noticeable in the red contracted kidney.

There is also a pale fatty kidney, *sui generis*, spoken of sometimes as the mottled kidney, analogous to the fatty liver, and not necessarily accompanied by albuminous urine. The epithelial cells of the healthy kidney always contain, I am told, a minute quantity of oil; and the true fatty kidney is the result, apparently, of a morbid increase of this fatty matter. It is a curious fact, that cats—London cats, at any rate—are very subject to this fatty state of the kidney.

Which of the two main forms of Bright's kidney is the most common I cannot tell you; that which is marked by the presence of the large white gland is the most distressful, inasmuch as anasarca is, in many ways, both distressful and dangerous. 'It invariably kills the patient' (says Dr. Wilks), while the other form 'may be found where death is brought about by other means.'

Bright's disease, in its chronic forms, is apt to arise and to steal upon its victims insidiously; to be latent and unsuspected until something or other suggests an interrogation of the urine, which gives a terrifying reply.

It will be profitable to consider a little more closely certain conditions which either suggest the test just mentioned, or accompany and help to characterise the complaint.

No one omits now-a-days, when an anasarcous patient presents himself, to inquire into the qualities of his urine. But I postpone for a while the further consideration of the anasarca.

Whenever you learn from a patient that he has to rise two, three, or more times in the night to make water, I advise you to test his urine for albumen, especially if the urine be free from sediment. This frequency of micturition by night is a symptom in some other disorders; in enlargements of the prostate, for instance. But enlargement of the prostate is seldom present, except in the later periods of life. When there is stone in the bladder, micturition is *less* frequent in the night than during the day, for obvious reasons. I cannot tell you how very often this unnatural frequency of the desire to void urine in the night has led me to the discovery of albuminuria; nor am I able to explain the symptom.

A very common and important secondary complication is the occurrence of what are called *head symptoms*—various manifestations of derangement in the cerebral functions: headache, which is exceedingly common, misty vision and partial blindness, noises in the ears, delirium, drowsiness, epileptic seizures, coma without paralysis, apoplexy from cerebral hæmorrhage.

So far as a limited extent of statistical calculation can be trusted, convulsions and coma without palsy more often occur in connection with the large white kidney; cerebral hæmorrhage with the small red gland.

How are these head symptoms to be explained? They result, no doubt, like all the other intercurrent disorders, from a poisoned condition of the blood. Some have attributed these secondary accidents to the urea that is detained in the blood. Dr. Bence Jones, on the other hand, believes that urea is 'probably not much more poisonous than nitrate of potash.' Others ascribe the symptoms in question to the poisonous agency of the carbonate of ammonia, into which the urea, while still in the blood-vessels, is liable to be converted. This is the opinion of Frerichs, who declares that so long as the peculiar head symptoms continue the expired breath of the sick man is tainted with an ammoniacal or urinous odour, reddened litmus paper held before the mouth or nostrils turns blue, and a glass rod dipped in hydrochloric acid, and exposed to the issuing breath, is presently surrounded with a whitish cloud. When no ammonia can thus be detected the nervous symptoms cease.

I am not competent to pronounce a dogmatic opinion on these controverted points. You will bear in mind that the incidental symptoms of which I have been speaking are generally imputed (and I think rightly imputed) to *uræmic* poisoning.

They who are expert in the use of the ophthalmoscope describe a condition of the retina which, when it occurs, is said to be conclusive evidence of the existence of Bright's disease. They describe a broad, glistening, white mound, which encircles the optic disc, and certain stellate white spots in the region of the yellow spot; both owing to fatty degeneration of the cellular and connective-tissue elements of the retina. This is a sign which you must needs see, and learn to know, for yourselves. It is not constantly present. I mention it as furnishing a sufficient explanation of the dimness of vision which is often complained of by these patients.

A very remarkable fact in the history of Bright's disease is the occurrence, when that disease is well established, of hypertrophy of the left ventricle of the heart, without any valvular damage to account for it. Of course, valvular disease of the heart, productive of hypertrophy, may coexist with Bright's kidney; but I speak now of cases in which there is no such valvular disease. The fact did not escape Dr. Bright's notice. Among 100 cases recorded by him in a tabular form, there were 27 in which no affection of the heart could be detected. In

52 instances the heart presented the characters of hypertrophy, and of those no fewer than 34 were free from any trace of valvular disease. Among the 34 there were 11 cases of disease affecting the aorta; in the remaining 23 no cause for the existing hypertrophy and dilatation could be found in the heart itself, or in the great blood-vessels. Dr. Bright suggested that the altered qualities of the blood might 'so affect the minute and capillary circulation, as to render greater action necessary to force the blood through the distant subdivisions of the vascular system.' But to Dr. Johnson must be given the merit of having discovered the true and full cause of this hypertrophy of the left ventricle, in the fact that the muscular walls of the small arteries, not only in the kidneys, but also in most or all of the tissues of the body, in these cases of chronic Bright's disease, are greatly hypertrophied. The fact itself is demonstrable, and beyond question. This excessive growth of muscular tissue implies long continued over-action; and since the tonic contraction of the small arteries is known to oppose the passage of blood, the hypertrophy of the left ventricle is presumably due to the excessive resistance offered to the circulation by the excessive contraction of the minute arteries—this excessive contraction being the consequence of the irritant action of deteriorated blood.

Here again, then, we have, in cardiac hypertrophy without valvular or aortic mischief to explain it, a most suggestive symptom of the existence of Bright's disease, whereof the progress is so frequently insidious.

On the other hand, cardiac disease may, I conceive, produce this renal disease. Probably albuminuria may be referable in all cases to one of two causes; to some morbid quality of the blood itself, or to some condition that produces general venous congestion. The single fact that both the kidneys are always more or less affected in Bright's disease shows that the complaint falls primarily within the category of symmetrical, and therefore blood disorders; and Dr. George Robinson long ago proved experimentally that an obstruction to the return of blood through the renal veins gives rise to an albuminous condition of the urine. We may well believe, therefore, that abiding obstruction, the consequence of cardiac disease, may lay the foundation of permanent albuminuria.

Persons who carry about with them those states of the kidney which constitute Bright's disease are subject to obscure lumbar pains, though these seem rare; to nausea from time to time, and retching; to bronchitis; to inflammations of the serous membranes, most often of the pleura; to head affections, of which, often, they die; giddiness, drowsiness, convulsions, apoplexy. Many of them, nay most of them, become sooner or later more or less anasarcous. Gradually increasing pallor is almost constant; disease of the heart is common; not only hypertrophy of the left ventricle, which I have just now explained, but valvular disease also, the result of endocarditis provoked by the circulation of impure blood. The skin, in general, even in the absence of fever, is remarkably dry and unperspiring. The patients, as I have before told you, are troubled by a frequent want to make water, especially at night, when they are in the horizontal posture; by flatulence of the stomach and intestines; and by caprice of the bowels, which are sometimes obstinately costive, sometimes prone to diarrhœa.

Now it is worth your while to remark, with respect to this category of symptoms, that (nocturnal micturition excepted) they have no special *primâ facie* reference to renal disease. They are all common enough in various other complaints. In truth they are mere secondary consequences of Bright's disease; and in so far as they are symptoms of it, they are *indirect* symptoms. Before Dr. Bright no one perceived, in such symptoms, any indications of disease of the kidney. The primary and fundamental organic malady reveals itself by no direct signals excepting those which are furnished by the urine.

Seeing, then, that this structural disease of the kidney is coupled with effects so grave and perilous, and seeing that one of its most positive and distinctive marks is an albuminous state of the urine, two questions of great interest at once present themselves.

1. Does albuminous urine *always* imply the presence of Bright's disease?

2. Is Bright's disease, when present, *always* accompanied by albuminous urine?

To both these questions the answer is—no.

Certain articles of food, and some medicines, have the effect, in some persons, of rendering the urine for a time albuminous: perhaps it would be more correct to say that certain forms of indigestion may cause this change. Albumen has also been detected in the urine after a blister upon the skin, or under that general state of irritation of the surface, called *eczema rubrum*, which is produced occasionally by mercury. In the crisis of some febrile disorders, in some cases of pregnancy, of heart disease, and of delirium tremens, in gouty attacks, in diphtheria, and in epidemic cholera, the same phenomenon has been observed. Whenever blood, proceeding from any part of the long tract of mucous membrane which lines the urinary organs, mingles with the urine, that fluid of necessity contains albumen, and coagulates if tested by heat or by nitric acid.

On the other hand, in the red contracted condition of the kidney, the admixture of albumen with the urine is apt for a while to disappear; and even in the case of the large white kidney, I have known it vanish for several hours, immediately after the effectual application of a hot-air bath; and after profuse purging by a full dose of elaterium.

Another important question, therefore, now arises. Finding albumen in the urine, how are we to know whether it does, or does not, indicate the presence of Bright's kidney?

We may judge, in part, by frequently testing the urine, and noticing whether the albuminous impregnation be transitory or persistent. If, week after week, it remains steadily present, it is almost surely indicative of that renal disease. Partly, again, we form our judgment by the absolute amount of the albumen in a given measure of urine. If the water be deeply charged with that unnatural ingredient, the presumption is strong that the kidney disease is in progress; and as the disease proceeds, another remarkable change takes place in the urine. Its specific gravity diminishes, and thus is in striking contrast with that of diabetes. This is therefore a cogent additional diagnostic change. The decrease in the specific gravity is less marked in the large white kidney than in the small red kidney disease, in which it descends sometimes as low as 1004. And the colour of the urine, I repeat, becomes paler in the advanced stages.

I need scarcely again remind you, that the question of specific gravity must always be viewed in relation to the absolute quantity of urine secreted. The specific gravity depends, of course, upon the proportion of the solid constituents of the urine contained in a given quantity. If the aqueous portion be augmented, the effect upon the absolute density will be the same as if the solid contents were proportionally diminished. But when, as happens in this renal disease, the specific gravity decreases while the quantity of the urine decreases also, that conjunction of phenomena becomes especially significant.

The density of the urine being thus unnaturally low, notwithstanding the addition of the new substance, albumen, it follows, as a matter of inference, that the solid constituents proper to healthy urine must be sensibly diminished: and they are found, in fact, to be so. These solid ingredients consist mainly of urea, and of certain salts. The aggregate solid contents amount, in health, to sixty-seven or sixty-eight parts in every 1000. In Bright's disease the quantity has been ascertained to have sunk to twelve or fourteen parts; and even, in an extreme case, to less than this—to about six parts.

The urine contains, then, albumen; and it is deficient in urea. These two facts suggested, naturally enough, to M. Solon, and to others, the notion that the albumen might be formed, by a sort of conversion, at the expense of the urea; since these substances, by a slight alteration in the ratio of their elements, pass respectively each into the other. But it is not so. Dr. Christison had observed, many years ago, that when the urine was deprived of the greater part of its urea, the quantity of albumen contained in it was small; and, on the other hand, in cases where the urea was considerable in quantity, the albumen was plentiful also. In a more recent work on this subject, the same physician states that the whole of his subsequent experience has been in conformity with this observation.

It being certain, therefore, that the albumen is not vicarious of the urea, what (you may ask) becomes of the urea? It is detained in the blood; and may readily be recognised there in considerable quantity: and herein lies the secret of the secondary affections which belong to this disorder, and of its great fatality. The body is poisoned in detail by the retention of its own excrements. The blood not being duly purified through that great emunctory, the kidneys, is spoiled for its purpose of nutrition. Besides containing urea, it undergoes other and more manifest changes. Its proportion of fibrin varies; and it gradually becomes poor in colouring matter; the serum is less albuminous also, and of a lower specific gravity than in health. The quantity of albumen in healthy blood averages from sixty-five to sixty-nine parts in 1000. In this malady Dr. Babington has found it reduced to sixteen parts. The average specific gravity of healthy serum is 1028; but in Bright's disease it descends to 1024, 1020, and even to 1013. Now Dr. Christison has made out the very interesting fact, that there is a definite inverse ratio between the coagulability of the urine, and the density of the serum. The more albumen there is in the former of these fluids, the less is there in the latter, and the lower is its specific gravity. So that the deficiencies of the one fluid balance the superfluities of the other. All this is very different from what takes place in diabetes, in which sugar is excreted with urine that is otherwise healthy; whereas, in Bright's disease, urea, which ought to be discharged, remains in the blood; and albumen, which ought not to be separated, is taken from the blood and carried out with the urine.

The complaint happens at all ages: less often, however, in extreme youth than afterwards, except when it is a sequel of scarlatina. Sabbatier records that he saw, while in the service of M. Baudelocque, a young infant affected with anasarca and albuminous urine. The first case described by M. Solon is that of an infant, seventeen months old, in whom similar symptoms appeared shortly after exposure to cold and wet. In 1838 a boy between five and six years old, anasarcous, and passing bloody and albuminous urine, was in the Middlesex Hospital, under the charge of my colleague Dr. Wilson. M. Constant, in the 'Gazette Médicale' for 1835, cites the case of a child of five years of age; and M. Rayer gives two plates, representing the kidneys of two children, the one five and the other six years old, who both died of dropsy with albuminous urine, the sequel of scarlet fever. In each of these the changes described by Dr. Bright were well marked, and the bulk of the kidney was considerably increased.

The malady is, however, much more common in adults: not, in all probability, because the kidney is more readily susceptible of it at one period of life than another, but because, as life advances, the circumstances which tend to produce or to foster it become of more frequent operation; exposure to cold and wet, which is a very common exciting cause, and (perhaps) disease of the heart.

It occurs, I presume for the same reasons, oftener in men than in women.

Again, it is matter of common observation that intemperate habits have often preceded the development of this disease. Yet we may conclude that intemperance in drinking is rather a predisposing than an essential cause, from the fact that the malady is not unknown among children, and other persons whose manner of life has been strictly sober. I had lately an example of this in a young girl, fifteen years old, who had never menstruated. And this leads me to remark that the renal disorder has been known, in many instances, to follow a sudden check or suppression of the catamenia.

Dr. Christison suspects that Bright's kidney happens chiefly in persons of scrofulous habit; and he found it, in several instances, coincident with phthisis pulmonalis. My own experience would not have led me to that opinion. I partake in M. Solon's doubts, whether the co-existence of pulmonary consumption and of this renal malady is more than casual. Dr. Bright tells us that 'the instances in which phthisis, or any form of scrofulous or tubercular disease, has been connected with the renal affection, have been decidedly rare.'

There is yet something to be said respecting the *dropsy*, which is so common an accompaniment of these renal changes; but I must defer it till to-morrow.

Before we separate, however, let me recur for a moment to the subject of *Suppression of Urine*, which I simply mentioned, without dwelling upon it, at the commencement of the last lecture. I then told you that the *Ischuria Renalis* of authors was probably in all cases nothing more than a symptom, was not in itself a substantive disease, and that it was one consequence of certain renal changes which I had not at that time described. Those changes have now been explained. Total suppression of urine is apt to occur in the progress of acute Bright's disease: in other words, of acute nephritis. It is apt to occur, also, suddenly, at a certain stage in the chronic degenerative progress of the large white kidney. And as that progress is often insidious, and even, for long, quite unsuspected, we find in these facts a sufficient explanation, I think, of the cases, recorded by several authors, of sudden suppression of urine in the midst of apparent health.

If the symptom itself be, in these cases, within the reach of any remedies, it must be of such remedies as I have already recommended as proper in the disease of which it is a symptom: the abstraction of blood from the renal regions by cupping, hot baths and fomentations, sudorific drugs, and brisk purgatives. If the secretion of urine fail to be restored, fatal coma is not far distant.

LECTURE LXXIII.

Anasarca: its consideration resumed. Distinction of Chronic General Dropsy into cardiac and renal. Characters and signs of each of these varieties. Treatment.

We were yesterday occupied with the circumstances of that remarkable malady, which has never received a good, distinctive name, but which is sometimes called *Bright's* disease, after the distinguished physician who first recognised and described it, sometimes yellow or mottled degeneration of the kidney, sometimes granular degeneration. I endeavoured to represent to you, by words and by drawings, the coarser changes of structure which are visible by the unaided eye in the two principal forms of diseased kidney; and I stated some of the results of that minuter insight into its morbid conditions which the microscope has brought within our power. I spoke of the symptoms which appear *essential* to the malady, and which consist in certain striking changes in the urine and in the blood of the patient. I mentioned also the symptoms which are *incidental* to the renal disease. But of one of those incidental symptoms, or secondary consequences, I postponed the full consideration till to-day: I mean the *anasarca*, with which most commonly, yet by no means always or necessarily, it is complicated.

This is, for several reasons, a very important symptom. It is often the first thing that prompts us to suspect, and to inquire after, the renal malady. It was through his researches into the relation subsisting between chronic dropsy and the conditions of the urine, that Dr. Bright was led to the discovery of the associated affection of the kidneys. The dropsical accumulation adds greatly to the patient's distress, and sometimes constitutes nearly the whole of it. It adds proportionally to his danger. Moreover, it is that consequence of the renal disorder over which our curative measures have the most control. Indeed, under this complication we have, practically, to consider the remedies of the dropsy, distinct from the remedies of the renal change.

In most cases, at the outset at least of the dropsy, the skin is dry, and the urine is scanty: and the anasarca is observed to increase, or to decrease, as the quantity of urine diminishes or augments. The aqueous fluid, which should escape from the surface and through the kidneys, collects in the subcutaneous areolar tissue. As the disorder advances, the tendency to effusion of serum through the sides of the blood-vessels is probably increased, not only by the sluggish movement of the blood in the veins, from progressive debility of the heart, but also by one of the causes of that debility, the thin and watery condition of the blood itself; a condition which I yesterday pointed out to you as one of the most uniform and striking effects of the primary disease.

And here I again take up the subject of anasarca and general dropsy. You will remember that, in an earlier part of the course, I entered somewhat fully into the general pathology of dropsies. At the same time I promised you that I would afterwards, and when you were better prepared to understand them, endeavour more fully to explain some grand distinctions which have been found to exist between different forms of general dropsy. Having now, at last, brought before you all the organic changes which are apt to give rise to anasarca, I am in a position to redeem that promise. In doing so I shall probably have to remind you of some things which you have already heard from me.

Anasarca, you will please to recollect, signifies the filling up of a considerable part, or of the whole, of the subcutaneous areolar tissue, with serous or watery fluid: and when to this is added a collection of liquid in the large serous cavities also, we call the complaint *general dropsy*.

It is obvious that this condition may exist, and in nature it does exist, in various degrees: from slight infiltration of the areolar tissue, scarcely noticeable until, after some hours passed in the upright posture, it accumulates in visible œdema about the ankles—to the other extreme, in which the integuments are everywhere stretched to the utmost, even to bursting; the insteps bulging upwards; the legs and thighs enormously enlarged, cylindrical, unshapely, and exhibiting partial vesications; the surface of the trunk of the body capable of being kneaded and moulded like dough; the skin of the penis distended, and in consequence of its confinement by the frænum, twisted and circumvolved so as materially to impede the outward passage of the urine; the scrotum, as big as a child's head, preventing the miserable patient from bringing his thighs together, and from lying upon either side; the hands swollen; the face and neck bloated. With all this, the peritoneum is generally full of liquid, and at length each pleura; and as the scene is about to close, there is water in the ventricles of the brain, or an anasarcous pia mater.

Now from whatever cause this watery condition of the whole body may arise, the effects resulting *from the presence of the water* are the same. And of what do patients in this state usually complain? Why, of shortness of breath, and palpitation of the heart; of a sense of impending suffocation if they attempt to lie down, or to bestir themselves actively; of tightness and distress across the epigastrium, relieved somewhat by eructation, augmented by food and drink; of weight and stiffness of their limbs; and, sometimes, of drowsiness.

The explanation of all this is easy and obvious. The shortness of breath may be accounted for on various grounds: by œdema of the lungs themselves, a state that is revealed to us through auscultation; by water in the pleuræ; by the pressure upwards of the diaphragm, which embarrasses still more the labouring heart and lungs; and this upward pressure is increased by any kind of distention or repletion of the stomach, diminished when the stomach is collapsed and when the upright position is maintained. The heaviness and want of pliability of the unwieldy limbs are, like the rest of these phenomena, purely mechanical. All parts are oppressed by the unnatural load of water.

But we must look beyond the dropsy: and inquire whether the complaint has set in suddenly, and simultaneously with febrile disturbance, invading all the districts of the body at once, and quickly reaching its present degree; or whether it has crept upon the patient slowly and by stealthy steps: whether it has had any obvious or probable exciting cause; or whether it has approached insidiously we know not whence or why: whether (in a word) the case be one of active and febrile dropsy, or of chronic and passive.

Of the acute or febrile form of general dropsy, I have said all that I have to say. Its chronic forms may be arranged in two great classes: those which depend upon disease or debility of the *heart* composing one class, those which depend upon disease of the *kidney* constituting the other. To these classes we accordingly apply the terms *cardiac dropsy*, and *renal dropsy*. They are often combined in the same individual; but taking the pure cases of each form, we may proceed to inquire into their peculiar features, how they may be distinguished, and what differences of treatment they may require.

And first of cardiac dropsy.

The mode in which disease of the heart may occasion general dropsy has been sufficiently explained already. We infer that the dropsy, in a given case, has this origin, if we find that thoracic symptoms, such as cough and dyspnœa, preceded the dropsy: or if we perceive direct signs of cardiac disease, such as distended jugular veins, irregular movements of the heart, unnatural impulse, altered sounds: or if we trace the history of some previous acute disease affecting especially the left side of the thorax: or if we learn that the patient has formerly suffered acute rheumatism: or if the patient's age be so much advanced as to make it probable that some of those organic changes in the heart and large blood-vessels are in progress, which are almost natural in the decline of life. And our inference is confirmed if there be no discoverable indication of renal disease.

But we see many persons who labour unequivocally under organic disease of the heart, yet who survive, even for many years, without becoming dropsical. The interesting question therefore arises, of what kind of heart disease is dropsy a consequence and symptom? You already know the answer. It is such disease as offers a certain amount of permanent obstruction to the passage of the venous blood. Hence dropsy is especially associated with dilatation of the right chambers of the heart. It would not be correct to say that the anasarca is *dependent* on such dilatation, for the dilatation itself is at once an effect and a sign of impeded transmission of blood from the right side of the organ. Nor is such dilatation a necessary attendant on the general accumulation of water. The impediment may be sufficient to gorge the right cavities, while it is yet too slight in amount, or too recent in duration, to have dilated them.

What, then, are the physical conditions which oppose to the blood in the veins such an impediment as we are now considering? The two great vital organs contained within the thorax, the heart namely and the lungs, form different parts of one common mechanism, the object of which is to supply every tissue of the body with blood that has recently been purified by exposure to the air: and these organs, thus closely related in their functions, are moreover so reciprocally dependent, that structural disease occurring in the one, tends to produce disease, sooner or later, in the other also.

And I wish you again to observe the order and direction in which disease is, almost always, propagated from one part of this apparatus to another. It is a backward direction—opposite, I mean, to the course of the blood. There are, strictly speaking, two hearts, which lie side by side, in respect of their anatomical position, but which, reference being made to their vascular intercommunication, are really separated from each other by the lungs. The great veins precede, and the great arteries follow, this chain of connected organs. Disease occurring in any one part of the chain becomes a cause of consecutive disease in the part immediately behind it. And this law obtains, as I have shown you before, in regard to the several chambers of the heart, considered as a single organ.

Thus, structural disease situated at the aortic outlet of the heart, and of such a kind as to hinder the exit of the blood from the left ventricle, gives rise to permanent changes in that ventricle; to hypertrophy with, or less commonly without, dilatation. The hypertrophy is strictly a compensating and conservative change; and when it is exactly proportioned to, and keeps pace with, the impediment which has given it birth, so as precisely to balance and countervail it, no delay takes place in the stream of arterial blood, and the injury is, as yet, confined to the left ventricle. That chamber is remodelled, and adapted to its purpose by the *vis medicatrix naturæ*; and no other evil manifests itself than, perhaps, some slight encroachment and pressure upon the neighbouring parts, in consequence of the augmented volume of the heart.

So long as the mitral valve remains healthy and effective, it offers a barrier of protection against the extension of the disease in the direction which is retrograde to the course of the blood. But at length, in most instances, the stress becomes sensible further back. The left auricle and the pulmonary veins become choked and distended; the blood is detained in the lungs. Then commences *dyspnœa*: at first occasional only, whenever the heart is tasked with the conveyance of a greater quantity of blood in a given time than usual, as in brisk movements of the body, or sudden emotions of the mind; or when it is oppressed by circumstances that diminish the capacity of the chest; by a full meal therefore, by flatulent distention of the stomach and intestines, by the recumbent posture. Afterwards the shortness of breath becomes more or less constant and distressing.

Now this loaded and embarrassed state of the lungs, even when it is permanent and has reached a considerable degree, may exist without materially interfering with the functions of the right or venous heart; for the pulmonary plethora may be relieved by increased secretion from the bronchial mucous membrane. Dyspnœa, even when it has become habitual, may precede for some time any appearance of dropsy.

At last, however, the effects of the original evil augmenting and extending, the right ventricle also becomes unable duly to propel its contents into the pulmonary vessels; it continues morbidly full, is first distended occasionally, then permanently, and at length really dilated; and with that dilatation we have a turgid venous system, of which we see a part in the prominent veins of the neck.

In this way, then, may be explained a series of symptoms which you will often witness, and be consulted about, in persons who are growing old. You will find irregularity of the pulse; preternatural impulse perhaps of the heart; occasional shortness of breath; large crepitation, habitually audible in the lower and hind portions of the lungs, and especially of the left lung; more or less expectoration, sometimes tinged, sometimes even loaded, with blood. Eventually the ankles begin to swell; and the patient (if his life be not cut short earlier in some other way) becomes by degrees decidedly and universally dropsical.

Many of the direct signs of diseased heart may exist, therefore, while there is no anasarca: intermissions and irregularity in its movements, palpitation, the impulse proper to hypertrophy. But when dropsy has supervened, we may expect those signs also which denote dilatation of the right chambers. The heart is heard and felt to beat beyond the præcordial limits; the pulsations become feeble and unequal, if they were not so before; the patient is liable to fluttering palpitations, to extreme and panting dyspnœa on the slightest exertion, even on taking food into the stomach, or adopting the recumbent posture; his skin assumes a dusky hue, and his lips and extremities are apt to be livid.

In these cases the anasarca first becomes manifest about the ankles. During the earlier stages the œdema disappears in the night, and returns towards the next evening. It is sometimes confined, for a long while, to the legs; but ultimately it creeps up towards the trunk of the body; the thighs enlarge, the loins and flanks become doughy, the scrotum fills, and water collects in the serous bags of the abdomen and thorax. In extreme cases the dropsy is universal, pervading the areolar tissue of the head and face and upper limbs.

As the accumulation of serous liquid is commonly gradual, the reticular tissue, partly perhaps through maceration, but chiefly from continued pressure and stretching, loses its elasticity; and the œdema is soft, and *pits* readily.

Sometimes, the fluid continuing to increase, the cuticle is raised by it, and large vesications take place on the limbs; or some part of the areolar tissue sloughs, and a breach of surface is made, and the fluid drains off by this vent in great abundance, to the signal relief of the patient. This beneficial accident furnishes us with a valuable practical hint.

Such, then, is the consummation of disease commencing in the left heart, and working its gradual way, through the lungs, to the right heart. But the obstacle may originate at a less distant link in the chain. The circulation may be checked, first of all, in the intermediate lungs: and it may be worth our while to consider, for a moment, the relations which subsist between general dropsy, and certain *pulmonary* diseases.

Whenever, in pneumonia, a large portion of one or of both lungs becomes impervious to air and to blood—or when pleurisy fills one side of the thorax with liquid, which, by its pressure, shuts out both air and blood at once from one half of the respiratory apparatus—the egress of the blood from the right heart, and, therefore, from the venous system, cannot but be checked. We might expect that dropsical effusion would be the result of these changes; and in truth it does sometimes occur. That it does not happen more frequently is to be attributed, I believe, to the free evacuations and the strict abstinence, which are early put in force in those complaints, and which relieve the venous plethora before it produces effusion.

So, again, lungs that are hollowed out into large cavities, or rendered solid over a wide space by numerous tubercles, are manifestly incapable of admitting into their vessels from the right ventricle the ordinary quantity of venous blood. In these cases, however, the whole mass of blood is diminished, and kept within the limit which does not imply distention of the veins, by the constant agency of various causes: by the imperfect nutrition consequent upon abdominal disease; by the sometimes copious expectoration; by the wasting diarrhœa; by the profuse nocturnal sweats. Accordingly anasarca is an unusual symptom in pulmonary phthisis, or shows itself in the latter periods only of the disease, in the form of œdema of the legs: and its occurrence then is mainly owing to the debility which affects, in common with the other muscular parts of the body, the moving organ of the blood.

The pulmonary disease which more commonly and certainly than any other, though often very slowly, leads to dropsy, is *emphysema* of the lungs. I showed you, some time ago, that in this morbid condition many of the smaller blood-vessels of the lung become gradually obliterated; and when the disease is extensive and advanced, large portions of the organ are visibly white and bloodless. Meanwhile, the nutrition of the body is not impaired; the same quantity of blood continues to be returned towards the heart, but it finds not a ready entrance into the pulmonary blood-vessels, when delivered from the right ventricle. A certain amount of accumulation becomes habitual in that chamber, and in the great veins; at length the capillary vessels feel the mechanical congestion, and more or less anasarca ensues.

So much for dropsy that is purely cardiac. Let us next consider the circumstances from which, during the lifetime of the patient, we draw the conclusion that the dropsy he exhibits is of renal origin.

There is not much, that I know of, which is very peculiar or distinctive in the characters of the anasarca itself. This however is observable;—that whereas in cardiac dropsy the anasarcous swelling begins in the lower extremities, in renal dropsy it is often noticed first in the face and in the upper extremities; in the eyelids, in the cheeks, and upon the backs of the hands. These you will remark are uncovered, and therefore visible parts. I believe that an equal amount of swelling would at the same time be detected, were it looked for, in the feet and ankles. Resulting more than the cardiac variety from an unnatural state of the blood, the watery effusion takes place more suddenly and more universally; from the vessels of every region of the body at once. Cardiac dropsy arises from mere mechanical delay of the blood—and that delay is felt most at the greatest distance from the heart, and in the most depending parts of the body. This early anasarca of the face and hands is the more marked in proportion as the renal disorder is recent and acute; or when, it being chronic, local inflammation, or febrile disturbance, is suddenly superadded. According to my experience it is less noticeable when the dropsy comes on slowly and insensibly during the progress of chronic degeneration of the kidney. It may be said also of this renal form of dropsy, that accumulation in the larger serous cavities is not, in general, a prominent feature.

If we find, upon due scrutiny, no material or adequate embarrassment of the respiratory functions, no deviation from the natural sounds of the heart, no derangement of its regular movements, no alteration in the force of its pulsations, or in the space over which they can be felt and heard, no distention of the large veins of the neck—then we have strong reason for suspecting that the anasarca is connected with some vice of the kidney.

But we cannot infer, from the *presence* of heart-symptoms, that the kidney is free from disease.

Our judgment is guided, or assisted, in some degree, by the *complexion* of the patient. When general dropsy depends upon disease of the heart, the cheeks and lips are occasionally florid, often purplish or livid, frequently dusky and loaded. Sometimes (as in chlorotic women, where the heart may be temporarily distended without any positive organic disease, and the blood is thin and poor) the face and mucous membranes are pale: but in the renal variety of dropsy there is a very characteristic hue; an evident lack of red blood, indeed, in the capillaries, but withal an unhealthy dingy sallowness, significant, to a practised eye, of some deep-seated alteration of structure.

Our suspicion that the kidney is the organ primarily in fault is strengthened, if we trace certain accidents in the history of our patient. An attack, for example, of illness, attended, perhaps, with temporary swelling of the body and disturbance of the urinary functions (acute dropsy, in short), soon after some exposure, under unfavourable circumstances, to the influence of cold; either applied to the external surface, or to the stomach by a draught of cold drink. Or, a similar though transient anasarcous condition, which came on during convalescence from scarlet fever. For, as I told you yesterday, there is reason to believe that in *acute* dropsy is often laid the foundation of some of those peculiar changes in the kidney which, since they were first pointed out by Dr. Bright, have been chiefly studied in their connexion with *chronic* dropsy. That as rheumatic carditis may occur, and become latent as to its effects for some time, and yet implant the germs of future cardiac dropsy, so the stress or congestion which befalls the kidney in cases of febrile anasarca, may set on foot a morbid process that long works silently and unobserved, but at last declares its operation by symptoms; the reproduction of the dropsy in a more chronic form being the most significant symptom of all. The acute attack may have been forgotten;

there may have been no obvious (though there may have been ill-understood) indications of the renal affection; and its existence has been, therefore, unsuspected.

The discovery of intemperate habits would also be of importance in aid of our diagnosis, if these same habits had not a like influence in causing disease of other organs as well as of the kidney, and especially of the heart. There may, however, be no such episodes as these in the patient's history. The dropsy may have come on immediately after some exposure, or obviously injurious influence, yet not with acute symptoms, and in a temperate subject: in which case it is probable that the renal disease had pre-existed in a latent state. Or the anasarca may have stolen on by degrees, without any apparent cause.

But the most conclusive evidence of the renal disease is to be found in the conditions of the urine; which were fully explained to you in yesterday's lecture.

I have been speaking of dropsies that are purely cardiac, and of dropsies that are purely renal. But I have already told you that disease of the heart and disease of the kidney frequently go together; and I have endeavoured to estimate their relation, in such cases, to each other. When both organs are structurally affected, the disposition to dropsical accumulation must evidently be augmented. What share they have, respectively, in producing the dropsy, it would be very difficult, and practically it is not very important, to determine.

In chronic general dropsy of a purely cardiac origin, the kidneys, being sound, offer the most convenient and eligible channel for carrying off the superfluous water. Diuretic medicines, therefore, rank among the most important of our curative expedients. When they fail to act, or prove insufficient for the purpose sought, we next have recourse, the state of the bowels permitting, to drastic or hydragogue purgatives.

Diuretics are notoriously of most uncertain operation; sometimes completely answering our wishes, oftener perhaps disappointing them altogether. Something may depend upon the way in which they are administered. There can be no doubt that liquids, after being conveyed into the stomach and intestines, pass thence into the blood by imbibition through the capillary vessels—nor any doubt that living membranes are subject equally with dead membranes to the physical laws of endosmose and exosmose. Hence it follows that remedies which are meant to reach the kidneys must be in a liquid form when taken, or must be capable of being dissolved afterwards in the fluids of the alimentary canal. In the last edition of his volume on 'Urinary Deposits,' Dr. Golding Bird lays down the further requisite condition, that the density of the solution must be considerably below that of the liquor sanguinis, or of the serum of the blood; lower, that is, than 1028. The proportion of solids (he says) dissolved in aqueous vehicles should always be less, when the purpose is diuresis, than 5 per cent.; otherwise that purpose is sure to be defeated: strong solutions of saline substances proving purgative through the exosmosis which they cause out of the blood—and weak solutions diuretic through the endosmosis which they cause into the blood. Dr. Headland, however, in his essay on the 'Action of Medicines'—while he admits that a weak solution is more likely to pass off by the kidneys and a dense one by the bowels—questions, indeed I may say disproves, Dr. Bird's explanation of these facts. He shows that salines are in all cases absorbed into the blood, and that whether they are subsequently excreted through the kidneys or by the bowels, depends more upon the *quantity* administered than upon its degree of dilution. In fact the kidneys are not able to eliminate more than a certain amount of these saline medicines, which, to obtain the desired diuretic effect, should therefore be given in small doses, moderately diluted. The operation of diuretics is apt to be foiled when the bowels are irritable or lax. So likewise any impediment to the free entrance of liquids into the tributaries of the vena portæ from disease or congestion of the liver, which keeps those veins full—any mechanical hindrance to the subsequent course of the same liquids towards the emulgent arteries, from disease of the lungs or of the heart, producing general venous congestion—will tend to baffle the aim of drugs which are esteemed diuretic.

When the urine is strongly acid, and deposits, on cooling, a sediment like brick-dust, it may be well to try, at first, the alkaline diuretics, and particularly salts of potass. Nitre added to the common saline draught; or a combination of the acetate and bicarbonate of potass; or the bitartrate in small doses; or the iodide of potassium; or the liquor potassæ. In my own experience the tincture of squills also has seemed to correct this superacid and turbid condition of the urine, while it increased its quantity.

The benzoate of ammonia is another salt which I have found to operate very powerfully, in several instances, as a diuretic.

Digitalis sometimes promotes, in a remarkable degree, the flow of urine. Small quantities of the tincture, or of the infusion, may be added to other formulæ. Or the powdered leaves may be combined in pills. But one of the best modes of exhibiting digitalis for this purpose is to give larger doses of the infusion, half an ounce, for example, in some cordial water, at intervals of four or six hours, till three doses have been taken in succession; and then to pause and note its effects; and to repeat the three doses, or not, accordingly.

The spirit of nitrous æther, and the compound spirits of juniper and of horse-radish, have all of them well-marked diuretic properties, and may, with propriety, be added to most of the liquid formulæ for augmenting the discharge of urine. And, as vehicles for more active, or more concentrated ingredients, those vegetable infusions or decoctions should be chosen which are reputed to possess similar virtues; such as the decoction of broom-tops, or of juniper-berries, or of winter-green, or the infusion of buchu.

Squills, turpentine, the tincture of cantharides, are drugs of a more stimulant nature, more peculiarly adapted to cases in which there is no febrile disturbance, and the kidneys are obstinately inactive.

Sometimes a combination or farrago of diuretic substances proves more efficacious than larger doses of any of the ingredients administered singly: and the operation of some of these combinations is undoubtedly quickened and exalted, in many instances, by the addition of mercury. A fluid drachm of the officinal solution of the bichloride in each dose of a mixture; or small quantities of calomel, or of blue pill, when the medicines are given in a solid form. A very useful pill of this kind, much recommended by the late Dr. Baillie, consists of from one to four grains of the pilula hydrargyri, mixed up with one grain of the dried powder of squills, and half a grain of the dried leaves of digitalis; to be given twice or thrice a day. Dr. Baillie states that squills and digitalis are much less effectual by themselves, than when combined with mercury: which operates probably in relief of the portal circulation, by promoting a free secretion from the liver.

In choosing purgative drugs to aid the effect of diuretics in carrying off the dropsical fluid, or to take their place when these fail to act, we select those which produce copious and watery discharges from the bowels. A combination of jalap and cream of tartar has been long and deservedly esteemed for its excellent operation in this way. Gamboge is also a good cathartic. It may be given two or three times daily, in grain or two-grain doses, with a drachm of cream of tartar, suspended in two ounces of peppermint water. Or half an ounce of cream of tartar, mixed in six ounces of peppermint water, may be administered in one dose every morning. Croton oil, and elaterium, are still more powerful evacuants of serous liquid from the intestines. One or two drops of the former, or from a quarter of a grain to a grain of the latter, will be about a proper dose. It is astonishing how much relief to the feelings of the patient, and how great a diminution of the dropsical symptoms, are sometimes obtained by these active cathartics. Patients will earnestly beg for a repetition of them, even when their operation is attended, for the time, with considerable pain or sickness, and much general distress.

In addition to these measures for the removal of the collected water, attention must be paid to the actual condition of the heart. If the dropsy have been the result of anæmia, or of cachexy of the system, you must endeavour to strengthen your patient, and to repair his impoverished blood, by nutritious food and tonic medicine, and especially by the administration of steel. I told you formerly that preparations of iron have an exceedingly good effect, oftentimes, even in cases of *organic* disease of the heart, consisting in dilatation and tenuity, and consequently weakness, of its muscular parietes. A draught containing from fifteen to twenty minims of the tincture of the sesquichloride of iron, with ten minims each of tincture of digitalis and spirit of chloroform, I have found an eligible and efficient form in these cases. It may be taken thrice or twice daily.

On the other hand, if there be violent palpitations of the heart, with a strong heaving impulse, you may appease the excessive action, and afford sensible comfort to the patient, by applying leeches, from time to time, to the præcordia.

Of the *renal* variety of chronic general dropsy, whether pure or mixed, the treatment is less accurately ascertained. The uncertainty which has perplexed men's minds respecting the nature of the renal disorder, has extended, in some measure, to their choice of remedies for it.

The impoverishing effect of the disease upon the blood, the probable dependence of some of the more distressful symptoms upon the thin serous condition of the circulating fluid, and the increased facility with which the dilute blood may transude outwards—these are circumstances which discourage any abstraction of blood, except under special circumstances. When the urine is very scanty, and there is pain in the loins, with a threatening of uræmic symptoms, blood may with propriety and advantage be taken from the renal regions by cupping or leeching. Fomentations or poultices on the loins are also efficacious in relieving pain, lessening renal congestion, and so increasing the secretion of urine.

One definite object, in the renal as well as in the cardiac variety, is to remove the dropsical fluid; from which the danger and the suffering often chiefly proceed. But it is a more nice question, when the *kidney* is involved in the disease, how this is to be accomplished. Can we, with the same safety as in cardiac cases, employ diuretics? It has been thought that we cannot. As the primary morbid state of the kidney is certainly often a state of active or inflammatory congestion, it has been feared that direct diuretics, such as are calculated to cause, keep up, or augment such congestion of the kidney, or to stimulate and irritate that organ, would be likely to accelerate the disorganising process of which it is already the seat.

You had better, when you can, observe the caution which these views suggest. You had better endeavour to empty the distended cavities, and to relieve the loaded areolar tissue, through the bowels, or through the skin. Sometimes, however, —more often indeed than in cardiac dropsy—we have the untoward complication of irritable bowels, or of habitual diarrhœa: and then drastic cathartics are inadmissible. But when this complication is not present, they are eminently useful.

Much benefit is sometimes derived from measures that act powerfully or steadily upon the cutaneous transpiration; and especially from warm, or hot-air, baths. The hot-air bath is, in many respects, preferable to the common warm-water bath, and even to the vapour bath. Upon the principle of heterogeneous attraction, the escape of the liquid from the surface of the body is more promoted by a dry heat, than by water artificially raised to a high temperature, and even than by an atmosphere made moist, as well as hot, by vapour. The risk, moreover, of exposure to cold, and the inconvenience, and hazard of fatigue, are much less: for the hot air can be brought, with but little trouble or expense, to the patient as he lies in bed. When this expedient is not at hand, an extemporaneous and very serviceable substitute offers itself in what has been called the *blanket-bath*, the naked patient being carefully enveloped or 'packed' in a warm blanket.

These measures failing—as fail they often will; and diarrhœa forbidding the use of drastic purgatives; or drastic purgatives and diaphoretics together proving insufficient; we *must*, even in renal dropsy, of whichever variety, choose the least of two evils; or rather we must incur the risk of one possible and contingent evil, for the chance of obtaining what, if obtained, is a certain and positive benefit: we must endeavour to remove the dropsical accumulation by means of *diuretics*, whether these accelerate the progress of the disease in the kidney or not.

Such diuretics, therefore, are, in the first instance, to be selected, as seem the least likely to stimulate the kidneys injuriously. The bitartrate of potass has been found one of the most certain and useful. Digitalis also is esteemed safer, and therefore more proper for this purpose, than many others: and the simultaneous exhibition of these two has, perhaps, the surest effect of all.

When diuretic medicines act as we desire, and increase the quantity of urine, they are commonly of great service by reducing the dropsical swelling. Sometimes, however, no impression is made upon the anasarca, although the discharge of urine becomes plentiful. This is a very discouraging prognostic circumstance. And the drugs that we employ are apt to be very capricious and disappointing. In some unhappy cases of renal dropsy, I have tried every known form and combination of diuretics, without augmenting the secretion from the kidneys. Urea appears to be the main natural diuretic; and in this disease it already exists in the blood in too great abundance. On the other hand, the watery ingredients of the blood are sometimes transmitted readily enough through the diseased gland. I have told you that one form of the malady is often attended with diuresis. We may suppose that obstructed tubules may sometimes be washed out by the descending streams of aqueous liquid; and Dr. Johnson's suggestion is worth your attention, that, in these circumstances, the safest and most useful diuretic is pure water. A diet consisting exclusively of skimmed milk has of late been recommended in renal dropsy. I know that this has in some cases proved strongly diuretic, and that under its use albumen has disappeared from the urine. But milk is ill-borne by many stomachs, and unless it has a diuretic effect, much benefit can scarcely be looked for under such a diet.

Mercury is for the most part inadmissible in these cases. Salivation is apt to be produced by a small quantity of this drug; and to be unusually troublesome and severe, without bringing any commensurate advantage. I mentioned to you formerly Dr. Farre's opinion, that mercury has the property of rapidly destroying red blood: if so, it must be regarded rather as an ally, than as an antagonist of this malady.

When internal remedies prove ineffectual, and outward applications to procure sweating miss their aim, we turn to those mechanical expedients which (in either variety of general dropsy) often afford ease, and prolong life, and may even sometimes, perhaps, achieve a cure.

I have told you that the tense and stretched integuments occasionally give way; the areolar tissue sloughs, and from the breach thus made, water wells copiously forth, and great relief ensues. Sometimes, though very rarely, the whole of the accumulated liquid has so escaped, and the dropsy has not reappeared. The sore has healed, and the natural cure has been complete.

This spontaneous mode of draining away the liquid has been imitated by art. For the unwieldy legs become painful as well as cumbrous; the integuments threaten to inflame, or to mortify; and if we can diminish the tension by removing a portion of the included fluid, we avert or lessen this danger. The penis and scrotum also in the male, and the labia pudendi in the female, become in many cases enormously swollen, and hinder the exit of the urine, which is therefore spilled upon the tumid parts, rendering them erythematous and raw, to the grievous aggravation of the patient's sufferings.

Now seeing that vesications sometimes form upon the dropsical limbs, and give vent, in some degree, to the fluid, practitioners have been induced to follow that indication by exciting artificial blisters. But they are not safe remedies. They are apt to produce gangrene of the surface thus inflamed. A more popular and a better expedient is the practice of acupuncture, which consists in perforating the integuments here and there with a fine needle.

It is surprising how much fluid may be let out in this way; and how much relief may be bestowed by this trifling operation. The liquid trickles rapidly forth; and I have known it soak through the patient's bed, and form a pool on the floor of the room. In one instance, a physician being my patient, the limpid fluid which thus oozed from a puncture in his thigh was caught, and collected in a glass, by means of a little gutter of oiled silk. It was found that ninety minims, or a fluid drachm and half, escaped in a minute; which is at the rate of eleven ounces and a quarter in an hour: and this drain went on for upwards of four hours.

The surface in which punctures of this kind have been made sometimes becomes red: erysipelas supervenes, which it is difficult to arrest, and the patient sinks. In some of these cases the same event would probably have occurred, even although no punctures had been made, from mere tension of the integuments, and the progress of the disease. When such appearances present themselves, the affected limb should be kept in a horizontal position; and strips of linen, wetted with a solution of goulard, should be applied to the inflamed surface.

The risk of these ill-consequences, and also the efficacy of the puncturing, are probably less when the needles are used upon the thighs than upon the œdematous legs. The punctures should not be too near each other: an inch and half, at least, should intervene between them. Neither should they be too numerous, nor too deep. The depth must depend upon the circumstances of the case; and especially upon the place where the puncture is to be made. The needle must not be pushed so deep as to penetrate or wound any fascia; for the danger of subsequent inflammation would thereby be increased.

These punctures soon close up; and of late years, both on the Continent and in this country, the plan recommended long ago by Mead has been revived, of making a vertical *incision* through the areolar tissue in either leg, on its inside, about two fingers' breadth above the ankle. This method is found more convenient for the patient than acupuncture. The fluid escapes rapidly, and it is generally believed that the hazard of sloughing is not greater than when mere punctures are made. The cut may be about half an inch long; it should have a waterproof covering, whereby the part may be kept warm.

Sometimes a large flow of urine by the kidneys follows the drain thus made from the legs: the distended veins are emptied by the discharge through the areolar tissue, the supply of arterial blood is in some degree restored to the kidneys, and their suspended functions are resumed. There is often a manifest corresponding relief to the respiration, and to the cerebral feelings and functions.

The peritoneum may, at the same time, require to be emptied in the same mechanical way, by help of a trocar. I am always loath to recommend tapping, until the symptoms actually call for it, and until all other means of dispersing the water have been tried in vain. But the circumstances that warrant or demand the performance of the operation, the dangers that attend it, and the means of obviating those dangers, were fully pointed out when we were on the subject of ascites.

By whatever means we may succeed in getting rid of the dropsy, there will still remain (except in the comparatively few cases that are unconnected with organic disease, and depend simply upon debility and anæmia) the necessity for guarding against the re-accumulation of the water, by remedial measures addressed to the faulty organs. You may sometimes keep the disease of these organs in check, even when you cannot cure it.

In cardiac dropsies, besides the medicines already specified, undeviating temperance and regularity of life must be enjoined; and the patient should carefully, and always, avoid all active motion or exertion of the body, and all strong emotion of the mind; whatever, in short, might tend to hurry the circulation. You will scarcely be able to enforce this prudence, without plainly showing the patient the risk he will incur by its neglect.

In the renal variety of the disorder, in addition to the appropriate remedies already enumerated, particular attention must be paid to the avoidance of all exposure to cold and vicissitudes of the weather, and to keeping the surface of the body warm. Such patients should be constantly clothed in flannel from head to foot. To those who are able to choose their place of abode, I should strongly recommend resort to a warm climate. Always in this renal disease be cautious in prescribing medicines which, accumulating in the blood, may perhaps become poisons.

The diet in the chronic forms of the disease should be nutritive, but unstimulating. M. Solon suggests that if, in the renal cases, urea be detected in the blood, the patient should be restrained from too animalised a diet. Dr. Budd has had the same thought, and has put to the test, I believe, in the Hospital-ship *Dreadnought*, the utility of withholding all articles of food that contain azote. I have found this restriction entirely useless in one painful case, in which it was fairly enforced. In fact, the principle of such restriction appears to be wrong: the urea is furnished to the blood, not in the primary assimilative process, but in that which is secondary and destructive.

The discovery of the fatty character of one of these forms of renal degeneration has led to another plausible suggestion; viz., that the patient should be instructed rigidly to abstain from every kind of fat. But if the mottled and fatty kidney really be, as it probably is, one of the numerous issues of the scrofulous habit; this advice would seem to be of doubtful propriety, when we call to mind the confessed efficacy of some of the fixed oils, and especially of the cod's liver oil, in the treatment of strumous disorders. It is indeed the opinion of Dr. Williams, who bears strong testimony to this efficacy, that it results from the solvent power of the thin fish-oils upon the more concrete molecules of fat, by the morbid accumulation of which, the peculiar degeneration in question is constituted.

One more point, and I have done. Much unnecessary penance used to be inflicted upon dropsical persons, by stinting their allowance of drink. It was natural to suppose that the accumulation would increase in proportion to the quantity of liquid swallowed. But experience has shown this opinion to be erroneous, and

'crescit indulgens sibi dirus hydrops'

has ceased to be more than a poetical doctrine. The patient may safely be allowed to exercise his own discretion in this respect. When the peritoneum is full, distress is apt to ensue upon the distention of the stomach by drinks; but this source of suffering is soon discovered and avoided. The sick man is better able than his physician to judge which evil is the greater; the torment of unslaked thirst, or the discomfort that may be produced by its immoderate indulgence.

LECTURE LXXIV.

Chylous Urine. Hæmaturia; its diagnosis, general and particular; Local disorders of the Urinary Organs on which it depends; Treatment. Disease of the supra-renal capsules; Bronzed Skin. Abdominal Tumours.

I HOPE I have convinced you that the morbid conditions of the urine are worth studying. You have heard, probably, of the quacks who call themselves 'water-doctors,' and who pretend that, by mere inspection of the urine of a patient living at a distance, they can tell what is the matter with him, and how he may be cured. This skill, which looks like conjuring, the scientific physician of the present day does really possess. Of some very important forms of constitutional disorder, and of some specific local maladies, he reads the sure evidence in the sensible and chemical qualities of the secretion from the kidneys. And I do not hesitate to say, that a rightly instructed person might form a more accurate opinion respecting a sick man fifty miles off, and prescribe for him more judiciously, upon being furnished with a vial of his urine, than some practitioners whom I have known could do, if they had the patient bodily before them. You may learn much (and so, no doubt, you ought) by prying into the arcana of the night-chair: but you may learn more, I am persuaded, by the habitual perusal of the chamber-pot.

I have not quite done with the subject.

Sometimes urine is voided, which appears to contain *chyle*. It looks white and milky, and stiffens as it cools into a tremulous jelly, like *blanc-manger*, and takes the shape of the vessel into which it was passed. The coagulum gradually separates again into two portions: one of which is liquid and whitish, and when left at rest for a few hours throws up to the surface a sort of creamy matter, containing (as cream does) a butyraceous or oily principle; the other is a delicate fibrinous mass, of flesh-like appearance, having a red tinge from the presence of some of the colouring matter of the blood. This is the character of the urine passed a few hours after a full meal. When the patient has long been fasting, the urine is simply opalescent, and the coagulum small and partial. It never contains any casts of the urinary tubules.

Dr. Prout attributes this curious deviation from the natural qualities of the urine to a double fault; first, in the organs of assimilation; secondly, in the functions of the kidney. The chyle, from some derangement of the assimilative process, is not raised to the blood standard; and being unfit for its purpose, is ejected through the kidneys; and these organs, instead of converting it, as usual, into the lithate of ammonia, suffer it to pass through them unchanged.

Of this rare disorder I have not met with an instance. Dr. Prout had seen more or less of fourteen cases. Five of the patients were males, nine females. Two of the males, and one of the females, were below the age of puberty. Eight of the cases occurred in natives of the East or West Indies, or in persons who had lived for many years in hot climates. Mr. Thomas informs me, that during a residence of ten years in Barbadoes, he saw at least a dozen well-marked examples of chylous urine in negroes. It would seem, therefore, that a tropical climate predisposes certain individuals to this affection. In one case, drinking cold water while the body was warm seems to have been the exciting cause; and exposure to cold was thought to have had something to do with the attacks in other cases.

The general health suffers less than you might suppose. Two of the females, while labouring under the complaint, became pregnant, and brought forth healthy children: and one of the two lived nearly twenty years, with the disorder upon her all the time. Another of Dr. Prout's female patients, who since his death has been seen by Dr. Bence Jones, first noticed that her urine was chylous in the year 1827. It continued so, with occasional intervals, till July, 1853, a period of twenty-six years, when the lady was (as she may be still) alive and, this state of the urine excepted, in good health.

In the slighter cases there is usually some degree of feverishness, some uneasiness in the back and loins, some thirst, a dry skin, and torpid bowels. When the malady is more severe, the symptoms approach to those of diabetes; the thirst is more urgent, the appetite unnaturally craving: and there is some degree of emaciation and debility. In this severer variety the urine is apt to coagulate before it leaves the bladder; and the patient experiences difficulty in passing it, the urethra being blocked up by the clots. Dr. Prout states that he has known this to constitute the most troublesome symptom of the disease. In one of the fatal cases the body was examined, and the kidneys found perfectly healthy. Occasionally the complaint ceases of its own accord, even for years, and then recurs, without any apparent cause. We may conclude from these circumstances that it is a purely functional disorder.

With respect to treatment, little can be said. Dr. Prout had found several things of temporary service, in the chronic state of the affection. The mineral acids; astringents, such as alum, and the acetate of lead; opium; counter-irritation. But the suspended symptoms had always sooner or later returned. Dr. Bence Jones has put several drugs fairly to the test, and found the gallic acid more useful than anything else, though not a specific, in this disorder. In the case of a man, who also had formerly consulted Dr. Prout, the urine had been chylous, constantly, for more than a year. It ceased to be so two days after he began to take the gallic acid, and it remained free from the fatty admixture for 232 days, the acid having been continued for nearly two months. This looked like a cure. But the complaint recurred, and went, again and again. Twice subsequently, upon administration of the gallic acid, the chylous condition ceased for the respective spaces of 237 and 242 days. The acid was frequently given to the large amount of two drachms daily, for weeks together.

Tannic acid was tried in this case: but it could not be taken in doses so large; and it caused much more nausea, and much more headache, than the gallic acid. The chylous state was always abated by rest, and aggravated by exercise. A decoction of Mangrove bark is said to have arrested the disorder.

Urine which contains albumen exhibits it in the solid form under the tests of heat and nitric acid.

Yet I have met with one remarkable case in which those tests did not produce the usual precipitate, although an enormous quantity of an albuminous substance was passing out of the body in the urine.

This urine was of a high specific gravity. It became thick with heat, from a deposit of phosphates, but cleared again with a drop of acid. It gave no precipitate with an excess of nitric acid, unless left to stand, or unless heated and left to cool, when it became solid. This solid was redissolved by heat, and formed again as the liquid again cooled.

The case was one of mollities ossium. A full analysis of the urine has been published, in the 'Philosophical Transactions' for the present year (1848) by Dr. Bence Jones, who found the peculiar substance thus secreted to be the hydrated deutoxide of albumen.

There was as much of this albuminous substance in the urine, as there is of ordinary albumen in healthy blood. So far therefore as the albumen only was concerned, each ounce of urine passed was equivalent to an ounce of blood lost.

Dr. Bence Jones informs me that the same substance, in small quantity, is contained in pus: and has received the name of *pyine*. He thinks it probable that the substance which characterises chylous urine is closely related to this.

Lastly, the urine may contain *blood* itself: and I proceed to consider some of the phenomena that occur in connexion with *hœmaturia*; under which term I would comprehend every kind of bleeding from the urinary organs.

Blood alters, of course, the colour of urine with which it is mixed; giving it, in some instances, a bright red tinge, and causing it, in others, to assume a dark hue; to become brown, like coffee, or even to approach to blackness. Hence we are sometimes too ready to conclude that urine of a distinctly red colour, or so very dark as to appear almost black, derives its peculiar tint from blood that has somehow mingled with it.

But, in truth, urine may be perfectly red, or nearly black, and yet be quite free from blood. There are certain substances which, when taken as food, invariably impart a red colour to the urine. One of these is the prickly pear, or Indian fig as it is sometimes called, the *cactus opuntia* of botanists. When the Spaniards first took possession of America, many of them were alarmed by observing that they passed what they supposed to be bloody urine; but it was soon discovered that the red colour of the secretion was owing to the liberal use they made of that fruit. Dr. Hennen, in his book on 'Military Surgery,' quotes a precisely similar example from Elliot's Journal of his Travels for determining the boundary of the United States. He says that his 'people ate very plentifully of this substance at an island of the Mississippi (Kayo-ani), and were not a little surprised the next morning at finding their urine appear as if it had been highly tinged with cochineal. No inconvenience resulted from it. It would seem (he continues) that the juice of this plant may be analysed into a crimson dye by other processes beside that of the cochineal insect.'

Another vegetable substance with which, in this country, we are more familiar, and which will produce the same effect, is *beet-root*. Desault relates the case of a person who noticed that he every morning voided urine of a deep red colour; exactly such as would result from adding fresh blood to that liquid, except that no deposit took place. The man became frightened at this, and consulted M. Roux, who, after some examination, began to suspect that the water owed its red appearance to some other cause than an admixture of blood. It turned out that his patient was in the habit of supping every night upon the red beet-root; and as soon as, by M. Roux's advice, he relinquished this article of diet, he was freed at once from his supposed bloody urine, and from his fears.

A similar tinge is said to be given to the urine by the use of *madder* as food, by some species of strawberries, and by drinks made of sorrel. *Logwood*, which we sometimes use in medicine, has the same effect. Rhubarb also, and senna, give to the urine, *if it chance to be alkaline*, a blood-red colour.

It is right that you should know these facts: for by swallowing large quantities of such substances, and by complaining of sensations which they do not really feel, impostors may endeavour, without any difficulty, or pain, or danger to themselves, to deceive others into a belief that they are suffering under some serious and disqualifying disease, and are proper objects of charity. Moreover, a knowledge of the effects of these vegetable matters may sometimes enable you, as in the case mentioned by Desault, to remove unfounded anxiety and alarm from the minds of persons who are innocently and unconsciously giving themselves red urine.

The natural tint of the urine inclines towards redness, independently of any admixture of blood, in many instances of fever, and of acute inflammation. Occasionally urine of a pink colour is passed by persons who are subject to obstinate dyspepsia connected with organic disease. This pink tint is most apparent when the water is contained in an opaque, shallow, white vessel.

Again, urine of so deep a colour as to be called, in common parlance, *black*, may or may not owe that hue to the presence of blood. When blood is the cause of the unnatural colour, the blackness must be ascribed to the chemical action of some free acid upon the blood: as I showed you formerly to be the case with blood that is vomited, in hæmatemesis.

I also pointed out to you some little while since, that the urine, in jaundice, sometimes *seems* to be black, when it is collected in large quantity, and in a deep vessel. This colour is merely a concentration of yellowness, as appears at once upon diluting the urine with water. It then assumes a bright yellow colour. The aspect of the skin in icterus will always secure you against mistaking or overlooking this cause of black urine.

There has been observed, also, though very rarely, a form of black urine, depending upon the presence in that secretion of a peculiar principle to which Dr. Marcet gave the name of melanic acid. The only specimen of really black urine that I ever saw, was shown to me by Dr. Prout ; who knew nothing, however, of the circumstances under which it was voided. . It appeared to me to be full of coal-dust.

With these two exceptions, almost all urine that is of a very dark or blackish colour owes that quality to the circumstance of its containing blood, which has been more or less altered, by various causes, from its original appearance.

When blood is present in any considerable quantity, a portion of it subsides to the bottom of the vessel, and may be recognised without any difficulty. And even when there is not enough blood to give a marked and characteristic deposit, a very small admixture of it will be found to disturb the natural transparency of the urine, rendering it of a smoke-brown, or dull cherry colour: whereas the reddish or pink urine which contains no blood is clear and untroubled; and if, on cooling, it throw down a sediment, that sediment may be redissolved by heating the urine—a result which does not take place when a portion of blood has been deposited. Another rough test is, that a mixture of urine and blood tinges a piece of white linen dipped into it, of a red colour. A better criterion is afforded by gradually raising the suspected urine to the boiling temperature. If it contain blood, a grayish brown flocculent precipitate, consisting of coagulated albumen tinged with the colouring matter of the blood, will form, and gradually subside, and leave the supernatant liquid clear, and with its natural tint. But if you use the microscope, that will furnish you with the best evidence upon this point. If there be blood in the urine, there will be blood-corpuscles, turgid or collapsed, diffused through the urine, or collected at the bottom of the vessel: and whatever modifications they may present, they may always, Dr. Bird says, be identified by ‘their non-granular surface, uniform size, and yellow colour under the microscope.’

We have by no means accomplished the diagnosis when we have merely ascertained that there is blood in the urine; and that the case is a case of hæmaturia. The question remains, of what is such hæmaturia a sign? The blood emerges from the urethra, but it may have been poured out at any point of a long and somewhat complex tract of mucous membrane. It may have proceeded from one or both of the kidneys, from each or either ureter; from the bladder; from the prostate gland; or from the urethra.

Hæmaturia strictly idiopathic must be very rare. Cullen remarks that neither he nor any of his friends had ever met with an instance of it. I shall mention presently the only example of hæmorrhage from the urinary organs, apparently idiopathic, that has fallen under my own notice.

Blood in small quantities is excreted with the urine in that acute affection of the kidney which I have already spoken of as lying at the root of febrile dropsy. Respecting its origin in such cases we need have no doubt: for besides the colouring matter and corpuscles of the blood, the urine is found to contain blood-casts, which have obviously been moulded in the urinary tubules, and in which epithelial cells, detached from those tubules, are often entangled. The same phenomena present themselves sometimes — though much more rarely—in the chronic forms of Bright's disease. Blood-casts, without epithelial cells, have been seen by Dr. Johnson in a case of strangury produced by oil of turpentine; and it is probable that the appearance and conditions of the urine are the same in strangury from cantharides. Hæmaturia is occasionally, I believe, vicarious of some other hæmorrhage, and especially of bleeding from the hæmorrhöidal vessels: so that it is always right, in obscure cases, to inquire whether the patient has been habitually subject to hæmorrhage from the rectum; and if so, whether that hæmorrhage is suspended. These cases have even been called *hæmorrhöides vesicæ*.

Hæmaturia occurs also, independently of any strictly local complaint, in the course of certain disorders which affect the system at large; especially in scurvy and purpura hæmorrhagica. Bloody urine is sometimes a symptom, and one of the most fatal augury, in typhus fever, small-pox, measles, and the plague.

But setting aside these more general forms of hæmaturia, let us inquire what local affections of the urinary organs, besides those already referred to, may give rise to hæmorrhage; and how, under different circumstances, we are to interpret this symptom.

One very common source of hæmorrhage from the urinary passages, is the presence within them of calculous matter. The pressure occasioned by the aggregation of the earthy mass, when it is formed in the kidney, or by its accidental change of position, lacerates or lays open by ulceration some of the smaller vessels with which it is in contact. And in those cases in which a calculus descends into the bladder, and is ultimately voided, it may, in succession, give rise to hæmorrhage, first from the kidney from which it is separated; secondly, from the narrow tube of the ureter through which it is forced; thirdly, from the bladder which it enters, and wounds, or irritates; and fourthly, from the urethra in the last stage of its progress out of the body.

There will be the same liability to hæmaturia, if the concretion, instead of coming down from the kidney, be formed originally in the bladder. The appearance of blood in the urine suggests therefore, in many cases, the fearful suspicion, that there is, or is likely to be, a stone in the bladder. Dr. Heberden, in his 'Commentaries,' says, 'urine made of a deep coffee colour, or manifestly mixed with a large quantity of blood, has within my experience been very rarely the effect of anything but a stone in the urinary passages. I therefore suppose a strong probability of this cause, whenever I see this appearance.'

Again, blood may proceed from the kidney, or from the bladder, in consequence of malignant fungous growths, to which those parts are liable: a disease which, though more surely fatal than the stone, is scarcely, to the unhappy subject of it, so appalling.

Hæmorrhage may take place from the surface of the bladder from chronic disease, not cancerous, of that membrane. Mr. Howship has recorded an instance of this kind which occurred in Mr. Heaviside's practice. An old East Indian, who had long been subject to nephritic complaints, was suddenly seized with what was thought to be retention of urine. A catheter was passed, but as no water flowed it was supposed that it had not entered the bladder, in the situation of which there was a manifest tumour. The patient died the next day; and the bladder was found distended by a very large coagulum of blood which had come from the diseased mucous membrane. There was no trace of hæmorrhage in the kidneys, nor in the ureters.

Hæmorrhage, to a considerable extent, occurs in acute inflammation of the bladder. In such cases the blood is mixed with other inflammatory products.

I hold in my hand a preparation showing disease of the prostate gland, which had been accompanied by hæmaturia.

Now we judge of the exact seat of the hæmorrhage, and of its cause, partly by the nature and appearance of the effused blood, and partly by the symptoms that precede or accompany the bleeding.

Dr. Prout states that 'when blood is derived from the *kidney*, it is in general equally diffused throughout the whole urine: on the contrary, when derived from the bladder, the blood for the most part comes away in greater or less quantity at the termination of the discharge, the urine having previously flowed off nearly pure.'

There are also certain modifications of the sensible qualities of the excreted blood, by means of which the same eminent physician thought he could pronounce, with considerable confidence, that the hæmorrhage was owing to malignant disease. 'The red particles of the blood (he says), discharged in the earlier stages of fungoid disease, have often a remarkable appearance, and *appear to the eye* larger than natural; so that after they have subsided to the bottom of the urine, they at first sight somewhat resemble grains of lithic acid gravel, and, like that substance, when the vessel is inclined, may be distinctly seen to roll along the bottom. From this peculiar appearance of the red particles of the blood, the presence of malignant disease may be often suspected before the symptoms assume a decided character.' In a more advanced stage of the disease, there is often a dark-coloured offensive bloody sanies in the urine, and more or less of mechanical impediment in passing it. I should conceive that the microscope might settle the diagnosis of such cases.

There is one phenomenon which, whenever it occurs, is very characteristic of hæmorrhage from the kidney, or from the commencement of the ureter. I mean the expulsion with the urine, of slender cylindrical pieces of fibrin, which have evidently been moulded in the ureter, and subsequently washed down into the bladder by the descending urine. These little coagula are commonly of a whitish colour, the red particles of the blood having been removed; and they look like slim maggots or small worms. They denote, with much certainty, that the hæmorrhage which they accompany is renal.

The presence of microscopic blood casts of the uriniferous tubes also affords conclusive evidence that the blood comes from the substance of the kidney.

Such, then, are some of the points of diagnosis furnished by the qualities of the excreted fluid itself.

The bleeding may be presumed to come from the kidney, or from the upper part of the ureter, when it is accompanied or preceded by a sensation of heat, or of weight, or by some degree of pain, in the situation of the kidney; especially if these uneasy feelings are confined to one side of the body. This presumption will of course be strengthened if calculi have been known to descend from the kidney; and converted into certainty if the patient suffer, together with the hæmaturia, a fit of the gravel; and if there be no symptom of stone, or of disease, in the bladder. As the blood comes, in these cases, from the calices or the pelvis of the kidney, the urine contains no blood-moulds of the renal tubes.

On the other hand, when no symptoms referable to the kidney or to the ureter are present, while there are signs of stone, or of disease of the bladder, or of a diseased prostate—a mixture of mucus with blood; occasional retention, or a sudden stop in the stream of urine; pain referred to the glans penis immediately after the bladder is emptied—then we conclude that the blood proceeds originally from that receptacle.

When pure blood comes away, either *guttatim*, or in a stream, unmixed with urine, and neither preceded nor accompanied by any desire to make water, it is probable that the *urethra* is the locus of the hæmorrhage.

Bleeding from the surface of the urethra doubtless may, and commonly does, proceed from some mechanical injury done to that channel: as in the passage outwards of a fragment of stone, or inwards of a surgical instrument. But it is probable that blood may sometimes exude in considerable quantity from ruptured capillaries of the same membrane, under circumstances which favour or produce a strong determination of blood to the genital organs. A young man came to the Middlesex Hospital with hæmorrhage from the urethra, and said that he had lost a considerable quantity of blood in this way, within a few hours. The hæmorrhage appeared to have been the consequence of excessive indulgence in sexual intercourse. His own account of the matter was that he had passed the night with a woman, in whom the monthly period had just returned; and he ignorantly fancied that the hæmorrhage from his own person was the result of a sort of contagion. However, the bleeding was permanently arrested by the introduction of a bougie, which was allowed to remain for a short time in the urethra. This was the solitary instance to which I alluded just now, of (perhaps) idiopathic hæmorrhage occurring within my own knowledge. When the hæmorrhage comes originally from the urethra, the blood may regurgitate into the bladder, and coagulate there ; and mislead an observer into the belief that the hæmorrhage was vesical.

It appears then, that, in many instances, certain local symptoms are associated with hæmaturia, and point distinctly to the part of the urinary apparatus whence the blood proceeds.

But many cases are very obscure. Blood sometimes appears, mixed in greater or less quantity with the urine, when there is no pain, nor any other sign which would lead us to fix upon one part rather than another as the source of the hæmorrhage. Now I believe that hæmaturia, bearing this indeterminate character, will generally turn out to be *renal*, and to depend upon concretions in the kidney. This conclusion will be strengthened if (as often happens) there are more or fewer *pus* globules excreted with the blood. It is true that the hæmorrhage which results from cancerous disorganisation, whether of the kidneys or of the bladder, may also be painless. But cancerous disease of these organs (unless it extends from parts in the neighbourhood, as from the rectum or from the uterus, to the bladder) is very rare ; and when it does occur, the nature of the case may usually be ascertained from those peculiar qualities of the effused blood which I have mentioned as being characteristic of malignant growths.

A calculus can seldom remain long in the *bladder*, at any rate will seldom cause bloody urine, without giving some other notice of its presence there : but concretions form in the kidney, sometimes in great numbers, and reach a considerable size, and remain there long, without furnishing any signal from which we might suspect their existence ; except (perhaps) the occurrence of hæmaturia. We know this, because calculi are frequently met with in the kidneys of persons who had never suffered any pain or obvious derangement of the urinary organs during life ; and because, in other persons, in whom such calculi pass down from the kidney towards the bladder, the first notice of their existence is often given by the acute suffering they inflict during their transit through the narrow ureter.

Yet though calculi may lodge in the infundibula, or in the pelvis, of the kidney, without manifesting their presence by exciting pain, it is very conceivable that by progressive enlargement they may lay open, or by accidental change of position they may wound, some of the smaller blood-vessels of the part, and so give rise to painless hæmaturia. It will strengthen the presumption that such is the source of the bleeding, if it have succeeded (as hæmorrhage from the urinary passages often does succeed) to a fall; a shock, or jar of the body; or jolting on horseback, or in a carriage. Similar movements may occasion bloody urine when there is stone in the bladder; but then the irritation will be *felt* in that sensible part; the hæmaturia will *not* be painless; the bleeding will not be the only symptom.

It is then, I say, my belief that very many of the obscure cases of hæmaturia may be referred to renal calculi; and if this view of the subject be correct, it will render it probable that the alleged instances of idiopathic hæmaturia ought thereby to be reduced in number.

There is a very curious kind of hæmaturia, endemic and very common in some tropical regions, and depending upon the presence in the bladder of the ova of an entozoon to which (in honour of one of its describers) the name of Bilharzia has been given. This affection seems to be most common in boys, and to disappear in later life: it has been noticed however in persons of adult age, and in both sexes.

The chief symptom is the expulsion of a very few drops of blood, scarcely ever so much as a tea-spoonful, or of a little mucus tinged with blood, after the bladder has emptied itself of clear urine. There is no great irritation of the bladder, or frequency of micturition or impairment of the general health. But the complaint has in it this of importance and of peril, that the ova sometimes become a nucleus around which calculous matter is deposited, and so the foundation is laid of stone in the bladder.

I have seen one person only, a son of the great traveller Dr. Livingstone, who had suffered this form of hæmaturia. It was contracted by him apparently at Kolobeng in Africa, where he was born.

Nothing certain has been ascertained as to the way in which these ova get into the bladder, nor as to any sure method of getting rid of them. You may read two interesting papers on this subject by Dr. John Harley, in the forty-seventh and fifty-second volumes of the 'Medico-Chirugical Transactions.'

The expulsion of the blood in hæmaturia, whether it be painful or not, is sometimes attended with severe rigors. I mentioned before that, in some persons, almost any irritation of the urethra, the passing of a bougie for example, will bring on a shivering fit. I had some time ago a patient under my care in the hospital, who had hæmaturia of an obscure kind, and the discharge of blood was always marked by a smart rigor. Dr. Prout speaks of an instance of obstinate hæmaturia in which a shaking fit constantly preceded the hæmorrhage. Dr. Elliotson, too, in one of his lectures, gives an account of a case of intermittent hæmaturia. The patient was under his care in St. Thomas's Hospital, and had formerly suffered the Walcheren fever. He was admitted for ague, and every time that the cold stage of his attack came on, he voided a quantity of pure blood from the urethra. He was cured, by quina, both of his ague, and of his hæmorrhage.

If we may trust to the records of physic, instances of periodic hæmaturia are not uncommon.

One circumstance yet remains, worth noticing, in respect of hæmaturia; and it depends upon the hæmorrhage itself rather than upon the disease of which the bleeding is a sign. I allude to the coagulation of the effused blood in the bladder, however it may have got there. This circumstance is sometimes the source of much inconvenience and suffering, and even of danger, to the patient. It may cause retention of urine and all its evil consequences; and a still worse event is that the coagulum sometimes supplies a nucleus upon which calculous matter collects, and thus gives origin to that horrible malady 'the stone.'

You will perceive, from what I have said, that the *treatment* of hæmaturia resolves itself, in most cases, into the treatment of the disorder, or bodily condition, with which the hæmorrhage is associated, and of which it is merely a symptom.

Sometimes, however, the bleeding itself is so profuse, or so long-continued, as to require direct efforts on our part towards its restraint.

'When (says Dr. Prout) the bladder becomes distended with blood, and complete retention of urine in consequence takes place, recourse must be had to a large-eyed catheter, and an exhausting syringe, by the aid of which, and the occasional injection of cold water, the coagula may be broken down and removed. If the hæmorrhage be so profuse that the bladder becomes again distended with blood in a very short time, the injection of cold water into the rectum or bladder is sometimes of great use; and should these means fail, from twenty to forty grains of alum may be dissolved in each pint of water injected into the bladder ; a remedy that seldom fails to check the bleeding, even when the cause is malignant disease. I have never known any unpleasant consequences follow the use of this expedient, and have seen it immediately arrest the most formidable hæmorrhage, when all other means had failed; and when the bladder had repeatedly become distended with blood, almost immediately after its removal.' A solution of tannic acid injected into the bladder might also be employed as a styptic.

Mr. Clover's apparatus for washing out the bladder after lithotrity would be available in these cases.

Among remedies given by the mouth, the same physician thought highly of the acetate of lead. I have mentioned before, in these lectures, a nostrum called, after the name of its inventor, *Ruspini's styptic.* This has often been known to put a stop to hæmorrhage which had resisted other remedies. I will read you one example of this from Sir Benjamin Brodie's published lectures. Speaking of hæmaturia dependent upon disease of the prostate gland, he says: 'Those medicines which operate as styptics when taken internally, and which are useful in cases of hæmorrhage from the lungs, are also useful in hæmorrhage from the prostate. I had a patient with very diseased prostate. A frightful hæmorrhage took place. The usual methods of treatment were adopted, but were of no avail. The skin became pale, the pulse became weak, and the patient was exhausted; yet the bleeding continued. Large quantities of blood were drawn off with the catheter: nevertheless the bladder continued to become more and more distended with blood, and was felt prominent in the belly as high as the navel. All other remedies having failed, I gave the patient a dose of the nostrum known by the name of Ruspini's styptic, and repeated the dose two or three times in the course of the next twelve hours. In about half-an-hour after the first dose was taken the hæmorrhage ceased; and it never returned. The patient lived a year and a half afterwards, and there was no reason to believe that any ultimate harm arose from the bleeding.'

For a long while this nostrum seems to have baffled analysis. The late Dr. Maton told me that Dr. Wollaston had examined it, and had arrived at the negative conclusion, that it contained no metallic substance. Dr. A. T. Thomson afterwards announced that it mainly consisted of a solution of *gallic acid* in alcohol, diluted with rose-water: but I have since been told by Mr. Squire that he carefully examined two or three bottles of Ruspini's styptic and that he could detect no gallic acid.

There is no substance more highly spoken of as a remedy for internal hæmorrhages by foreigners, and especially by the French, than the extract of *Rhatany* root, the *Krameria* of our Pharmacopœia. A woman was sent to me by my colleague, Mr. Arnott, complaining that for some weeks she had been passing bloody urine. She had gone through the ordinary routine of treatment without benefit. There were no symptoms present which threw any light on the precise source or cause of the hæmorrhage. I recommended a trial of the rhatany, and she began to take a scruple of the extract, mixed with water, three times a day. As in Sir Benjamin Brodie's case, the hæmaturia ceased after the first dose, and it did not return for many months. I mention this instance the rather, because the *gallic acid* enters into the composition of this vegetable extract also.

Now the gallic acid is one of those substances which, when introduced from the digestive organs into the blood, passes through the round of the circulation unchanged, and reappears in the urine. We may conceive, therefore, that it stays internal hæmorrhage by exerting its astringent property upon the ultimate capillary blood-vessels in its passage through them. It certainly is applied, in solution, after its elimination from the blood, to the urinary passages: and thus, in hæmaturia, it may be presumed to produce its styptic effect upon the bleeding surface.

To the same principle are owing, I believe, the astringent and styptic virtues of uva ursi, bistort, tormentil, the pomegranate, kino, catechu, and the several preparations of gall nuts. But as the efficacy of the gallic acid in restraining internal hæmorrhage is now well established, I would recommend you to employ it in its separate and simple state; since it may thus be prescribed in quantities more definite and precise, than would be possible in its natural combinations with so many different vegetable matters. It may be given three or four times a day, in doses of five or ten grains each, suspended in water by means of mucilage.

In cases of vesical hæmorrhage, with an alkalescent state of the urine, much benefit has been obtained from a steady use of the muriated tincture of iron. The oil of turpentine, in doses of twenty minims each, is a valuable styptic in such cases; but it must not be given when the kidney is known or suspected to be the source of the bleeding.

In the earlier courses of these lectures I have said nothing—for I knew nothing—of the morbid conditions of the *supra-renal capsules.* I was ignorant alike of their physiology and of their pathology; of their uses and of their diseases. A pathology, however, they have, which vindicates the importance of these little organs in the bodily economy, although it does not disclose their functions or purpose. A peculiar spoiling of their structure is linked with a strange and mortal disease; and the inward morbid change is revealed before death by one very conspicuous signal—unregarded, indeed, till our own day, and reserved for the sagacity of our distinguished countryman, Dr. Addison, to discern and to interpret. It is very remarkable that two physicians living at the same time, in the same town, and attached to the same hospital, should have brought to light two most serious forms of disease, utterly unknown before, in parts of the body so contiguous: and have thereby won for themselves an enduring place in the records of medical science. Henceforward the names of Bright and of Addison must be held in honourable remembrance whenever mention is made of renal and of supra-renal pathology.

Without dwelling upon the steps which led Dr. Addison onwards to his remarkable discovery, I will describe the group and succession of symptoms that accompany and indicate the (perhaps) specific morbid condition of these internal organs.

It is not very uncommon to meet with a sick person whose history is of this kind. He knows not how, nor precisely when, his illness commenced, but he has gradually fallen from his usual state of health: has become weak, pale, thinner, but generally not emaciated, languid, spiritless, unequal to bodily or to mental exertion, with flabby muscles, and a soft pulse, which commonly is very feeble also.

For this enfeebled and anæmic condition you can trace no intelligible cause. There has been no exhausting profluvium; no loss of blood, morbid or artificial; no diarrhœa, diuresis, or other drain upon the strength; no wasting excess or indulgence; no mental shock or anxiety. No fault is discoverable in the lungs, in the heart, in the kidneys, in the digestive organs; no direct evidence of malignant disease in any part. Yet the diminution of strength is progressive—faintness and vomiting frequently supervene—the mind becomes confused, the pulse grows weaker and weaker, until at length the flickering lamp of life goes out, or is extinguished in a sudden convulsion.

This unexplained train of symptoms should suggest the suspicion of disease in the supra-renal capsules; and the suspicion will be turned into something like certainty, if, during the progress of the case, a change of colour begin to be perceptible in the patient's skin, first and chiefly in parts that are uncovered by the dress, as the face and hands, but elsewhere also. The colour which thus takes the place of the natural hue, is a brownish yellow. You fancy, while it is yet slight, that the hands and face may have been tanned by the sun: or that the patient may be labouring under a dirty-coloured jaundice. This latter notion is, however, at once refuted by the pearly whiteness of the conjunctivæ, the paleness of the finger-nails, and the absence of any bile-tinge in the urine. Dr. Addison speaks of this colour, which deepens with the advance of the disorder, as being dingy, smoky, as presenting various tints or shades of amber, or of a chestnut brown. It is somewhat like the stain produced by the juice of walnuts, or by the tincture of iodine; or, finally, the skin looks, in the affected parts, as though it had been *bronzed*. And the name of *bronzed skin*, familiarly given to the complaint, is preferable, while we are still so ignorant of its real nature, to any more formal appellation which might be derived from the Greek or Latin.

This tawny hue is not diffused uniformly over the skin, nor have the darkest parts any definite outline. It occupies principally the front of the body and of the limbs. The brown colour varies in intensity, being darkest in the flexures of the body, in the armpits, and groins, also in the nipples and their areolæ, round the navel, and upon the scrotum. Spots that have been blistered become very dark; and sometimes even the rings that are made by the pressure of garters. In a well-marked case the hue of the patient resembles that of a mulatto: and it has been ascertained that the pigment to which the deepened colour is owing, is limited, as in the mulatto, to the cells of the *rete mucosum*, where it is present in a granular form. In many of the patients very characteristic dark stains may be seen running along their lips, and upon their gums, and the inside of their cheeks. In one of Dr. Addison's published cases black spots were sprinkled over many of the abdominal viscera.

My own experience of this formidable disease has been but small. The barrister whose case (the 6th in the book) is described by Dr. Addison, was a patient of mine; and it was a vast comfort to me to have his advice and help under circumstances so distressing.

In January 1856, I was consulted at my own house by a surgeon from the country, thirty-four years old, who was evidently drooping under the same malady. His face had a tanned appearance, his hands were bronzy, with pink contrasting finger-nails, his scrotum was very dark, and a few soot-like spots were scattered here and there upon his skin. His urine was light coloured. His stools had never wanted bile. There was no tinge of yellow in his eyes.

He told me that he first noticed the dark colour of his scrotum in 1853. He died in April 1857. The disorder existed therefore for four years at least. He began to perceive that he was unwell—to feel weak, to waste, to suffer fits of languor and depression, and to change in complexion—in November or December 1854. Cold weather always distressed him.

The course of this gentleman's disease was marked by fluctuations. He regained flesh and strength at Hastings, the bronze-like hue varied in depth, his spirits were less depressed, and he resumed very active duties, as a coroner, and in his calling as a surgeon; but he sank at last under some acute affection of his larynx.

The supra-renal capsules were found large, and infiltered with crude and suppurating tubercular matter. In the year 1847 Mr. S. was near dying from an abscess in the right loin. From the cicatrix of this abscess a closed sinus was distinctly traced to the right capsule.

This case was remarkable for its duration, and for the energy which the patient displayed even in a late stage of the disease, and between occasional periods of most distressing prostration, palpitation, and sickness. During his last fatal attack it was noticed that a very fœtid smell proceeded from his body.

Casting my memory backwards I cannot but suspect that similar instances, transiently seen among my home patients, may heretofore have been erroneously set down as obscure cases of jaundice.

Some care is requisite in forming an exact diagnosis of this remarkable malady. Dr. Addison, to whose acumen its first recognition was due, drew his conclusions from a very small number of observations. From one supposed instance he fancied that the darkened parts were sometimes diversified with white spots and patches, in which the skin was blanched, was whiter than healthy skin, the hairs on these spots being also perfectly white. But it is now well known that these appearances denote a very different cutaneous condition, to which the name of *Leucoderma* has been given. In this affection the spots are sharply defined, and the edge of the spreading tint, whether of brown upon the natural skin, or of white upon the brown, is always convex. Again, a hasty or inexpert observer might easily confound the peculiar bronzing with another skin disorder, *Pityriasis*, which also is characterised by a brown colouring, the result of a vegetable parasitic growth upon the surface of the chest, abdomen, back, and upper arms. The epiphyte is readily demonstrable by the help of a microscope: and it may be killed and cleared away by a lotion containing corrosive sublimate.

Not less accuracy is needful in determining the character of the change which takes place in the capsules. What the functions of these little bodies may be, Physiology has not yet pronounced. They are amply supplied with blood; they have a cortical and a medullary structure analogous somewhat to those of the nervous centres; and they have direct and numerous relations with the great solar plexus.

In the cases in question they are mostly enlarged, of irregular surface, and harder than healthy capsules. Sometimes their external membrane is found glued to the surrounding parts by bygone inflammation. When cut through by a sharp knife the cut surface is seen to have lost entirely the natural distinction between the cortical and central portions of the gland. It presents a confused mixture of gray, firm, translucent substance, with softer yellowish granular matters, and sometimes with a little puriform fluid; and so far it resembles the débris of pulmonary tubercles. Under the microscope cellular elements in a shrivelled and decaying state are discernible, fatty degeneration, and often some gritty calcareous material, or stuff like putty, or a few crystals of cholesterine.

This condition of the supra-renal capsules, concurring with the true bronzing of the skin, and with rapid failure and extinction of the powers of life, constitutes a distinct and veritable disease. It has been only through loose or inexact observation of one or other of these three factors, that doubts on this subject have arisen. Dr. Addison's title to the discovery of a new and real malady has been well vindicated by his friend and sometime pupil Dr. Wilks.

In support of the positive evidence that we now possess, it may not be superfluous to adduce the negative also. In 500 inspections of the dead body conducted by Drs. Wilks and Habershon, in all of which the renal capsules were examined, disease of those organs was never found without its having been predicted during life, except in two instances. In one of these the skin had shown a slight dinginess, and a large mass of cancerous disease involved the whole of one of the capsules. In the other case also one only of the capsules was affected; a few malignant tubercles grew from its surface.

Whether the disorganisation of the renal capsules is the cause of the symptoms, or whether the disorganisation and the symptoms are common effects of some undiscovered morbid influence, we do not know. In every well-authenticated instance of the disease, the renal capsules of both sides have been similarly, though perhaps unequally implicated. This fact furnishes strong ground for regarding it as a blood disease; and the connexion of the capsules with the spinal cord on the one hand, and with the semi-lunar ganglia and the great solar plexus on the other, warrant the further conjecture that such blood disease may depend upon some antecedent fault in the nervous apparatus, upon which the healthy functions of the smaller blood-vessels are unquestionably dependent. The prostration of the vital energies which characterises so remarkably acute inflammations within the abdominal cavity; the fatal effect, often, of a sudden smart blow upon the epigastrium, or even, under certain circumstances, of a large draught of cold drink; seem analogous to the rapid sinking witnessed in this disease: and all are referable, with much probability, to the great plexus, or centre, of the sympathetic system of nerves which lies in that region. The supposition that the disorder takes its origin in the nervous system is further strengthened by the noteworthy fact that in many of the recorded cases there were pains in the back, and even caries of the spine, generally in its dorsal or lumbar vertebræ, which lie in contact with the supra-renal capsules. In Mr. S.'s case, as I have told you, a healed sinus was traced from the scar of a bygone lumbar abscess into the right renal capsule.

A smell like that which proceeded from his living body has been noticed in several other instances. In all probability it is merely the peculiar cadaveric odour which is apt to accompany the slow extinction of life.

Whether the organic mischief in the affected capsules is something *sui generis* may, I think, be doubted. It is clear that not every disorganising process suffices for the production of the disease. Cancer of the capsules, always probably secondary, does not necessarily produce it. Some writers have regarded the change as essentially tubercular. Tubercles have, a few times, been found in the lungs, not numerous however, nor far advanced; nor oftener probably than in various other disorders. Certainly the supra-renal disease is a very rare accompaniment of pulmonary consumption.

If I were obliged to offer a conjecture on this subject, it would be that the primary affection of the capsules is of an inflammatory character; and that the morbid conditions discovered after death are explicable by that kind of metamorphosis through which, in all organs of the body, spoiled tissues, and the intermingled products of their inflammation, degenerate into cheese-like and fatty matters.

It would be idle to speak to you of any *cure* for these cases. A strengthening plan, both of diet and of drugs, would naturally suggest itself. Mr. S. believed that he improved greatly for a time under quinine, and Margate ale. The pallor and the feebleness invite to the use of steel: but the malady has been permanently arrested by no special effort of our art that I am aware of.

You will find a very complete account of what is known respecting this singular malady in Dr. Greenhow's small volume on 'Addison's Disease.'

I am unwilling to take leave of the cavity of the abdomen, without saying a few words (very few they must be) respecting the various kinds of *tumour* to which it is obnoxious. It may seem strange that the diagnosis of abdominal tumours, which manifest themselves to the touch and to the sight, should be so difficult and puzzling as it often is. I mentioned some reasons for this before: the loose and shifting manner in which some of the viscera of the belly are packed and fastened; their liability to enlarge beyond their natural limits; their accidental dislocations under disease. It would be vain to attempt here even a sketch of the infinite variety of these deviations from the healthy state. Every case of abdominal tumour forms a separate object of study, and must be judged of by its proper circumstances. All that I can profess to do, is to offer you some rough hints on this interesting subject.

Some kinds of tumour result from morbid growths; such are all the varieties of cancer: some from the presence and multiplication of parasites; of which we have examples in collections of hydatids: some are produced by the distention of hollow organs; as when concretions, or fæcal matters, or gases, lodge in the intestines; or when urine accumulates and is pent up in the bladder: some consist in the mere enlargement of parts.

Let us enumerate the principal of these; that you may know what chiefly to expect.

1. There are, I say, tumours from lodgments in the *bowels*; and these are more hopeful, as well as less common, than most kinds of abdominal tumours. Sometimes the stomach, or some part of the intestinal canal, is distended in consequence of a mechanical impediment to the course of its contents: and this impediment may be invincible.

2. *Ovarian* tumours are very common. Of these I spoke at some length in a former lecture.

3. The *liver* is very liable to enlargement: either from simple congestion of its blood-vessels, or of its biliary vessels; or from the interstitial deposit of adipous or of lardaceous matter; or from the intrusion of malignant growths; or from colonies of hydatids.

4. So also the *spleen* swells, from fulness of blood, or from specific deposits in its substance.

5. The *kidneys* sometimes attain a vast size; being occupied by malignant or by scrofulous disease, or swollen by pus, or by urine, that finds no vent.

6. Enlargements of the *mesenteric glands*; cancerous degeneration of the peritoneum, especially where it forms the *omentum*; tumours connected with the *uterus*; aneurisms of the *aorta*; vast distention of the *gall-bladder*; constitute other species of abdominal swelling, which I simply mention without further comment.

Now our judgment of the character of a given tumour is naturally influenced by its *place*. In the right hypochondrium, we suspect the liver; in the left, the spleen; in the epigastric region, the liver or the stomach; in the hypogastric, the womb, or the bladder; in either flank, an ovary, or perhaps a kidney; in the track of the colon, we guess at fæcal collections.

But sometimes the situation of the tumour fits more than one, or than two, suppositions. Between the ribs and the ilium on the right side we may have an enlarged ovary, a tumid kidney, a distended cæcum. A prominence in the epigastrium may be due to enlargement of the left lobe of the liver, to cancer of the stomach, to an infarcted transverse colon, to a ventral aneurism. Above the pubes, the distended bladder, or the enlarged uterus, may equally project. The sigmoid flexure of the colon loaded with fæces, the left kidney exaggerated by disease, a bulky ovary, may either of them occupy the same sinistral space.

Moreover, the colon deviates strangely, and not seldom, from its natural course and position: and the magnified viscera may invade, by their displacement, or by their irregular expansion, the regions that are proper to other organs.

Our conjectures are assisted by the associated symptoms, and by observation of the regular performance, or of the disturbance, of particular functions. Yet here, also, we meet with continual sources of fallacy. Pressure from a tumour without, as well as infarction within, may impede the passage of alimentary matters through the bowels, of urine through the ureters; and cause, in the one case, flatulence and tormina, in the other, retention or suppression of urine. Growths foreign to the liver may, nevertheless, press upon its excretory ducts, and occasion jaundice. And so of other parts and functions. I mean, that the functions prominently deranged are not always the functions of the part actually involving the tumour, but of organs which are secondarily and accidentally subjected to its disturbing influence. Your sagacity will be abundantly tried in balancing the evidence of different symptoms in these obscure, yet palpable, forms of disease: and after all you will often doubt; and often, when you do not doubt, you will mistake.

Enlargement of the liver may usually be distinguished from other tumours of the right hypochondrium, by *percussion*. Try from the clavicle downwards. At first, you get a hollow sound. Then, a little below the nipple perhaps (for the spot varies much in different subjects) the sound begins to grow dull. If this dulness be traceable, without change or interruption, to the tumour, the inference is strong that the tumour is hepatic. Any other tumour there situate leaves, most commonly, when the patient is recumbent, a palpable sulcus above it; or a space in which the sound, upon percussion, is different from that which is yielded by the liver.

Do not mistake a merely *displaced* for an *enlarged* liver. The liver may be pushed downwards by large effusion into the right pleura, or even by a large dilated heart. If the liver can be felt by palpation in the hypochondrium, there is disease somewhere; it is either displaced or diseased. A spleen that can be felt is an enlarged, and probably a diseased spleen. Of these two viscera, the liver and the spleen, remember that, unlike other abdominal tumours, they ascend and descend with the corresponding upward and downward movements of the diaphragm in respiration.

Percussion helps us to discriminate an ovarian from a renal tumour. When the swelling is large, the intestines lie behind the one, in front of the other: and the sound is affected accordingly.

Tumours that are readily *moveable*, are generally intestinal, splenic, omental, or ovarian. Sometimes the healthy kidney is capable of considerable movements.

A pulsating tumour is not necessarily an aneurism. The healthy artery will lift almost any sort of hard swelling that happens to lie directly over it. Aneurisms pulsate *laterally* as well as anteriorly.

The occurrence of hæmatemesis or of melæna would corroborate your belief that a tumour in the right hypochondrium was hepatic—in the left, was splenic.

Even when you are satisfied as to the organ affected, there comes another question, scarcely, in some cases, less difficult than the first—What is the *nature* of the tumour?

Suppose, for the sake of illustration, that your inquiry relates to the liver. If the tumour be large, prominent, smooth, roundish, of slow growth, and the general health be not materially deranged, it is most likely a *hydatid* tumour. If along the edge and upon the surface of the augmented liver, you can feel large inequalities and projections, and if the complexion and general state of the patient are expressive of failing health, the enlargement is, in all probability, *cancerous*: and if there be other traces of carcinoma in the system, this conclusion becomes almost certain. Small superficial hard irregularities betoken the *hobnail* liver; which is, sooner or later, accompanied by ascites. When, without pain or jaundice, the liver of a phthisical patient transgresses its natural boundaries, it is, presumably, a *fatty* liver, or a *waxy* liver.

By applying a similar method of investigation to other ventral enlargements, you may frequently hit the right scent, and trace the mischief to its true source. To treat the subject in detail would require a volume. I may refer you to a series of papers by Dr. Bright, in the 'Guy's Hospital Reports;' where you will find a host of examples, and much valuable information, concerning the most common and the most important kinds of 'abdominal tumours and intumescence.' And again to a series of excellent lectures in the 'British Medical Journal' by Sir William Jenner, on 'Extra-pelvic Tumours of the Abdomen.'

LECTURE LXXV.

Acute Rheumatism; Symptoms; Varieties; Treatment. Chronic Rheumatism; Phenomena; Plan of Cure.

Gout: Description of a Paroxysm; Progress of the Disease; general state of the Health in Gouty Persons; Causes of the Disease; Diagnosis between Gout and Rheumatism.

I PROCEED, this afternoon, to the consideration of that very common, very painful, and sometimes very perilous disease, *rheumatism.* There are two species of it, the acute and the chronic. They graduate, however, insensibly into each other; and the chronic is not seldom a sequel of the acute form. Yet this is not necessarily so. Chronic rheumatism occurs in persons who have had no preceding attack of the disorder in its acute stage or degree. Acute rheumatism often passes away at once, and leaves no legacy of the chronic.

Rheumatism implies inflammation: but, as I mentioned in an earlier part of the course, it is inflammation of a peculiar or specific kind. In the first place, it is inflammation of a particular tissue—the *fibrous tissue*: and it may therefore manifest itself wherever that tissue is engaged in the fabric of the body. No doubt the inflammation does involve other tissues also: but it is always, probably, by extending to them, through what has been called *contiguous sympathy.* Thus we have the *synovial* membrane of a joint inflamed in many cases, the inflammatory action having spread from the fibrous textures around the joint: or, as I formerly pointed out to you more in detail, the serous surface of the pericardium, and the serous surface (or what is analogous to a serous surface) of the inside of the heart, and especially that part of it which is carried over the valves—each and all of these serous membranes are extremely liable to be affected with inflammation in the acute form of rheumatism; but in all of them it is probable that the *fibrous* texture was the first to suffer. The pericardium is, as you know, a fibro-serous membrane; and fibrous tissue is interposed between the folds of the serous membrane, in the cardiac valves.

Rheumatism, therefore, is essentially inflammation of the fibrous tissue: and it most commonly seizes upon the fibrous parts that lie around the *larger joints*; the ligaments and the tendons: and with respect to this disease you may almost consider the perpetually moving *heart* as one of the large joints. Yet this inflammation, when confined to the fibrous tissues, is not *common* inflammation: or else (take which view you please) the fibrous tissues behave under inflammation in a manner peculiar to themselves. At any rate, the inflammation does not reckon among its events (as common inflammation does) either the effusion of plastic lymph, or suppuration, or gangrene. If suppuration sometimes occur (and it certainly occurs very rarely) it is because the rheumatismal inflammation has extended to contiguous textures, and then and there has run the ordinary course of inflammation. The *areolar* tissue around a joint may thus inflame and suppurate. The inflammation of the synovial membrane *may* be of sufficient intensity to give rise to the formation of pus. When, however, the inflammation extends to the serous tissues within and around the heart, the products of the inflammation are just the same as when inflammation of the same textures, of the common kind, is anyhow produced.

Acute rheumatism, then, consists in redness, heat, pain, and swelling (that is to say, in inflammation) of the parts lying around, or entering into the composition of, one or more of the larger joints of the body; generally of several at the same time, or in succession; with a disposition to shift from one joint to another, or to certain internal organs, and especially to the membranes of the heart; and with fever.

This tendency to shift its place—to what is usually called metastasis—is a very remarkable feature of the disease. The inflammation will appear, in one joint, suddenly, and as suddenly subside in another which it previously occupied: and then, perhaps, it will jump back again to its old quarters. In very many instances, however, it invades fresh joints without wholly ceasing, and sometimes even without diminishing at all, in those formerly affected. It may visit in this way every large joint in the body, and even seize upon some of the smaller ones; or it may possess nearly all of them at once. It is most commonly seen to affect the ankles and knees, the knuckles, wrists, and elbows. Less often it is seated in the shoulders also; and in the hips. The joints of the fingers frequently suffer; and I have seen more than one instance in which the joints of the jaws were manifestly implicated in the rheumatic inflammation. But by far the most serious and dangerous leap which the morbid process is apt to take, is to the membranes of the heart.

In truth, acute rheumatism is a blood disease. The circulating blood carries with it a poisonous material, which by virtue of some mutual or elective affinity, falls upon the fibrous tissues in particular, visiting and quitting them with a variableness that resembles caprice, but is ruled, no doubt, by definite laws, to us, as yet, unknown.

I pointed out to you, in a former lecture, the symptoms of rheumatic carditis, its consequences, the various ways in which it connects itself with the joint affection, and the treatment by which we are to attempt to moderate it. I dismiss, therefore, this the most momentous complication of acute rheumatism, from the present discussion; and shall confine myself solely to the disease as it manifests itself externally.

The shifting and migratory inflammation of the textures lying around or composing the larger joints, is ushered and attended with high inflammatory fever; with a remarkably full, bounding pulse; with flushed cheeks; head-ache; profuse, drenching, sour-smelling perspirations, which distress and weaken the patient, but bring no present relief to his pain; with a white-coloured, dirtyish, thick fur on the tongue, which is red, however, at its tip and edges; with turbid and acid urine. But this severe and inflammatory fever (*synocha* Cullen calls it) has no tendency to degenerate into a typhus-like form: and *that* is a striking feature in the complaint. Neither is the intellect affected, except when carditis takes place: and then, as I stated formerly, violent delirium is apt to ensue, misleading the practitioner, drawing his attention away from the chest, where grave and often fatal changes are in progress, and fixing it upon the head, where no inflammation at all exists, but which is disturbed through derangements of the cerebral circulation consequent upon the cardiac disorder. With this exception, we do not find patients in acute rheumatism *delirious*. Throughout all this febrile disturbance, there is no coma, no dulness or bewilderment of aspect, no marked trouble of the stomach or of the bowels, no vomiting, no diarrhœa, no petechiæ, no aphthæ, no sordes about the mouth, all which are of ordinary occurrence in the course of common continued fevers. The producing poisons, different in origin and in kind, differ no less in their effects upon the animal economy.

The joints are exquisitely *tender*, as well as painful. The fibrous tissues, which are endowed with but little sensibility in their sound and healthy state, become acutely painful when occupied by inflammation. The pain is increased by pressure;

and therefore by whatever implies pressure; by movements of the joints consequently. The patients are reduced to perfect helplessness by the pain; and lie fixed in one position, from which they are afraid to stir. Their common phrase is, that they have entirely lost the use of their limbs: and this is true enough in fact, but not true in the medical sense of those words. They have not lost the *power* of moving them; there is no *palsy*; but they *dare not* move them, because the effort gives them so much torment. They dread the touch of the physician, the handling of the nurse, the shaking of their bed by the footstep of an approaching friend.

As in other disorders which depend upon the presence of some imported or inbred poison within the body, so also in this—the constitutional symptoms generally precede the local. Nay, I believe that the fever may sometimes run its whole course without any manifest affection of the joints. I have never seen this: but Dr. Graves declares that he had known several instances of persons who, having undergone attacks of genuine acute rheumatism, did afterwards experience febrile symptoms exactly resembling in character, in intensity, and in duration, those which they had previously suffered, although from first to last not a single joint was inflamed.

Pain in the affected joints is more constant than swelling; and swelling more constant than redness. The swelling differs also in different cases in rather a remarkable manner. In fact, two varieties have been made of acute rheumatism. The distinction was first drawn by Dr. Chambers, at St. George's Hospital; and afterwards made public by Dr. Francis Hawkins in his Gulstonian lectures. The varieties are spoken of under the names of *fibrous or diffused* rheumatism: and *synovial* rheumatism. I will briefly state their distinctive characters.

In the one, then, the inflammation commences in the immediate neighbourhood of one of the larger joints: not *in* the joint, but *near* it. It attacks the tendons, fasciæ, ligaments, and possibly also the muscles. At first there is not much redness, or swelling; but after the pain has been of some duration, there is a puffiness around the parts affected, caused apparently by turgescence of the blood-vessels, and at length slight pitting, or *œdema*, may supervene, from effusion into the surrounding areolar tissue: and what redness is present is disposed in streaks, following the course of the tendons.

On the other hand, in the synovial variety, which shows itself more frequently and more plainly in the knee than anywhere else, the pain which marks the onset of the complaint does not last long before some degree of swelling is perceptible, together, in most instances, with slight redness of the skin; and this swelling is not due so much to turgescence of the blood-vessels, or to œdema of the areolar tissue, as to fluid poured into the *cavity* of the joint. And the form and character of the swelling indicate that it results from fulness and distention of the synovial membrane. It is tight and elastic, and protrudes, as it were, through the spaces that intervene between the tendons and ligaments by which it is in other parts bound down and restrained: and *fluctuation* is often distinctly perceptible in the superficial joints, when both hands are applied to them.

These are the *local* differences between the two forms of the disease. And there are differences equally well marked between the constitutional symptoms that attend them.

It is in that form which κατ' ἐξοχὴν is called *fibrous* rheumatism, that the inflammatory fever runs so high; that the tongue is so thickly furred; that the round, full, bounding pulse occurs; that the profuse, spontaneous, acid perspirations break out, which exhaust the patient's strength without alleviating his sufferings; that the urine is high-coloured, and deposits a copious sediment like brick-dust.

In the *synovial* form, the fever is either less intense from the beginning, or soon moderates after the joints begin to swell; the tongue is less foul; the patient sweats much less. It is to this form that the term *rheumatic gout* is often applied. And growing experience has led me to believe, that in this popular appellation the real nature of the complaint is most truly expressed. Gout and rheumatism are very similar in kind: and what has been called synovial rheumatism, while it forms a connecting link between the two, and partakes of the characters of both, is more nearly allied to gout than it is to rheumatism.

Conformably with this belief, it has been noticed that the tendency of the inflammation to settle upon the cardiac membranes is much greater in the fibrous than in the synovial disease. This is a most important difference.

I know of no other exciting cause of acute rheumatism than exposure to cold, and especially to cold combined with moisture. And this is the reason why the disease is very common among the poorer classes of society, who are more in the way of that cause, and cannot guard against it so effectually as their wealthier brethren; among whom it is comparatively rare. The cold probably exercises its injurious influence by checking the elimination, through the skin and other emunctories, of the poisonous principle as it forms, and by thus heaping it up in the blood.

This poison in the blood constitutes that predisposition to the disease, without which it would never occur. In the absence of the poisonous material, no exposure to wet and cold will produce acute rheumatism; but out of its abundant presence acute rheumatism may and often does arise, independently of exposure to atmospheric vicissitudes, or of any other possible exciting cause. We see it break out in hospital patients who have for some time been protected from such exposure. The predisposition appears to be sometimes inherited. Dr. Fuller tells us that he traced this hereditary character in twenty-nine per cent. of the rheumatic patients admitted into St. George's Hospital. The poison itself is probably a product of unhealthy assimilation.

Acute rheumatism is principally a disease of youth: prevailing most, I believe, from the age of puberty to that of thirty-five or forty. I have repeatedly, however, seen it in children; sometimes as early as the third or fourth year: and I stated to you some time ago, that the chance of the joint affection being complicated with rheumatic carditis is the greater in proportion as the patient is the younger. With, perhaps, two exceptions, I never knew the disease occur in an unequivocal form before puberty, without its being attended with inflammation of the lining or of the investing membrane of the heart.

I have already told you what I believe to be the proper plan of treatment to pursue when rheumatic carditis is present: in that case the affection of the limbs is of secondary consequence. But what are we to do when there is no complication of the joint disease; no invasion of any of the viscera?

Why, if you seek for instruction upon this matter in books, or even among practical men, you will meet with a very perplexing diversity of opinion. Apart from the cardiac affection, acute rheumatism has no *danger* about it; and the articular inflammation almost always terminates, sooner or later, in recovery, whether the heart be implicated or not. And most persons who have been for any considerable time in practice have their own favourite method of conducting the disorder to its termination. While many have employed free blood-letting, and other active antiphlogistic remedies, some, on the contrary, even in the present day, put their trust in bark. Some have given large doses of calomel in the outset of the disease, such as half a scruple or a scruple, with or without a grain or two of opium; repeating the dose daily, or oftener, with occasional purgatives perhaps, till the urgent symptoms gave way; and in this manner I have seen the disease apparently cut short. But I have also known many instances in which the disease was painful, and protracted, and obstinate, although this practice was adopted early and fairly prosecuted. Some physicians, again, give smaller and more frequent doses of calomel and opium: and some think opium alone to be as useful as this combination. Others depend mainly upon colchicum: others upon large and repeated doses of conium: and some attempt the cure of acute rheumatism through sweating the patient by means of guaiacum, and similar stimulant medicines, and a profusion of bed-clothes. Lastly, some physicians of high and deserved repute, forswear drugs altogether in the treatment of this malady.

Now you may be sure—when men's opinions concerning the treatment of a disease which is of common occurrence and easy recognition, are thus unsettled and diverse—you may be sure, first, that no specific for that disease has yet been discovered; and secondly, that the disease is not very obedient, or not steadily obedient, to any remedial plan. When first I began to practise, I pleased myself, now and then, with the belief that I had ascertained the best cure for acute rheumatism; so rapidly and decidedly did the disorder recede and cease upon the administration of such or such a remedy. But, on the next trial of it perhaps, my expectations have been miserably disappointed. This marked improvement has happened under the use of colchicum, of conium, of calomel with opium, of alkalies. I did not, in the prosperous cases, mistake spontaneous recovery for cure. The change was too great and immediate, and the instances of success were too numerous, to admit of that explanation. Whether it be (as I suspect) that slighter diagnostic marks have been overlooked, and that sometimes *gout* has in reality been cured under the semblance of rheumatism—whether bodily idiosyncrasies have withstood the influence of remedies—or whether atmospheric agencies have kept up the disorder in spite of proper treatment—I cannot tell: certain it is that we are occasionally baffled, and the patient continues to suffer, notwithstanding the diligent enforcement of all the approved remedies and plans of treatment, one after the other. I am far, however, from thinking that remedies are useless; and I do by no means assent to the dictum of the first Dr. Warren, who, when asked what was good for acute rheumatism, replied, 'Six weeks.'

One principle to be kept in view in these cases may be laid down broadly and decidedly. That practice must be the best which tends most surely to obviate or to lessen the risk of cardiac complication. Now I believe it to be quite true that the plan of treatment which is most calculated to moderate and to bring to an end the uncomplicated disease, is also the plan of treatment which is the best adapted to the paramount object of keeping it uncomplicated.

It is very difficult to obtain authentic experience as to the effect of treatment in preventing the cardiac complication; or in assuaging its intensity if already present. The whole subject is closely beset with sources of fallacy. Upon some points most persons are now agreed. The parts affected by the rheumatic inflammation must be kept *at rest.* This dogma, you know, applies to all inflammations alike. Now the joints may be kept at rest by confining the patient strictly to his bed. The pain produced by attempts to move will help to enforce obedience to this rule: and, in a certain sense and degree, the perpetually moving heart will obtain repose in this way; will be protected against whatever increase of its normal action might be occasioned by even slight bodily exertion. Another approved rule is that the *temperature* of the patient's surroundings should be made as *equable* as possible. To this end he should lie wrapt in a blanket, or in a flannel dress; for linen, wet with perspiration, readily becomes cold, and likely to chill the body. Again, during the height of the fever the patient should abstain altogether from solid animal food.

Drs. Gull and Sutton, after carefully watching a certain number of cases, treated almost solely on this 'expectant plan' of absolute rest, equable external temperature, and regulated diet, have come to these most important conclusions: that the tendency to heart affection in this disease is strongest during the first few days of the fever; that if the heart escape during the early days, there is every day a lessening tendency to its implication; and that if at the end of the first week the heart remain free, there is little or no tendency to its becoming diseased during the later weeks, provided the patient is treated by rest and regulated diet.

If this be so, it is manifest that we may easily deceive ourselves as to the apparent efficacy of drugs in warding off the cardiac complication. Our treatment, seemingly so successful, may have been pursued when there was probably no tendency to the heart disease.

I strongly advise you to adopt, in all its rigour, the plan I have now been describing; but I do not advise you to be content with these 'expectant' measures, which consist simply in allowing the disease to run its natural course under the most favourable outward circumstances.

The acid properties of the perspiration, as manifested even by its peculiar smell—of the saliva, as tested by litmus paper—of the urine, as shown by its deposits, warrant the hypothesis that the poison which the whole disorder would seem to be an effort to discharge from the blood is some sort of *acid*. Dr. Prout conjectured that the phenomena of acute rheumatism might depend upon the presence in the blood of *lactic* acid; and some very remarkable experiments made by Dr. Richardson lend weight and likelihood to this conjecture. Into the peritoneum of a healthy cat he introduced a solution of lactic acid in water. In two hours the action of the cat's heart became irregular. The next morning the animal was found dead. There was no peritoneal inflammation; but marked endocarditis in the left chambers of the heart. The mitral valve was inflamed and thickened, and covered on its free borders with firm fibrinous deposits. The whole inner surface of the ventricle was highly vascular. A dog, on which a similar experiment was tried, died in two days. Unequivocal evidence of endocarditis was disclosed upon examination of the heart. The tricuspid valve was swollen to twice its ordinary size. The aortic valves, inflamed and enlarged, presented fibrinous beads along their edges; and the entire endocardial surface was red. The pericardium was simply dry. There was, however, no affection of the joints.

Upon this hypothesis, then, *alkaline* drugs would appear to be chemical antagonists of the poison; and they have the further good effect of hastening its ejection through a natural emunctory, inasmuch as they are most of them diuretic. Accordingly the treatment of acute rheumatism by alkalies is also, and long has been, a common and a favourite treatment among physicians. Nitre in large doses has been much used and commended; and the alkaline carbonates are very generally prescribed. The bicarbonate of potash in solution has been largely and fairly tried by Dr. Garrod, who has administered it in the average quantity of two scruples, repeated every two hours, by night and by day, for several days together. Of 51 cases so treated, the average duration under treatment was between six and seven days, and the average duration of the whole disease was between thirteen and fourteen days. The medicine soon rendered the urine alkaline. It had no injurious influence on the bowels, or on the bladder. It seemed rapidly to calm the pulse, and to allay the febrile heat. In no instance did any affection of the heart arise after the patient had been forty-eight hours under its influence.

It is an additional recommendation of alkaline remedies in this disease, that they hinder (if we may trust the medical chemists) the deposit of the fibrin of the blood, and may therefore be regarded as so far tending to prevent the formation of endocardial vegetations. The alkaline carbonates may be prescribed in the pleasant form of an effervescing draught, containing an excess of 30 or 40 grains of the carbonate ; the dose being repeated every three or four hours while the fever runs high, and the joints are swollen and painful.

The favourite drug of the late Dr. Golding Bird in this and in some other blood-diseases, was the *acetate of potass.* This intelligent physician was himself more than once the subject of acute rheumatism, and his testimony to the efficacy of the acetate is so strong and direct that I cannot forbear quoting it. 'I would not willingly (he says) use language which was not completely compatible with experience, but I do not still hesitate to declare that I have never seen the disease in question yield with so much facility to any other remedy. In the severest cases which have been admitted into the hospital under my care, I have seen the cure to be more rapid, and the immediate relief to the patient more marked, by the use of the acetate of potass in quantities of half an ounce, administered, largely diluted, in divided doses, in twenty-four hours, than by any other treatment. In three days I have repeatedly found the exquisite pain of the joints nearly absent, the patient comparatively comfortable, and able to bear with greater ease the helpless state in which the still swollen joints place him. In no case has any ill effect followed the use of the remedy, and whilst the cure has been far more expeditious, the ill effects of colchicum and mercury have been avoided. The pain remarkably and suddenly lessens as soon as the urine becomes alkaline and rises in specific gravity. I can indeed unhappily attest my experience in my own person on the marked alleviation and rapid cessation of the pains of rheumatic fever from the use of the drug, and can gratefully compare its influence with the tedious and painful results of mercurial treatment in a former attack. It is difficult to decide on the comparative immunity from pericarditis in acute rheumatism under particular modes of treatment, but the impression on my mind is very deep, that the tendency to this fearful complication is very much lessened as soon as the urine is rendered alkaline by the acetate.' He was in the habit of administering it 'in some aromatic water, or what is far more grateful, in plain water, to which a few drops of oil of lemons have been added.'

Alkalies, then, or the alkaline salts, are always, in my opinion, fit remedies to be employed in the treatment of acute rheumatism. They may be added largely to the common effervescing saline draught, or they may be simply dissolved in water. I have been assured by Dr. Fuller, whose experience of rheumatic fever has been very considerable, that he never finds the heart to become affected if he can first make the urine alkaline.

For settling this momentous therapeutic question upon a trustworthy basis, multiplied observations are plainly requisite to determine two things: 1. Whether the law suggested to Drs. Gull and Sutton by a few cases only—namely, the cessation of the tendency to cardiac complication after the first few days—be a general law: and, 2. Whether the experience respecting the immunity from such complication of patients whose urine has been made alkaline, relates to the early or to the later days of the disease.

It may be needful to clear the bowels once for all by a purgative; but repeated purgation is clearly undesirable, involving, as it must, frequent breaches of the great rule of rest, increase of pain in the affected joints, and exposure to the risks of cold.

Of external remedies in relief of the articular *pains*, less use has been made than might well have been made. The affected joints rarely sustain any permanent damage. The inflammation is apt to leave them of its own accord, even suddenly, and sometimes as suddenly to return. Leeches, or cupping-glasses, would seem therefore needless, or superfluous. Repulsion of the poison which is making its exit through a joint, back again into the circulating blood by cold applications—to settle possibly upon some internal part, and especially upon the heart—would be positively hazardous. No peril, however, of this sort belongs to *warm* fomentations, which often afford exceeding comfort. Both Dr. Basham and Dr. Fuller have fairly ascertained their safety and their value. Theory suggested that the lurking or emerging poison might be neutralized and rendered innocuous by making these fomentations *alkaline*, and experience appears to confirm this notion. I may quote Dr. Fuller's remarks on this subject. Warm fomentations 'soothe the parts, promote perspiration, and thereby favour the elimination of the poison. As the object of the application is to allay the pain, and to counteract the extreme acidity which always accompanies if it be not the cause of rheumatic inflammation, it is manifest, theoretically at least, that an alkaline and opiate solution should prove the most effectual remedy. And so in practice it is found to be. I have tried hot water; I have tried a warm solution of nitrate of potash, as recommended by Dr. Basham; I have tried a simple alkaline solution; and I have tried a mixed alkaline and opiate solution, and the latter has proved far the most powerful in allaying the pain of rheumatic inflammation. In every instance in which it has been employed, the relief obtained has been almost immediate. In order to guard against any source of fallacy, I selected fourteen instances in which corresponding joints were affected, and applied a fomentation of warm water to the one joint, and an alkaline and opiate solution to the other, and almost uniformly the pain and inflammation continued in the former, and speedily subsided in the latter.'

The solution usually employed by Dr. Fuller is made by dissolving half an ounce, or six drachms, of the carbonate of potass, or of soda, in nine ounces of hot water, and adding six fluid drachms of Battley's *liquor opii sedativus*. Thin flannel, soaked in this hot lotion, is applied to the inflamed joints, and the whole is wrapped in a covering of thin gutta percha.

Even when the joints are not very painful, and this sedulous fomentation is not absolutely necessary, nor perhaps practicable, they should severally be enveloped in dry cotton wool.

Dr. Herbert Davies has of late advocated the application of blisters to the affected joints. He finds the pain and the swelling to be sensibly and speedily relieved in this way. I am unable to tell you anything of this practice from my own experience. After all, should the pain resist these local measures, recourse must be had to opiates; and the best way of employing them is probably the subcutaneous introduction of some salt of morphia. With respect to the synovial form of acute rheumatism, as the risk of the inflammation falling upon the heart is but slight, the necessity for a rigid adherence to rest and flannel is less imperative. In this form the preparations of colchicum have sometimes an almost magical effect. Large doses are not requisite. Twenty minims of the wine or of the tincture may be given every six hours, until some result is obtained. Or a grain of the inspissated juice, or of the acetous extract of colchicum, every four hours. Under this treatment the disease sometimes vanishes within three or four days; the medicine producing sickness and purging, and the rheumatism, or the rheumatic gout, rapidly declining. Occasionally the same favourable event takes place, although there has been no disturbance of the stomach or bowels.

There are two kinds of *chronic* rheumatism: one attended with local heat and swelling, although the constitution at large sympathises very little or not at all with the topical inflammation; the other characterised rather by coldness and stiffness of the painful joints. In the former of these the pains are increased by pressure, and by movements of the limbs, and by external warmth; the warmth of a bed, for example; and there may be even some slight degree of pyrexia at night. In truth this form of chronic rheumatism claims a near relationship with the acute, *into* which it sometimes passes, and *of* which it is frequently the sequel. It is important for you to know that, in these cases, you may, with less hesitation, less fear I mean of driving the inflammation to some more vital part, apply leeches, and cold washes, to the painful joints. The complaint is often obstinate and lingering, and prone to recur. It frequently involves and cripples the smaller joints, especially those of the knuckles and fingers; rendering them knobby, and distorting their form and position. The fingers take a permanently oblique direction, slanting outwards towards the ulna: and Dr. William Budd has drawn attention to the curious fact that the corresponding joints of the two sides of the body are always affected exactly in the same manner. To use a paradoxical expression, the deformity is symmetrical. One crooked joint is just the copy of its fellow. Surely this indicates the *constitutional* origin of the disorder; the infection of the *blood*.

In the other form of chronic rheumatism, what some call *passive*, the remedies that answer best are of a different kind. The pain is alleviated by friction of the joint, and the patients are most comfortable when they are warm in bed, and especially when moderate perspiration is present. They are singularly benefited also by summer weather. Persons who are much troubled by this wearing complaint, and who can afford to live where they please, would do well to take up their residence in a warm climate. Wherever they may be, such patients should be protected against atmospheric vicissitudes by warm clothing: they should be cased in flannel from the neck downwards. Warm bathing is of great service; and especially baths of salt water, of a temperature not less than 100°, that they may act as a stimulus to the cutaneous circulation; warm douches; the vapour bath; or the hot-air bath, of which, as I have said before, the patient may receive the benefit as he lies in bed. And to warm clothing, and warm bathing, may be added friction with some stimulating liniment, and what is called shampooing. It is in these cases that stimulating internal medicines are often of use. Turpentine; some of the animal oils, the cod's-liver oil for instance; guaiacum. Opiates, too, are frequently remedial of the pain; and there can be no better form for their administration than that presented to us in the celebrated Dover's powder; the *pulvis ipecacuanhæ compositus* of the Pharmacopœia.

Whatever its value may be as a remedy for acute rheumatism, the iodide of potassium is certainly available for the relief of the chronic disorder. It is *most* sure to act beneficially when that fibrous part, the *periosteum*, is principally affected. Its virtues in the case of venereal nodes (*i.e.* in venereal inflammation of the periosteum) were first distinctly pointed out by Dr. Robert Williams, of St. Thomas's Hospital. I believe it is equally effectual, upon whatever cause chronic inflammation of the same part, with nodes and thickenings, may depend.

Some of you probably saw a woman who was lately my patient in the hospital, and who had been worn down to a skeleton by the pain she had endured from chronic periostitis giving rise to nodes, which did not appear to be traceable to syphilis. She had been in the habit of lulling the pain by large opiates at night, and begged to have them after her admission. I gave only the iodide in the ordinary dose (five grains thrice daily), and she slept without opium; and in a week or two lost her nodes, and was perfectly well.

Closely allied to acute *rheumatism*, and yet perfectly distinct from it, is the singular disease which in this country is popularly called the *gout*: which Cullen, in the first instance, was disposed to term arthritis, but as arthritis would imply inflammation of all or any of the joints, and as the primary and favourite seat of gouty inflammation is in the great toe, he afterwards adopted the ancient name of *podagra* (foot-pain).

The same author has given, in his 'First Lines,' an excellent account of the phenomena which constitute a paroxysm of gout. It is copied from Sydenham, who drew from nature, for he had himself suffered frequent and severe visitations of the disease during a period of thirty-four years.

The attack begins, most commonly, an hour or two after midnight. The patient, who may have gone to bed and to sleep in his usual health, and without suspecting what was about to happen, is awakened by a pain in one of his feet, mostly in the first joint or *ball* of the great toe; but sometimes in other parts of the foot—the heel, the instep, the ankle. With the coming on of this pain there is generally more or less of cold shivering, which gradually ceases as the pain gets worse, and is succeeded by heat. The pain grows more and more violent and intolerable; and is spoken of by those who suffer it as amounting to torture. It is a grinding, crushing, wrenching pain; or a burning sensation as if a hot iron were pressed into the joint. Some humorous Frenchman described it in this way. 'Place (said he) your joint in a vice, and screw the vice up until you can endure it no longer. That may represent rheumatism. Then give the instrument another twist, and you will obtain a notion of the gout.' The pain is attended with great restlessness and misery, and exquisite tenderness. The patient cannot bear the weight of the bed-clothes upon the affected limb; nor the jar of a heavy foot-fall in his chamber. In a vain search after comfort he is perpetually shifting his foot from place to place, and from posture to posture. At length, about the ensuing midnight, the pain remits; sometimes gradually, sometimes so suddenly that the patient attributes the relief to his having at last found an easy position. He falls asleep in a gentle perspiration, and when he awakes the next morning he finds the part, which had been so painful, to be red, swelled, tense, and shining, surrounded by more or less œdema, and by turgid veins. The same series of symptoms recur, in a mitigated degree, for some days and nights; and then the disease often goes entirely off, not to return till after a long interval.

As the œdema subsides, and the redness fades, the cuticle of the part that has been inflamed peels off; and this process of desquamation is generally attended with troublesome itching.

Such is a picture of an attack of gout, occurring in an adult subject, for the first time, and in its most regular and genuine form.

Attacks of this kind are preceded, in most instances, by some marked disorder of the functions of the stomach; diminished appetite, flatulence, heartburn, nausea perhaps. And during the paroxysm the urine is often, but not always, dark-coloured, acid, scanty, and turbid; depositing copiously a pink, or brick-dust sediment. The stools, also, are unnatural; pale, or of a dark green, and very offensive. After the fit, when the complaint has ceased entirely, it generally (says Cullen) 'leaves the person in very perfect health; enjoying greater ease and alacrity in the functions of both body and mind than he had for a long time before experienced.'

But the disorder, which has thus departed, is very apt—nay, unless extreme care be taken to prevent it, and even in spite of all care, it is almost sure—to return. At first, perhaps, it recurs not oftener than once in every three or four years; but after some time the intervals are shorter, and the attacks become annual, happening about the same time of the year: afterwards they come twice every year; and at length they return several times during the course of the autumn, winter, and spring. And as the fits are more frequent, so also are they more protracted, till, in the advanced state of the disease, the patient is hardly ever free from it, except perhaps for two or three months in summer. I do not mean that all this occurs invariably in all cases alike; but this is a sketch of the general course of the complaint.

There are other phenomena also to be noticed as time goes on, and as the disease is repeated. At first, I say, it commonly appears in one foot only; afterwards every fit includes both feet, the one after the other; and as the disease continues to recur, it not only attacks both feet in succession, but after having ceased in the foot which was secondly visited, it will return again into the foot first affected, and perhaps a second time also into the other. It passes, too, into other joints, both of the upper and lower extremities, large as well as small; so that there is scarcely a joint that may not, at one time or another, be seized upon. But as the disease proceeds, and the fits get to be more numerous, the pains are commonly less violent than they were at first; the patient is, however, more affected with sickness, and suffers more in his general health.

Again, after the earlier attacks, the joints usually recover entirely their former strength and pliancy; but when the disorder has returned again and again, they are not so readily nor so completely restored to their previous condition, but remain weak and stiff: and sometimes they lose at length their capacity of motion altogether.

Also, in some gouty persons, but not in all, after the disease has frequently recurred, what are called *chalk-stones* appear; concretions that look exactly like chalk collect around and outside the joint, filling up the areolar tissue, and lying, in general, immediately beneath the skin; or even sometimes bare of skin, having made their way through it. A namesake of mine, Mr. Henry Watson, describes, in the first volume of the 'Medical Communications,' the case of a Mr. Middleton, who was accustomed, when playing at cards, to chalk or score the game upon the table with his gouty knuckles. The material of these curious concretions is deposited at first in a half fluid state, and resembles cream or soft mortar; but the more watery ingredients being afterwards absorbed, it becomes dry and hard. Of course when this stuff is deposited in any quantity on the outside of a joint, it must limit to some extent, or it may entirely prevent, the motion of that joint. The concretions consist mainly and essentially of the urate of soda, in a crystalline form. Another, and an odd place, in which these deposits are extremely common in gouty persons, is the cartilage of the external ear, usually upon, or just within its helix. They have a high, because a pathognomonic, value; for they furnish visible and incontestable evidence of the presence of the *gouty diathesis.*

Gout is a disease that was well known, and well observed, in its outward manifestations, by the ancients. In its genuine form it could neither be overlooked nor mistaken. Many very interesting facts relative to this painful disorder have accordingly from time to time been ascertained: and I proceed to notice the chief of these; but I must do so with as much brevity as I can.

First, then, gout is a *hereditary* disease. I do not mean to say that the disposition to it is always a transmitted disposition; but that the complaint is much more likely to occur in persons in whose pedigree it can be traced, than it is in other persons. It may, no doubt, be generated by certain habits of life; and, on the other hand, in spite of an inherited predisposition, the disease may be staved off or averted. Let the son of a rich and gouty nobleman change places with the son of a farm-servant, and earn his temperate meal by the daily sweat of his brow; and the chance of his being visited with gout will be very small. Granting this, we see a reason, independent of the general analogy of hereditary disorders, why the gout may be expected sometimes to leap over a generation, just as family likenesses are known to intermit; while yet the *disposition* may descend to the children of those who, in their own persons, have never suffered the *disease*. Among 522 gouty persons, concerning whom Sir Charles Scudamore had collected information, 332 could trace their disease to the father, mother, grandfather, grandmother, uncle, or aunt. In the remaining 190 the disease was not known to have existed in either upward branch of the family-tree.

2. There is a pattern of body which is believed to be favourable to the acquisition of gout. 'It attacks especially (says Cullen) men of robust and large bodies, men of large heads, of full and corpulent habits, and men whose skins are covered with a thicker *rete mucosum*, which gives a coarser surface.'

3. Whether, in a given individual, there be an inherited tendency to the disorder or not, its access is promoted in a remarkable manner by a full and luxurious mode of life, and by sedentary or inactive habits.

4. It is observed of gouty persons, that they are usually subject to nephritic complaints also, to fits of the gravel, to renal and vesical calculi. These disorders of the urinary organs commonly begin to manifest themselves after the gout has plagued the patient for some time. They do not coincide with the paroxysms of gout, but the two happen alternately: or (what is equally expressive of the connexion between the two forms of disease) the children of gouty and nephritic parents inherit often the one or the other of these maladies; but 'whichever may have been the principal disease of the parent, some of the children have the one, and some the other. In some of them the nephritic affection occurs alone, without any gout; and this frequently happens in the *female* offspring of gouty ancestors.'

The urinary concretions to which gouty people are so subject, and the morbid states of their urine generally, belong to the *lithic* diathesis. Dr. Prout held that 'the lithic or uric acid, developed principally during the mal-assimilation of the albuminous textures, may be considered as the characteristic feature in gout.' And the chemical composition of the *chalk-stones* which sometimes accompany gout, is in accordance with this statement; and illustrates strongly the connexion between gout and gravel. The so-called chalk-stones are chiefly composed, as I have said before, of uric acid combined with soda; of the urate of soda. Sometimes this very urate of soda, perfectly white, is deposited in large quantities in the *urine*. Dr. Prout had seen it copiously secreted of the consistence of mortar, so as to block up the urethra in its passage outwards. Now this is just the stuff which is deposited around the joints, and which hardens and crystallizes as it collects. I may mention here again that many persons have the gout long, and severely, without having any of these concretions. They are incidental to the more chronic forms of the disorder, in which the pain and the fever, though of long duration and frequent recurrence, are slight in degree.

5. Gout attacks especially the male *sex*. Some few women, however, suffer it, in its regular and decided form; and generally these women are robust and plethoric. Cullen noticed its occurrence in 'several females whose menstrual evacuations were more abundant than usual.' But in women the disease chiefly happens after the catamenia have ceased to appear. Heberden knew a woman who had numerous sores from chalk-stones.

6. Cullen observes that the gout does not usually come on till after the *age* of five and thirty. Heberden, who in his long and extensive practice among the higher classes of society in this town saw as much, perhaps, of this disease as any physician ever did, says that he never met with a case which he could decidedly pronounce to be gout, before the age of puberty. Sir Charles Scudamore has collected a statistical account of 515 examples of gout, in which the period of the first assault had been noted. Of these, 142 began between the ages of 20 and 30; 194 between 30 and 40; and 118 between 40 and 50. The greater number, you will observe, was between 30 and 40.

However, I believe that where the inherited disposition is strong, and the habits of living are such as to foster that disposition, gout may show itself, occasionally, even prior to the age of puberty: but this is, certainly, the exception to a very general rule.

7. Gouty persons are subject to various ailments, which spring from the same fountain as the well-marked paroxysm: derangements in the functions of the digestive organs; of the heart and lungs; of the brain and nerves.

The most familiar of these ailments is indigestion, with its various circumstances of impaired appetite, sickness, vomiting, flatulency, heartburn, acid eructations, gastrodynia. Pains and cramps occur in several parts of the trunk, and shoot thence into the upper extremities, and are relieved upon the extrication of wind from the stomach. The bowels are irregular; colicky diarrhœa being sometimes the prevailing fault, but more commonly costiveness. With all this the patient is apt to be excessively dejected, irritable, and hypochondriacal, morbidly attentive to every bodily feeling, disposed to exaggerate his sufferings, and apprehensive of the worst event.

When the viscera of the thorax are affected, the patient has palpitations, fits of dyspnœa, faintings, or even pangs like those of angina.

In the head occur pain, giddiness, transient affections of the vision and of the hearing, threatenings of palsy and apoplexy.

All these, you may say, are feelings and ailments to which any and all persons are liable. True: but the remarkable peculiarity which connects them, in some men, with gout is this;—that they often all clear away and disappear upon the breaking out of a paroxysm of that disease in the foot.

Hence such symptoms are regarded as indicating one variety of *irregular* gout. Cullen, led by a questionable theory, classes them under the head of *atonic* gout. Sometimes the patient so affected is said to have *lurking* gout; or *masked* gout.

In another variety of irregular gout, the complaint commences, in the ordinary way, in a joint; but the pain and inflammation do not reach the ordinary degree of intensity, or at any rate do not continue for the usual time and then recede gradually in the accustomed manner, but they disappear abruptly and entirely, while symptoms of severe and alarming disorder arise, as suddenly, in some internal part. This Cullen names *retrocedent* gout. It affords an example, as I conceive, of true metastasis. The internal part most commonly attacked is the stomach. It becomes affected with a peculiar feeling of anxiety and distress; with sickness, vomiting, or violent pain which the patient calls spasm, and which perhaps *is* of a spasmodic character. More rarely the retrocession is to the heart, when syncope or urgent dyspnœa ensue; or to the head, when it may terminate in a stroke of apoplexy, or of paralysis.

In a few cases the disorder, thus alighting on some other part than a joint, is plainly inflammatory. The most common example of this is gouty inflammation of the urethra, with scalding and a puriform discharge; simulating very exactly an attack of gonorrhœa. So also there is a gouty form of ophthalmia, or gout in the eye; gout in the testicle; and some years ago an eminent physician of my acquaintance suffered a violent and dangerous attack of what was considered to be gout in the throat. Dr. Cullen speaks of these inflammatory affections under the title of *misplaced* gout: but they may well enough be ranked under one of the preceding heads of masked, or of retrocedent gout.

The disposition to gout may be engendered, and when inherited will infallibly be strengthened and developed, as I have told you already, by certain habits of life: by sensual indulgences, and (but in a less degree, I believe) by want of bodily exercise. Of this we have the strongest negative evidence in the remarkable immunity from the disease enjoyed by the working poor in our rural districts. One never hears of the gout among agricultural labourers. Sir Gilbert Blane states that, during ten years in which he was physician to St. Thomas's Hospital, although in his private practice he reckoned 130 patients who had gout, being about one in twenty-six of the whole number, he had not a single case of it among 2,406 patients in the hospital. This I think strange, for in the London hospitals it is not very uncommon for us to meet with gout; but then it is in persons who have lived fully and inactively: in the servants of wealthy families for instance, butlers, coachmen, porters—men who often live more luxuriously, and more idly a great deal, than their masters. And among the rich, those who are most subject to gout are notoriously those who indulge most in what are called the pleasures of the table; who eat largely of animal food, and drink much wine; especially if they are indolent withal. Such men generate for themselves the lithic acid diathesis; and if the gouty tendency happen to have been born with them, they incur the disease, under these habits, with more or less readiness, according to the degree of that innate disposition. Strong exercise certainly obviates, in some measure, the evil effects of this mode of life, by promoting the excretions of the body: but gout used to be exceedingly common in the old-fashioned fox-hunter, who 'rode hard,' while he also 'lived hard.' Mere sedentary habits do not produce gout, as we learn from the comparative exemption of women; and of the poor, who, following sedentary employments, are yet compelled by their poverty, which is so far a blessing to them, to be temperate. Men who eat much meat, generally indulge themselves in drinking also: the two causes go together, and it is difficult to estimate their separate influence. Butchers, who live fully upon animal diet, are said to be rarely affected with gout, but then they necessarily take a great deal of exercise. It appears that the use of wine, and of malt liquors, fosters the disposition to gout much more than the abuse of distilled spirits. The paucity of gouty patients among the lower classes in this gin-drinking town suffices to show this. I have been told that gout is very little known in Glasgow, where the commercial men live richly, and lead sedentary lives, but do not drink much wine, their favourite beverage being rum-punch, of which they are not at all sparing. Dr. William Budd says that the disease is common among the 'ballasters' on the Thames; that, although they are not a numerous body, many are admitted with gout every year into the *Dreadnought.* Now these men being much exposed to inclemencies of weather, and using great bodily exertion, which is attended with profuse sweating and much exhaustion, think themselves warranted in drinking (besides spirits) two or three gallons of porter daily. This shows the effect of malt liquor in producing the gouty habit of body.

On the other hand, the inbred gouty tendency may be so strong as to be scarcely kept in check by the most abstemious regimen.

A fit of the gout may be *brought on* by various circumstances; in other words, the possible *exciting* causes of gout are many. A paroxysm has been frequently known to follow immediately upon an unusually severe debauch. Strong mental emotion has sometimes the same consequence, especially emotion of a depressing kind. Excessive fatigue—more particularly fatigue produced by too much walking exercise on any one day—is another exciting cause. And this is unlucky, for it is apt to discourage a patient from again making use of a proper and even a necessary amount of exercise of that kind. Another exciting cause which frequently operates is external injury. The first attack of gout often fixes upon the seat of an old hurt: and a very slight recent injury is sometimes enough to determine a paroxysm—a trifling bruise, or sprain, the pressure of a tight shoe; nay, Dr. Heberden tells us that he verily believes he has seen an attack of gout brought on by the bite of a flea; showing how easily the disease may be excited, when there is a strong predisposition to it. This it is which makes us so often doubt the accuracy of gouty persons, when they tell us that they are lame from a mere sprain.

Dr. Cullen enumerates sundry debilitating circumstances, which, as such, appear to operate in calling into action the gouty disposition. And there can be no doubt that a state of weakness does often favour the eruption of the malady. A friend of my own had lately a most serious attack of continued fever, in the course of which he became hemiplegic, and his life was despaired of. Soon after the fever had left him, and while he was yet extremely feeble, he had three attacks of gout in quick succession.

Pains have been taken by several writers, especially by Heberden, to lay down the distinguishing characters between gout and rheumatism. A first assault of gout can scarcely be confounded with an attack of acute rheumatism. The limitation of the inflammatory redness to one foot, and the restless distress of the gouty patient, contrast strongly with the helpless and motionless condition of the rheumatic, who is pinioned, so to speak, in many limbs. There may be more room for doubt and mistake in the advanced state of gout, when many joints have at length become involved; but even then you may generally decide by enquiring into the history of the patient, and learning the circumstances of his early attacks.

The main points of distinction may be broadly and generally stated thus.

In gout the small joints are first and chiefly affected, especially the joint of the great toe: in rheumatism, the large. The redness of the gouty inflammation is more bright and vivid than that of the rheumatic; and the fluctuations between agony and ease are more complete and more frequent. Gout usually affects one joint only at a time: rheumatism usually several at once. The inflammation in gout is attended with turgid veins, and with more œdema than in rheumatism; and is followed, in the majority of instances, by desquamation and itching, phenomena which we do not notice at the close of rheumatic inflammation. Gout is not attended with those drenching acid sweats which are so characteristic of acute fibrous rheumatism. The gout is decidedly hereditary: rheumatism, though probably hereditary too, is much less distinctly so. The gout occurs rarely or never, whereas rheumatism is not very uncommon, before the age of puberty. In gout, though many functions suffer, and especially the digestive functions, there is no tendency to carditis: in rheumatism, with far less general disturbance, but more fever, that tendency is very marked. Gout is often, rheumatism is never, associated with chalk-stones: and conformably with this distinction, Dr. Garrod has taught us that uric acid in excess is present in the blood of gouty, and not present in that of rheumatic patients. Gout is the punishment (some have thought it the privilege) of the rich, of persons who live fully, luxuriously, and indolently: rheumatism is most frequently the appanage of the poor, and of those who toil.

LECTURE LXXVI.

Pathology of Gout. Prognosis. Prejudices respecting the disease. Treatment: during the paroxysms; during the intervals. Cutaneous Diseases.

I YESTERDAY described the phenomena of gout, from its primary outbreak to its crippling consummation. I told you what observation has collected concerning its causes; and I pointed out the circumstances which distinguish it from rheumatism. Let us look a little closer into the essence of this curious malady.

The pathology of gout has been the theme of endless controversy. Humoralists and solidists contend alike for the triumph of including the disease within the pale of their respective theories. The very name, *gout*, derived through the French *goutte* from the Latin *gutta*, expresses summarily the doctrine of those who imposed it: and we trace the same, or a similar idea, in the appellation of the kindred disorder, *rheuma*tism.

'The opinion (says Cullen) which has generally prevailed, is, that gout depends upon a certain morbific matter, always present in the body; and that this matter, by certain causes thrown upon the joints or other parts, produces the several phenomena of the disease.'

You will find this doctrine at the bottom of all Sydenham's speculations on the subject. But Cullen doubted it, and even endeavoured, in an elaborate argument which you may read in his 'First Lines,' to disprove it. He held gout to be an affection of the nervous system. I shall not trouble you by detailing his argument, for I consider it an utter failure. I am satisfied that the ancient doctrine, which asserts the *humoral* origin of the disease, is the true one. 'Morbific matter' (it may well be called a *poison*) is generated, or detained, under certain circumstances, within the body, and silently collects in the blood; until, after obscure threats, perhaps, and prelusive mutterings, it explodes in the foot; and then the bodily economy, like the atmosphere after a thunder-storm, is, for a while, unusually pure and tranquil. To some such conclusion as this the result of all modern research seems clearly and unfailingly to tend. Sir Henry Holland, for example, in one of his thoughtful and thought-exciting volumes, expresses his belief in 'a *materies morbi*, which, whatever its nature, is capable of accumulation in the system, of change of place within the body, and of removal from it.' In this, and in several other propositions relative to gout, enunciated in distinct terms by this learned writer, I fully concur. Some speculations first put forth by Dr. William Budd, in a communication to the Medical and Chirurgical Society, throw a strong light upon this perplexed subject; and bring the phenomena, not only of gout, but also of many other important complaints, within the operation of one general, comprehensive, and intelligible law. I shall take leave to refer once more, in a very cursory manner, to some of Dr. Budd's positions.

I need not remind you of the various ways in which extraneous matters find entrance into the blood. Poisons, under their proper shape and name; medicines, which misapplied become poisons; our natural food and drink, which the folly of man converts into poison; the products or dregs of the secondary assimilative process; these are common sources of impurities, more or less hurtful, which mix and circulate with the vital fluid. Some of these extraneous matters escape harmlessly by one or more of the waste-pipes and emunctories of the body. Some are entangled in its solids: but not indiscriminately; for different substances have their special or their favourite resting-places. All this is well known to persons conversant with toxicological researches.

Now this doctrine, of the elective affinity between certain tissues or parts of the body, and certain morbific principles conveyed to them by the blood, is applied by Dr. Budd to elucidate the very curious fact of the symmetrical local manifestations of many disorders; which disorders are themselves so far general that they derive their origin from the circulating fluids. This symmetry he finds the most exact in chronic constitutional complaints, wherein the local morbid changes are effected in a manner which approximate closely to the processes of healthy nutrition. He shows good reason for believing (what, if the whole theory be true, we should expect) that the same symmetrical phenomena are modified by the *amount* of the poison collected in the system. If there be a certain quantity only, it may settle in some favourite or congenial spot, on one side of the body. If there be more than enough to saturate that part, it goes next to the corresponding spot upon the opposite side;

or, perhaps, to an analogous part of the other limb of the same side. If there be more still of the poisonous material, it flies to, and occupies, other parts also. He further shows that the elective affinity is more exclusive, and the bond of union stronger, in respect of some morbid principles, than of others: and in proportion as the affinity is weak, so is the local manifestation of the disease apt to shift, by metastasis, from place to place. When the matter which has thus entered, or combined with, a certain tissue or organ, is anyhow loosened and released from that union, or repelled from the part, it is again set afloat in the blood, to 'break out' elsewhere; to tease various organs, perhaps, or to derange the whole economy. The alternation so often to be noticed between certain cutaneous eruptions and internal disorders of function, is a striking and familiar example of this. The eruption afforded presumptive evidence of the detention of some peculiar morbid principle in that part; and the internal affection which succeeds the disappearance of the eruption, denotes that the morbid principle has re-entered the blood. Some of these peccant or poisonous matters fix permanently in the affected spot or spots; and some of them may even be recovered in substance from the dead tissue by chemical means: the poison of lead, for example, from the symmetrically palsied muscles. Others appear to be expended gradually in the part, and so eliminated from the system. Dr. Budd observes, that the regular arrangement of these local tokens, whether they be outward or internal, is disturbed by the presence of fever. Probably the febrile tumult may itself be owing to the quantity of the noxious matter in circulation within the body. He states, also, that, *cæteris paribus*, this morbific matter is most apt to pounce, in the first instance, upon parts which have been previously hurt, or which are mechanically irritated at the time. For which reason a part that has once been affected by it is more likely than other parts to suffer again.

Now, see how thoroughly the ascertained phenomena of gout accord with this theory. Certain habits of life produce fulness, and richness, and impurity of blood; the same habits which breed the lithic acid diathesis. Lithates are poured forth with the urine, and sometimes deposited in vast masses around the gouty joints. At first, after obscurer intimations of the presence of the poison in the system, it thunders in the foot; and there, perhaps, is all discharged and spent. The chemist Berthollet found that the skin of a part affected with gouty inflammation communicated instantly to litmus paper a deep red colour: a large quantity of acid was evidently passing off by exhalation from the inflamed surface. If the poison be too copious to find sufficient vent in one joint, it attacks another, or more than one other. 'Quandoque etiam primis morbi diebus, cum materia peccans adeo exuberat ut ei capiendæ pes unus impar sit, utrosque simul pari vehementiâ fatigat: sed ut plurimum pedes successivè, uti diximus, adgreditur.' These are Sydenham's words. A chain of repeated paroxysms at length purifies the blood: 'donec tandem materiâ peccante prorsus absumtâ, æger pristinam obtinuerit sanitatem.' The descent of the disorder upon a particular joint is often determined by a recent blow or sprain, or by the chronic weakness consequent upon an ancient hurt. If the inflammation, after thus settling, be repelled from the foot, the poison, being driven again into the blood, may light upon some vital organ, and place the patient's life in immediate jeopardy. The late Dr. Parry, of Bath, had at one time under his care two patients who had attempted to cut short or to ease a paroxysm of gout, by plunging the affected foot into cold water. This gave instant relief to the pain, and in both instances the inflammation presently abated; but in both, also, *hemiplegia* occurred a few hours afterwards.

Nearly a century ago Mr. Murray Forbes, anticipating Dr. Prout, ascribed gout to the presence of uric acid in the blood; although he confessed his inability to discover it there. This hypothesis has been verified, extended, and matured into a sound and stable theory by Dr. Garrod. That able chemist first ascertained that uric acid does exist in very minute quantity in the blood of health; but that in the blood of gouty patients it is always present in detectable excess. By a simple, easy, and beautiful experiment he contrives to obtain, strung upon a fibre of hemp which had been left in the serum of blood drawn from a gouty person, during its evaporation to dryness, a row of microscopic crystals of uric acid.

Next, he neglected no opportunity of investigating the morbid anatomy of gout. And he found that while masses of urate of soda are in some few gouty persons deposited *around and without* the affected joints, the cartilages *within* are (not sometimes, as Dr. William Budd had previously noticed, but) *always* more or less encrusted and inter-penetrated with this salt. Always, I say, for it has invariably been found when searched for; and in one remarkable instance, which may be regarded as a *test* instance, this urate of soda was seen to be present in considerable quantity in the metatarso-phalangeal joint of the right great toe of a man who had suffered one single attack only of gout, limited to that foot, thirteen years before his death from cardiac disease.

On the other hand, Dr. Garrod has never detected, though he has diligently looked for, a similar deposit in the cartilages of the joints, or elsewhere, of persons who had had acute or chronic rheumatism, or any other articular disorder.

He regards this peculiar deposit as the cause, and not as an effect, of the gouty inflammation. He believes that the inflammation so provoked tends to destroy the urate of soda in the blood of the inflamed part, and consequently in the whole mass of the blood in the body: and that the presence of the urate of soda in the circulating blood is probably the cause of that disturbance of the general health which precedes the gouty seizure, as well as of many of the anomalous symptoms to which gouty persons are subject.

The apparition of this urate of soda in a solid and visible form identifies infallibly the nature of the case in hand: while it stamps gout generally with the brand of a distinct and specific disease.

The prognosis of gout may be gathered, without much further suggestion on my part, from what has already been said. The inflammation which befalls the joints has no worse *event* than the thickening, or, perhaps, the chalk-like deposit, which it produces; so that *gout in the extremities* is not a mortal disease. But as it is not always confined to the extremities, the life of a gouty person is justly held to be insecure. 'La goutte articulaire (says some French author) est celle dont on est *malade*; et la goutte interne est celle dont on *meurt*.' You will find that all insurance companies exact, *cæteris paribus*, a larger premium from those who have had the gout. When it proves fatal, it is by translation of the disease, or rather of the gouty virus, to some vital part; to the stomach, the heart, the lungs, the brain: or by the slow degeneration of organs, especially of the kidneys.

As the early visits of gout are generally followed by a striking change for the better in the health and feelings of the patient, it is not to be wondered at that the disease, in its genuine and decided form, should have sometimes been wished for, and even courted. It is commonly thought that a fit of the gout clears the system of all other disorders. It does, indeed, clear it, for the time, of those disorders which resulted from the poison of gout. But this fact has led to great practical mistakes. First, to the error of looking on inertly, and doing nothing to remedy the ailments which are supposed (often very wrongly) to depend upon lurking gout, and to require a fit of the gout for their cure; and, secondly, to the more dangerous experiment of endeavouring to force on such a fit by excess and intemperance. Men forget, or do not know, that the enemy thus reinforced, instead of evacuating the fortress by its outports, may retreat triumphant into the citadel. To drop metaphor, such a course of living may, indeed, determine an attack of the disease in the extremities, but it involves the fearful peril of some fatal internal seizure. Besides, the benefits expected from external gout belong to its earlier returns alone. The more numerous the fits, the faster does the general health break, and the more stubbornly do the associated symptoms cling to the patient: and many persons linger on, martyrs (as they say) to the disease, long after they have ceased to be fit for any of the business of life, or capable of any of its pleasures: suffering, rather than living, if 'non est vivere, sed valere, vita.' Nevertheless, as Heberden observes, 'People are neither ashamed nor afraid of it; but solace themselves with the hope that they shall one day have the gout; or, if they have already suffered it, impute all their other ails, not to having had too much of that disease, but to wanting more. The gout, far from being blamed as the cause, is looked up to as the expected deliverer from these evils.'

And this mistaken ambition is heightened, no doubt, by the notion, still more absurd and ridiculous, yet very generally prevalent, that it is a *creditable* thing to have the gout; a notion which evidently originated in the fact of its being peculiarly incidental to the wealthy and the great, to men of cultivated minds, and intellectual distinction. Nothing can show more strongly the power of fashion than this desire to be thought to possess, not only the tone and manners of the higher orders of society, not their follies merely and pleasant vices, but their very pains and aches, their bodily imperfections and infirmities. All this is more than sufficiently ludicrous and lamentable: but so it is. Even the philosophic Sydenham consoles himself, under his sufferings from gout, with the reflection that it destroys more rich men than poor—more wise men than fools. 'At vero (quod mihi aliisque licet, tam fortunæ quam ingenii dotibus mediocriter instructis, hoc morbo laborantibus solatio

esse possit) ita vixerunt atque ita tandem mortem obierunt magni Reges, Dynastæ, exercituum classiumque Duces, Philosophi, aliique his similes haud pauci. Verbo dicam, articularis hicce morbus (quod vix de quovis alio adfirmaveris) divites plures interemit quam pauperes, plures sapientes quam fatuos.'

The treatment of a gouty patient naturally divides itself into that which is proper during the paroxysm, and that which is proper during the intervals between the paroxysms.

It was maintained by the great physician whose words I have just been quoting, that all artificial evacuations during a fit of the gout are useless or hurtful. He therefore discountenanced blood-letting, purging, and the use of diaphoretic medicines. It was nature's prerogative, he said, to exterminate the peccant matter in her own way; namely, by depositing it in the joints, whence it might be dispersed by insensible transpiration. Evacuant remedies had no other effect than that of recalling into the blood this peccant matter, which nature had already thrust forth to the extremities of the body; whereby it happened that the virus, which should have been eliminated through the joints, fell upon some of the viscera; and so the patient, who was in no danger before, became in peril of his life. I mention all this to show you how nearly identical was Sydenham's theory of the gout with that which is now rapidly regaining its lost ground in this country, and which I firmly believe to be the true one. After his time, and upon his authority, the treatment of gout lapsed into an inert expectancy. Even Cullen came to the conclusion that the best thing to be done is to commit the sick man to 'patience and flannel alone.' Here and there, indeed, an advocate of more active measures sprang up. Dr. Rush thought that venæsection was always safe, and generally serviceable: and some persons, following the bad example of the illustrious Harvey, were for extinguishing the inflammation by immersing the affected joint in cold water. Heberden, however, had clearer and juster views upon the subject. He perceived that one reason why physicians did nothing to check the paroxysm was, that they did not know what would check it. He agreed with Cullen in thinking 'that no medicine for curing the gout had yet been found;' but he did not partake of his belief in 'the impossibility of a cure by medicines.'

'The itch (he observes) is supposed to be wholesome in some countries, where it is endemical; and an ague has been considered as a minister of health, whose presence and stay ought by all means to be courted. These opinions are now pretty generally exploded in England; and I hope the time will come when a specific for the gout, as certain as those which have been discovered for these two disorders, will ascertain the equal safety and advantage of immediately stopping its career, and preventing its return.'

That time has come: for the colchicum, judiciously employed, may fairly be accounted a specific for the gouty paroxysm. And it is remarkable how long this truth has been seen, though not distinctly or steadily. The hermodactyl of the ancients is the modern colchicum, and was in high estimation among them for its efficacy in the same forms of disease as are benefited by the colchicum now. It bore, with some, the name of *anima articulorum*, the soul of the joints, because (as Quincey states) it prevented 'the lodgment of such gritty matter as occasions the gout and arthritic complaints.' And I think there can be no doubt that the active principle of the quack medicine so much in vogue for the cure of gout some years ago—I mean the *eau médicinale*—either was the same with that of the meadow-saffron, or was derived from the same family of plants which botanists have associated together under the title of 'Melanthaceæ.'

This drug has certainly the property of easing, in an almost magical manner, the pain of gout. How it operates is not so clear. It is apt to produce nausea, faintness, and diarrhœa; but its curative influence is not conditional upon the occurrence of these symptoms. Sometimes the rapid disappearance of the gouty inflammation is its only perceptible effect. The patient may be in helpless agony, with a tumefied red joint, to-day; and walking about, quite well, to-morrow. The colchicum is therefore plainly an anodyne. It also sensibly modifies the condition of the urine, rendering it less acid, and even alkaline; and increasing its quantity. These effects are consequent, I presume, upon changes in the blood wrought by this substance, which thus, and there, proves somehow an antidote to the poison of gout.

There are, as you may be aware, various preparations of colchicum in use: the wine of the bulb; the wine of the seeds; the vinegar of colchicum; the acetous extract, made by evaporating that vinegar; the inspissated juice of the plant itself. These are all of them active and valuable medicines; and I should pretend to more knowledge than I possess, if I undertook to tell you which of them is the best.

The mode of administering the remedy, in a regular fit of the gout, is simple enough. For example, you may give forty or sixty minims of the *vinum colchici*, in a saline draught, at bed-time; and half a drachm more, in a warm black dose, the next morning; and you may repeat this sequence if the gout continue. Some persons give twenty minims every six hours, with a drachm of Epsom salts, and a drachm of syrup of poppies in the draught, till the symptoms yield; but I prefer the other plan. In this way the pain is usually calmed, and the swelling reduced in a few days; or even, as if by a charm, in a few hours.

But you must not be satisfied with thus quelling the pain and inflammation. A strong prejudice at one time existed, and still exists among some practitioners, against the colchicum. It was said that it had indeed the power of cutting short the paroxysms, but that it cut short the patient's life also: that they who trusted to it for getting rid of the gout, very seldom lived more than two or three years afterwards. How far this was true I cannot tell: but even admitting it to be true, it was not, I conceive, so much the fault of the medicine as of the patient, or of the physician who did not properly admonish the patient. Men were very glad to get rid of their gout on such easy terms; and they will sometimes say to us now: 'I have, as you see, got the gout. This is Monday. I must be in the House of Commons, or at the head of my regiment, or attend such and such a meeting, on Wednesday: and I expect that you will enable me to do so.' Or even sometimes the reason may be that they are engaged to feed at some grand dinner two or three days afterwards. Now if patients are content, and are suffered to be content, with expelling the gout from their toe, without observing abstinence more than a day or two, and without any further medication at the time, we can easily perceive the probability of their being soon attacked by some formidable internal complaint. I apprehend that the proper way to eradicate the lurking residue of the mischief is to continue to give small doses of the colchicum; five minims of the wine, for instance, two or three times a day, for a while. Moreover, purgatives must be employed, if that remedy do not prove aperient. Not violent purgatives, however, which, by weakening the patient, seem to strengthen the power of the gouty virus. With mild cathartics, moderate doses of mercury will generally be advisable, to correct the subsisting disorder of the hepatic functions; and the patient must adopt and pursue abstinent, or at any rate strictly temperate habits, in respect of meat and wine.

And as I think that the dregs, if I may so speak, left behind it by a gouty paroxysm, may be dispersed by the continued use of what, in the usual acceptation of the word, I may call *alterative* doses of colchicum (doses, that is, which produce the desired effect gradually, and by insensible operation), so I think it probable that many a fit of the gout might be averted, if the remedy were given in the same way upon the first occurrence of the ordinary premonitory troubles. Many of those troubles never appear to reach the crisis of a fit. There are headaches, attacks of asthma, derangements of the digestive organs, which, occurring in a gouty person, are presumed to be fainter intimations of the presence of the gouty poison in his blood; and if such symptoms yield (as unquestionably they often do) to colchicum, the presumption draws near to proof. Sir H. Holland has well remarked that the meadow-saffron, by its curative effects, may bring sundry maladies, hitherto thought anomalous, under the same law of morbid *gouty* action; just as the Peruvian bark has reduced many complaints, that were previously vague in their nature, within the category of *aguish* distempers. The same author conjectures that as hypochondriasis is certainly often symptomatic of the gouty poison in the male, so may sometimes the kindred disease, hysteria, be in the female.

There is, however, one remedy, introduced of late years only into our Pharmacopœias, which will probably be found more useful than any other as a prophylactic against gout: I mean *lithia*, the oxide of the alkaline metal lithium, the third fixed alkali. This substance combines readily with uric acid, and the urate of lithia is the most soluble of all the known urates. It was first employed for therapeutic purposes by Dr. Garrod, in the treatment of uric acid gravel, and in chronic gouty states of the body. The tendency of this salt is to free the blood from any excess of uric acid which it may contain, and so to prevent, or restrict the amount of, that urate of soda, the presence of which constitutes the essential condition of the gouty attack. It is diuretic also.

This remedy has the additional recommendation that it forms a pleasant and refreshing drink when taken in aërated water. The British Pharmacopœia prescribes a *Liquor Lithiæ effervescens.*

Whether the strong alkalising power of this salt has ever been used in the treatment of acute rheumatism I do not know.

Strange stories are recorded—strange, but I believe true—of instantaneous cures of the gout by strong mental emotion; by sudden terror, by violent wrath. Dr. Rush relates an instance of this. An old man, who for several years had suffered an annual attack of gout, was lying in one of these paroxysms, when his son, by some accident, drove the shaft of a waggon through the window of his room, with vast noise, and a great smashing and destruction of glass. The old man leaped out of bed, forgetting his crutches; and his wife, on entering the apartment, was surprised to see him walking up and down, and exclaiming angrily against the author of the mischief. The late Professor Gregory, of Edinburgh, was in the habit of mentioning another example to the same effect, authenticated to him by a naval surgeon. It occurred in the person of an officer who was freed from an attack of gout, when at sea, by an alarm of fire. Southey again, in his autobiography, speaking of a Mr. Bradford, says, 'By that time he had become a victim to the gout. An odd incident happened to him during one of his severe fits, at a time when no persuasions could have induced him to put his feet to the ground, or to believe it possible that he could walk. He was sitting with his legs up, in the full costume of that respectable and orthodox disease, when the ceiling being somewhat old, part of it gave way, and down came a fine nest of rats, old and young together, plump upon him. He had what is called an antipathy to these creatures, and forgetting the gout in the horror which their visitation excited, sprang from his easy chair, and fairly ran down stairs.' Whether this influence of certain states of the mind be rightly alleged or not, it is clear that we can never hope to make any practical use of such a remedy. Indeed, a fit of the gout has been sometimes *brought on* by a mental shock.

The treatment of a gouty patient in the intervals between his attacks of gout, whether regular or irregular, must be chiefly regimenal. The instances are not few of men of good sense, and masters of themselves, who, being warned by one visitation of the gout, have thenceforward resolutely abstained from rich living, and from wine and strong drinks of all kinds, and who have been rewarded for their prudence and self-denial by complete immunity from any return of the disease; or upon whom, at any rate, its future assaults have been few and feeble. On the other hand, many who are liable to gout are taught by sharp experience that a single debauch, a casual glass or two of champagne, even an unusual indulgence in the use of animal food, may suffice to bring their enemy suddenly upon them. I am sure it is worth any *young* man's while who has had the gout to become a teetotaller. But the case is different with the *old*, and with those whose health has been broken by the inveterate disease. They must be allowed a certain quantity of their accustomed good cheer, or they become an easier prey to the disorder. In such cases you must trim, as well as you can, between opposite dangers—between the Scylla of excess, and the Charybdis of debility.

It is the same with respect to exercise. The young and the hearty can scarcely take too much: the old and the dilapidated, by one act of over-exertion, may incur the penalty of an attack. Although I can do little more than point out general principles for your guidance, I may remark, in reference to exercise, that it should never be *violent*, lest it excite a paroxysm by straining any part, or by causing great fatigue: that it should be *habitual*, daily—not used by fits and starts, and interrupted by long periods of indolence or inaction: and that it should be *active* muscular exercise, as distinguished from passive exercise or gestation. No mode of exercise is so good as that of walking; and with this may be agreeably and beneficially conjoined riding on horseback.

Early and regular hours are also of much importance, and the avoidance of severe mental application. Sydenham relates that one of the most atrocious attacks of gout he ever underwent was induced by intense thought and study, in the composition of his medical works.

The regimen which I have been recommending may require some power of self-control: yet in reality it implies no severity of mortification. It is perfectly compatible with life's best enjoyments: but to be effectual it must be adopted early, as soon as the disease threatens; and steadily persevered in. Gouty persons, however, do not like these restraints. They are ready to believe that an attack of gout will do them good; or, if they are disabused of that error, they are desirous that some medicine may be found which will avert the disease, without their being obliged to forego their accustomed indulgences. 'To gratify this desire (says Cullen) physicians have proposed, and to take advantage of it empirics have feigned, many remedies.' One of these was the famous Portland powder, of which Heberden remarks: 'Unum est ex multis quæ vocantur remediis specificis, quorum ortum, et splendorem, et occasum vidi.' It consisted chiefly of bitters and aromatics, and had descended, with some slight variations in its composition, from the times of Galen. Another preventive has recently been praised by Dr. Graves, of Dublin, as being highly serviceable, although (what is a suspicious circumstance) it had, like the Portland powder, fallen out of fashion. These are its ingredients:—Two ounces of orange peel, an ounce of powdered rhubarb, and two ounces of the *pulvis aloës cum canellâ* of the Dublin Pharmacopœia, steeped for a week in a quart of brandy. A table-spoonful of the strained infusion is to be taken, mixed with two or three spoonfuls of water, night and morning. Sir Henry Halford recommends what I think a better form of prophylactic remedy; viz. a few grains of rhubarb, with double the quantity of magnesia, every day: or some light bitter infusion, with tincture of rhubarb, and about fifteen grains of the bicarbonate of potass.

Now what has been observed respecting preventive remedies of this kind is, not so much that they are inefficacious, as that, *when exclusively trusted to*, they are unsafe. I believe that they are often useful by improving the digestive process; but they are dangerous substitutes for a course of temperance and exercise.

When gout attacks the stomach, either by retrocession or primarily, it often proves rapidly fatal. The gastric affection is not, in general, inflammatory: so we judge, at least, from the *juvantia*. The attack, which consists of violent pain, and a sense of weight or of constriction in the epigastrium, with sickness, vomiting, and a disposition to faint, is often relieved by the employment of stimulants. But such remedies would be likely to aggravate inflammation. It will always be well, when symptoms like these occur, to inquire whether any indigestible food has been lately taken: for *gout* (so called) *in the stomach* has sometimes turned out, under the test of an emetic, to have been nothing more than *pork in the stomach.* In the true gouty seizure, antacids will frequently remove the pain; magnesia, in full doses, with rhubarb. If this do not succeed, opium may be resorted to; and if it should be vomited, opiate enemata may be injected. Dr. Heberden thought that opium and hot spices were more efficacious and less inconvenient, in these cases, than wine and spirits; but when they fail, a glass of brandy will often allay the pain completely. The mustard poultice, or the turpentine stupe, applied over the epigastrium, has been followed by strikingly good effects. And it is in these emergencies, contingent upon retrocedent or misplaced gout, that we are justified in the endeavour to induce gout in the extremities; not however by internal stimuli, but by enveloping the feet in a mustard poultice, and so enticing or provoking the foe to quit his hold of the interior, and to appear in the outposts. And this expedient should be practised, whatever may be the internal organ upon which the gouty disorder has settled.

Sometimes, but much less commonly, actual gastritis does seem to ensue; and therefore all these cases are anxious and alarming cases. I do not know how the inflammatory affection can be discriminated from the non-inflammatory, unless it be by the occurrence of *tenderness* with the pain, and of fever. You must treat such cases as you would treat an ordinary case of gastritis, taking no further heed of the gout, except by the application of stimulating cataplasms to the feet.

This concludes what I proposed to say respecting gout and rheumatism: diseases of which the local seat is not exactly external, nor yet do they belong strictly to the interior of the body, except in their accidental complications. They form a link of connection between the internal and external disorders which fall to the care of the physician; and I proceed, in the next and last place, to speak of those complaints which either concern the integuments alone; or which, at any rate, are attended with some notable affection of the skin.

Under the general head of *cutaneous* diseases, are included maladies of very different kinds, and of very different degrees of importance. Some are attended with fever, and run a definite course, and are often dangerous to life. Others are chronic, irregular in their progress, troublesome perhaps, and obstinate, and disfiguring, yet implying no peril to the existence of the patient. Some again are contagious, while many are not so. But before I enter upon any further account of these diseases, I wish to make you acquainted with the names by which the various morbid appearances presented by the skin have been known since the time of Dr. Willan.

That author—whose works have been augmented by Dr. Bateman, so that perhaps I ought to say *those* authors—divides cutaneous diseases into eight orders, distinguished from each other solely by the appearances upon the skin. I shall omit the last of these orders, the order of *maculæ*, such as freckles and congenital spots and discolorations, because in fact these are not diseases at all.

The first, then, of the appearances described by Dr. Willan are *papulæ*; pimples. These are little elevations of the cuticle, of a red colour, and solid; not containing, I mean, any fluid. They are of uncertain duration, and often terminate in scurf. They are supposed to denote inflammation of the papillæ of the skin. If you would see an example of a papular eruption, look at that of small-pox, at its very earliest outbreak.

The second are *squamæ*; scales. These are small, hard, thickened, opaque, whitish patches of unhealthy or dead cuticle. The subjacent surface is red. They are well seen in lepra and psoriasis, and are very common in syphilitic eruptions.

The third are *exanthemata*; rashes. They consist of superficial red patches on the skin, variously figured, and irregularly distributed, and of all sizes. We have examples of them in some of the most important febrile cutaneous diseases; scarlet fever, measles, and others. Most commonly they are followed by a peeling off, or desquamation as it is called, of the cuticle.

It is a pity that some other technical name was not chosen for these rashes; because the term *exanthemata* has long been familiar to the profession as the title of an order of diseases in Cullen's Nosology.

The fourth are *bullæ*; blebs, miniature blisters. Portions of cuticle, of considerable magnitude, are detached from the subjacent skin, by the interposition of a thin transparent liquid; with inflammation beneath them. Such occur in erysipelas sometimes, and in pemphigus.

The fifth are *pustulæ*; pustules. Circumscribed elevations of the cuticle, containing pus, and having red inflamed bases. Minute abscesses. We have instances of these in common boils; and in the eruption of small-pox when at its height and maturity. They end in crusts, or scabs.

The sixth are *vesiculæ*; vesicles. Small elevations of the cuticle, covering a fluid which is generally clear and colourless at first, but becomes afterwards whitish and opaque, or pearl-coloured. These are exemplified in the eruption of cow-pox, and in the chicken-pox. You will observe that these vesiculæ differ very little, except in size, from the bullæ, or blebs. They often terminate in small scabs.

The seventh are *tubercula*; tubercles. This also is an unlucky name, since the word tubercle is almost appropriated, in the present day, to the scrofulous deposits which infest the lungs and other parts of the body in pulmonary phthisis. However, these cutaneous tubercles are small, hard, superficial tumours, circumscribed and permanent; or if they suppurate at all, the suppuration in them is partial. Sometimes they slowly ulcerate at the summit. The imperfectly suppurating pustules of the modified small-pox, and certain red spots which are apt to haunt the face, particularly of young persons, furnish examples.

Now it is very convenient, for the purposes of distinguishing different diseases, and of describing them, to know these outward marks when you see them, and to use these names. But they form a very unfit basis for any *classification* of diseases. Maladies may usefully be classed according to their causes; according to their intimate nature; according to the general plan of treatment they may require. But the superficial markings of disease have no definite relation to any of these heads. Besides, a complaint which is papular to-day may be vesicular to-morrow, and pustular next Saturday. Yet the classification most commonly followed in this country, and in France, is that of Willan and Bateman. Here we find collected under one and the same division, maladies which nature has stamped with broad and obvious marks of distinction; the febrile with the non-febrile; contagious complaints with those which have not that property; ailments that are local and trivial, with diseases of grave import, and deeply-rooted in the system at large. And, on the other hand, distempers which nature has plainly brought together, and connected by striking analogies and resemblances, this methodical arrangement puts widely asunder. I point out, without professing to remedy, these imperfections. I cannot even undertake to give you any full or systematic account of the many disorders comprised in this classification. There is, however, one group, so remarkable, so important, and so highly interesting, that I shall consider it as much in detail as I can. I allude to the group which Cullen comprehends under the title *exanthemata.* With this exception, the advancing year warns me that I must contract what I have to say respecting diseases of the skin within very narrow limits.

LECTURE LXXVII.

Exanthemata. They are contagious; sometimes epidemic. Period of the eruption; period of incubation. Theory of contagious Febrile Diseases. Thermometry of Disease.

Of the numerous complaints which are ranked among the diseases of the skin, some, I observed in my last lecture, are attended with fever, and some are not. Among the former there is a most interesting group, distinguished by other and far more important characters than the mere presence of fever, or peculiar marks upon the skin; characters that enabled Cullen to collect these diseases into a separate order, to which he gave the name of *exanthemata*. This is his description of them: 'Morbi contagiosi, semel tantum in decursu vitæ aliquem afficientes; cum febre incipientes; definito tempore apparent phlogoses, sæpe plures, exiguæ, per cutem sparsæ.' Contagious diseases; affecting a person once only in his life; beginning with fever. At a definite period small inflammations appear, often numerous, scattered over the skin. These, you will allow, are very remarkable characters. They are not all strictly and universally true, perhaps, of all the forms of disease which I propose to bring now under your notice; but they apply with more or less exactness to the several species of continued fever, to the plague, to small-pox, chicken-pox, measles, scarlet fever, and erysipelas.

Hooping cough, and the mumps, might be placed in the same catalogue, although in them there is no specific eruption on the skin: but I have already spoken of these two disorders.

Before I take up the consideration of any one of these diseases in particular, I shall premise a brief survey of certain circumstances that are more or less common to them all. A preliminary examination of the exanthemata as a class, will give you, I trust, clearer ideas respecting each of them: at any rate it will enable me to dispense with much needless repetition afterwards, and so to save both your time and my own; a matter of some consequence at this advanced period of the session.

In Cullen's description, the *contagious* property of the diseases comprehended in this group holds the first place; and it is their most important property. You will hear persons disputing about the *term* contagion; but such disputes can only arise from the want of a distinct definition of the sense in which it is employed. I understand a disorder to be contagious when it is in any way *communicable* from one person to another. Some, looking to its etymology, would restrict the word contagion to the cases in which there must be absolute *contact* of the healthy body with the sick body, or with its visible offscourings. When a disease can be conveyed through the medium of the atmosphere, or by means of other intermediate substances called fomites, they would call it *infectious*. And there can be no objection to such a distinction, provided it is borne in mind by the reader or hearer, as well as by the writer or speaker. But since in all cases the disease is conveyed to the body of the recipient by particles of matter proceeding from the body of the sick, and since it seems very unimportant whether those particles are in a fixed or in a volatile form, whether they are imparted by direct contact of the two human bodies, or by being wafted through the air, or carried upon articles of clothing, I shall include both and all these modes of communication under the single term contagion. This, in fact, is what is done in common discourse: all disorders that are 'catching,' I shall take leave to call *contagious*.

In this sense I believe that all the diseases just now enumerated *are* contagious; some, no doubt, much more strongly and distinctly so than others. They might fitly be called *procreant* diseases. Some of them are undeniably contagious. For example, we are privy to, and sometimes willing agents in, the communication of small-pox from one human being to another. There are others concerning the contagious nature of which medical opinion is less settled and unanimous. Many have denied that continued fevers are communicable from person to person. The evidence from which I conclude that they are so I shall lay before you when I have described those disorders. Even they who admit that they are contagious, are of opinion, many of them, that they sometimes break out spontaneously, without the intervention of any specific virus. No one questions, I fancy, the contagious property of measles; or of scarlet fever. Whether the plague, and whether erysipelas, be always or ever so produced, has been thought more doubtful. There are strong reasons for believing that the small-pox, at least, has now no other source than contagion. How it first arose it may be difficult to conjecture, but it is not known to originate spontaneously now-a-days.

Small-pox may in truth be regarded as the representative, or most definite type, of this group of diseases. I shall therefore take, by anticipation, some well-ascertained facts in its history, for the sake of illustrating the general subject. It is a malady which could scarcely be mistaken for any other; and of which the horrible aspect, disfiguring consequences, and fatal tendency are so strongly marked, that its appearance has always been watched with affright by mankind in general, and with intense interest by the philosophic physician.

In the acme of this disease, when it is severe, the whole surface of the body is covered with a vast multitude of little pustules. A minute drop of the matter contained in any one of these pustules, just so much as may suffice to moisten the point of a lancet, is inserted, we will suppose, beneath the cuticle of a healthy man, who has not been near the sick man. What follows this engrafting? Nothing, apparently, for several days: but then febrile symptoms burst forth: and by-and-bye a crop of papulæ appear sprinkled over the skin; and these gradually ripen into pustules precisely resembling that from which the engrafted drop was taken.

The very same phenomena ensue when a healthy man enters the chamber of a person who has the small-pox, and breathes, for a certain time, an atmosphere tainted with the emanations from his body.

The points to be noticed here are—1, the manifest introduction of the virus into the system: 2, its dormancy for a while; in medical language, the occurrence of a period of incubation: 3, the breaking out, at length, of a disease identical in its symptoms and in its character with that of the first of the sick persons: and 4 (most surprising of all), the enormous increase and multiplication of the poisonous matter.

I say the *history* of small-pox leads to the settled belief that this disorder, of which few persons are not readily susceptible, never occurs except from contagion. It does not appear to have been known in Europe till the beginning of the eighth century. No mention of any such malady is to be found in the Greek or Roman authors of antiquity. Now whatever may have been the deficiencies of the ancient physicians, they were excellent observers, and capital describers, of disease: and it seems to me scarcely possible that a disorder so diffusive, and marked by characters so definite and conspicuous, should have escaped their notice, or if known should have been *obscurely* portrayed in their writings.

On the other hand, Mr. Moore, in his learned and interesting 'History of Small-pox,' has shown that it prevailed in China and Hindostan from a very early period; even more than 1,000 years before the time of our Saviour. That it did not sooner extend westward into Persia, and thence into Greece, may be attributed partly to the horror which the complaint everywhere inspired, and the attempts that were consequently made to check its progress by prohibiting all communication with the sick, partly to the limited intercourse which then took place among the eastern nations, but principally to the peculiar position of the regions through which the infection was diffused; separated as they were from the rest of the world by immense deserts and by the ocean.

The disease is said to have broken out in *Arabia* at the siege of Mecca, in the year in which Mahomet was born; i.e. in the latter half of the sixth century. It was widely propagated by his wars, and by those of the Arabs afterwards; and it is generally believed to have first found entrance into Europe at the time of the overthrow of the Gothic monarchy in Spain by the Moors; when to avenge the well-known outrage upon his daughter 'Count Julian called the invaders.' Whensoever and wheresoever it came, it spread with fearful rapidity and havoc.

What I wish you to remark is this: that while almost all men are prone to take the disorder, large portions of the world have remained for centuries entirely exempt from it, until at length it was imported; and that then it infallibly diffused and established itself in those parts.

Of the more modern history of the disease our knowledge is more precise and sure. It tends uniformly to the same conclusion.

There was no small-pox in the New World before its discovery by Columbus in 1492. In 1517 the disease was imported into St. Domingo. Three years later, in one of the Spanish expeditions from Cuba to Mexico, a negro covered with the pustules of small-pox was landed on the Mexican coast. From him the disease spread with such desolation, that within a very short time, according to Robertson, three millions and a half of people were destroyed in that kingdom alone. Small-pox was introduced into Iceland in 1707, when 16,000 persons were carried off by its ravages; more than a fourth part of the whole population of the island. It reached Greenland still later, appearing there for the first time in 1733, and spreading so fatally as almost to depopulate the country.

Evidence to the same effect is furnished by the results of vaccination in some countries. Take one instance. Vaccination was adopted in Denmark in 1801, and made compulsory in 1810. From that time small-pox disappeared altogether, for fifteen years; whereas during the twelve years preceding the introduction of the preventive disease, upwards of 3,000 persons died of the small-pox in Copenhagen alone.

Now it is a very instructive fact respecting this disease, thus rankly contagious, and arising from no other source than contagion, that when it is epidemic in any place, many instances of it occur which we can by no means trace to contagion. Dr. Gregory tells us that of the numerous cases received into the Small-pox Hospital (to which he had long been physician) not one in twenty was capable of being referred to any known source of infection; the disease being ascribed by the patient to cold, fatigue, change of air, or some other innocent circumstance. A prisoner who had been for some time shut up in solitary confinement in the Penitentiary at Millbank was seized with small-pox.* Surely all this should warn us against inferring of analogous disorders (of continued fevers, for example) that they are necessarily not contagious, because we often fail to discover any way in which the poison could have been applied. If small-pox be produced by contagion alone, and yet the mode in which the contagious matter has been communicated eludes sometimes our closest scrutiny, then we must conclude that the same thing may happen in *other* contagious diseases, of which the contagious property may not be so strong or so obvious. Nay, the argument from analogy will lead us a step further. If once a disorder of this kind is clearly proved to be sometimes the effect of contagion (and this I think I shall be able to prove to you of continued fevers), we cannot help entertaining a doubt whether the disorder in question really has ever in reality any other cause. It is chiefly with a view to the light which they throw upon the obscurer subject of continued fever, that I am thus anticipating some points in the history of the contagious nature of small-pox. That the question admits of *no* doubt has been ably argued by Dr. William Budd, from the *propagating* qualities of these specific diseases.

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* This fact was by some regarded as furnishing presumptive evidence that small-pox may, now-a-days, have sometimes a spontaneous origin. I am at length able to destroy that inference by the testimony of the following letter, which Dr. Pratt has permitted me to make public:—

Rothsay Terrace, Cardiff:
June 9, 1860.

DEAR SIR,—While visiting my son I by chance took up your Lectures, and in the 83rd Lecture saw mentioned the case of small-pox which occurred in the Penitentiary. It will perhaps be satisfactory to you to have some explanation of that case.

It happened about forty years since. At that time my father was medical officer to the establishment, and resided there. I was practising at Kennington; and, having been called to visit a case of confluent small-pox, and being informed that the patient had previously had the disease, I was desirous that my father should see it, and incautiously sent for him. He saw the patient with me, and immediately returned to the Penitentiary. The patient died. A few days after this case occurred in the Penitentiary, much to my father's annoyance, who, as well as myself, was satisfied that he had introduced the disease. It was absolutely necessary to keep this a secret, as it would have seriously affected him with the Committee, as he was not allowed to visit private patients after taking up his residence in the building.

I remain, &c.,
CHARLES EDWARD PRATT, M.D., Edin.

To Dr. Watson.

A similar case of difficulty, similarly cleared up, is related in pp. 65 and 66 of Dr. Gregory's book 'On the Eruptive Fevers.'

Again, it is noticed of small-pox—and it is the same with the other diseases in this group—that the human body is not always equally susceptible of its contagious influence. Some individuals are more readily affected by it than others: and the same individual more so at one time than at another. There are even some who seem to be incapable of taking the small-pox—just as some, who are quite as much perhaps in the way of it as their neighbours, never become infected with the great pox. Mr. Cross, in his 'History of a Variolous Epidemic' which occurred at Norwich in 1819, tells us that, of 215 persons who had not been vaccinated nor had the small-pox, and who were living at Norwich in the same houses with persons ill of that disease, fifteen did not become affected with it; and of these fifteen it was ascertained that ten had escaped under similar circumstances of exposure before. I mentioned, on a former occasion, the fact that a certain dog, in Paris, could not be made to take the contagion of rabies.

It is not at all uncommon for persons to resist the influence of contagion at one period and to yield to it at another, even when the exposure has appeared to be less complete. Mr. Cross gives a striking example of this. A man, who believed that he had had the small-pox, lived for twelve years as a nurse in a house of reception for persons inoculated with that disorder.

At the end of that time he caught the small-pox, which proved fatal to him. Now this might have been, and probably was, as the man supposed, a *second* attack. The late Mr. Lockley told me of an instance still more remarkable, as being free from that ambiguity. Nearly the first patient he ever attended, if not the very first, was an old woman, who for years had been in the habit of going from village to village as a nurse; and of nursing a great number of persons labouring under small-pox, which she had never had, and against which she (naturally enough) believed herself proof. At length she was taken ill, and died of small-pox, under Mr. Lockley's observation, at the age of eighty-four. Take one more illustration from another of these disorders. In 1845, a lady with whom I am acquainted went through an attack of measles, that disease being prevalent in the village where she was then residing. She had never had the measles previously: yet she had, long before, personally tended eleven of her twelve children when ill of the same complaint.

Generally we can assign no reason for these variations and differences. Age seems to have something to do with them. Infants are but little susceptible of the operation of contagions. Debility, howsoever produced, certainly augments the disposition to be affected by this, as by other causes of disease. The strength and the dose of the poison must also be taken into account. As some men can drink a much larger quantity of wine (which is an alcoholic poison) than others, without being intoxicated, and are differently influenced by the same quantity at different times, so is it also with the animal poisons we are now considering; so is it, as I showed you before, with the mineral poison of mercury.

This fluctuating power to resist contagion is most conspicuous, perhaps, when viewed in reference to scarlet fever. After the very earliest periods of life, children catch infectious disorders of all kinds readily enough; more readily than in mature age. The poison of scarlet fever operates with less certainty upon adults than the poisons of small-pox, and of measles. Some medical men escape scarlet fever altogether, although brought much into contact with it by their vocation. I do not know that I ever had scarlet fever.

Another fact, well worthy of notice, is that small-pox, which is so rankly contagious, and which has at present no other source besides contagion, has its alternate periods of slumber and of activity. This metropolis, and most of our large towns, are never entirely free from it. Scattered cases occur, here and there; and when thus thinly disseminated, the disease is said to be *sporadic*. But there are seasons in which it spreads rapidly and extensively, and assumes the form of an *epidemic* distemper. We are now living (1871) in the midst of a very severe epidemic of small-pox. The same is equally true of the other complaints included in this group. Sometimes they are confined to single families; sometimes they pervade a whole district.

Hence you can never infer that any febrile disorder is not contagious, merely because it prevails epidemically. Many epidemic diseases are probably not contagious. But the two properties may and do meet in the same malady. They are not to be set in opposition to each other, or regarded as incompatible properties, as they have been by some ingenious writers.

With respect to these epidemic visitations of the exanthemata, certain general facts have been ascertained, very useful and necessary to be known.

1. The strength of the contagion, and the severity and fatality of the disease, vary at different periods of an epidemic. In general the contagion is the most active, and the disorder the most fierce, at the outset of the epidemic. By degrees its violence slackens, and it ceases to spread. This is partly to be explained by the fact that the number of persons who are susceptible of the disease, and who have not yet been attacked, are fewer and fewer as the epidemic proceeds. The fire languishes for lack of fuel. But this does not seem to be all. The disease dies out before it has affected all those who are capable of receiving it. We might, I think, expect, prior to experience, that the earlier cases would usually be the severer; for the weak, who are less able to struggle with the complaint, and they who, by peculiarity of constitution, are most susceptible of the morbific influence, are likely to be the first to suffer.

2. There are great varieties also in the general character of the symptoms that occur in different epidemics of the same disorder. At one time, or in one place, inflammatory symptoms run high; in another place, or at another time, there is an early tendency to debility and sinking. One epidemic is more malign than another. And the practice varies accordingly: so that these are facts of the greatest importance. The prevailing character of the malady is attributed to what is called the *epidemic constitution of the season*. And when we have made

out, by observation, what this epidemic constitution is, we have obtained a clue to the proper management of the disorder.

These differences in the prevalence of the disease, and in the character of its symptoms, are not to be explained by any variation in the exciting cause, which is a definite animal poison; nor can they be reasonably ascribed to any appreciable quality or agency of the weather *at the time*. They must depend upon changes that have been slowly wrought upon the human body: and those changes, constituting an acquired predisposition, are probably due, in part at least, to *previous* conditions—atmospherical perhaps, social possibly—which have exercised a long and gradual influence upon all the individuals of a community.

After stating, in the first clause of his definition, that the exanthemata are contagious diseases, Cullen announces, in the next place, the very curious fact, that they occur but once in a person's life. 'Semel tantum in decursu vitæ aliquem afficientes.' In this they offer a remarkable contrast to ordinary inflammations, which, having happened once, are, for that very reason, more apt to happen again.

You will take care to observe, that it is not the mere circumstance of the disease being *contagious* that makes the difference. Those disorders which shield the system against their own future recurrence are, all of them probably, contagious; but the converse does not hold. It is not true that all contagious disorders protect the constitution from their own return. Gonorrhœa, purulent ophthalmia, tinea capitis, cholera, erysipelas, relapsing fever: some of these not only do not secure a patient from a repetition of the disease, but perhaps even render him more liable to it in future.

Neither is the proposition absolutely and invariably true of any disease. Like most general rules, it admits of occasional exceptions. There is not one of the group enumerated in the beginning of this Lecture, which has not been known to occur more than once in the same person. Small-pox has, in many instances, affected the same individual twice; even when the first attack had been so severe as to have engraved upon his skin deep traces of its visit. There are a few instances recorded of its *third* occurrence. It was believed, at one time, that whenever the disease was thus repeated, it was *always*, in the first instance, severe; and this severity, taken in conjunction with the repetition, was thought to indicate a strong natural susceptibility of the disorder. But it has since been noticed that the primary visit is sometimes unusually mild: and this fact (so powerful is the love of theory) has led to the supposition that the first attack was not sufficiently intense to affect the whole mass of the blood, and to exhaust the inborn susceptibility. I believe that the two attacks have always been separated by a considerable interval of time. I have myself known two very striking instances, about which there could be no mistake, of the recurrence of measles in several children of the same parents. This proclivity to be again affected by the specific poison seems to run in families. It is less uncommon for scarlet fever to happen a second time in the same individual. No contagious disease therefore furnishes complete future protection against itself. But that this privilege belongs, as a general rule, to small-pox, to measles, and to scarlet fever, there can be no doubt: and the validity of the rule is applicable in the order in which I have here mentioned them. It applies also, more or less uniformly, to the other exanthemata. A person who has undergone a well-marked attack of continued fever is rarely liable, according to my experience and belief, to suffer the same species of fever again. The plague is said to afford a temporary safeguard against itself. Very few persons have it twice in the same season. During one epidemic Dr. Russell found that, among 4,400 individuals who underwent the disease, only twenty-eight contracted it a second time.

The next clause in Cullen's definition asserts the supervention of the cutaneous marks—in technical language, of the *eruption*—at fixed times (*definito tempore*) after the commencement of the general fever. It is clear, therefore, that the cutaneous inflammation cannot be the cause of that fever, but is itself an effect of the contagious poison. Here, again, we have a point of distinction between the febrile exanthemata and inflammatory fevers, or what Cullen calls the phlegmasiæ, in which the local inflammation commonly precedes the pyrexia.

In reality the circumstance to which I have just referred shows the impropriety of ranking these diseases under the head of *cutaneous* diseases. They would more rightly be called *blood* diseases. The disseminated cutaneous inflammation is a curious and an important circumstance; and it is the symptom which, in the majority of cases, is most *distinctive* of the disorder: but it is not an essential circumstance. Thus, although there are commonly distinct eruptions (quite independent of common petechiæ) at certain stages of typhus and of typhoid fever, a lenticular mottling, somewhat like that of measles, or a number of rose-coloured spots, yet these are not seldom absent altogether. And the very same thing happens in other diseases of the same group—diseases which all the world thinks and calls cutaneous. A febrile affection, often a fatal one, but attended with no rash, proceeds sometimes from the contagion of scarlet fever. The fever—or rather the patient—is *not* scarlet. The worst form of *cynanche maligna* is of this kind. It is just the same in measles. Authors speak of morbilli sine morbillis; of scarlatina sine scarlatinâ; and even of variolæ sine variolis. Catarrhal symptoms certainly occur sometimes in weakly children who have been fully exposed to the contagion of measles; and the complaint has proved fatal, without there having been any eruption at all. So in the plague: certain cases arise in which there are no buboes nor carbuncles, yet which undoubtedly originated from the common contagion of the prevailing epidemic.

The time at which the eruption comes out differs in the different diseases; and even in the same disease it is subject to occasional variation. The rule with respect to small-pox is, that the spots begin to be visible on the third day; that on which the sickness and fever commenced being reckoned the first. As far as I have observed, this rule is a very constant one. It has been noticed, however, that when the disease is confluent, and therefore severe, it occasionally shows itself in eruption on the second day; and when quite distinct and mild, sometimes not till the fourth.

The regular period for the outbreak of the cutaneous affection in measles is the fourth day: it scarcely ever begins sooner; but it is often later—on the fifth or the sixth day, or even later than that.

Cullen assigns the fourth day for the ordinary appearance of the rash in scarlet fever also. But in this he decidedly is wrong. Sometimes it is, I believe, perceptible on the first day; but its most general period is the second day. In severe and unfavourable cases it may be postponed till the fourth day, or longer.

The eruptions which are tolerably constant in some species of continued fever, observe less exact regularity in the time of their arrival. According to Dr. Murchison, the spots of typhus fever appear on the fourth or fifth day; those of typhoid or enteric fever rarely before the seventh day.

The period which intervenes between the reception of the poison, and the supervention of distinct symptoms—the period during which the virus, though doubtless at work, seems to lie dormant in the system—the period (in one word) of *incubation*, differs also in the different diseases of this group, and varies even in different cases of the same disease. Although no well-marked changes occur during this period, I believe that some slight deviations from the usual condition and feelings of the infected person might often be observed, if they were expected and looked for.

The period of incubation in continued fevers is very uncertain. In a paper upon this curious subject, published in the ninth volume of the 'Medical Gazette,' Dr. Gregory states it as his opinion, derived from much enquiry, that ten days is the *average* period. Dr. Haygarth reckoned the minimum period at seven; the maximum at seventy-two days. Sir William Burnett, in his 'Account of a Contagious Fever at Chatham,' gives the history of a party of men belonging to the 'St. George,' lying at Spithead, who were sent, on the 3rd of January, 1811, to assist in navigating the 'Dolphin' troopship; the crew of which were affected with typhus fever. On the 10th (seven days after exposure) fourteen of these men were sent to the Hospital-ship from the 'St. George,' ill with the fever; and many subsequently, up to the 21st of January (the eighteenth from exposure); after which period no cases occurred. According to Dr. Murchison's experience, the latent period is usually about nine days in typhus fever, but may vary from a few hours to twelve days; in enteric fever between one and two weeks.

The period of dormancy is more definite, yet still liable to some variation, in most of the other exanthemata. 'At the Small-pox Hospital,' says Dr. Gregory, 'abundant evidence has been afforded that the period of incubation (when the disease has been *caught*) is usually about twelve days.' It is, therefore, a remarkable fact that, 'when the small-pox is received into the system by *inoculation*, seven days only elapse between the insertion of the virus and the establishment of the fever.' Mr. Marson, after more than twenty years' attention to this subject, states that almost invariably there are twelve days of apparent freedom from illness after the infection of the disease has been received, then comes severe indisposition for forty-eight hours, and then the eruption begins to appear.

Dr. Bateman puts the period of incubation in measles at 'from ten to fifteen days.' I have known several instances in which the date of a single short exposure was exactly ascertained, and in which the disease commenced precisely a fortnight afterwards. In scarlet fever the average period is shorter; not more than from three to six days. In the plague it is, I believe, more variable; but generally not exceeding a few days. Dr. Russell tells us that, among those inhabitants of Aleppo who shut themselves up after having been in the way of the contagion, no instance occurred of the appearance of the malady later than the ninth or tenth day.

To say that a febrile disorder is contagious is the same thing as to say that it is produced by an *animal* poison. Now, there are many poisons, very deadly poisons too, which cause diseases that are not communicable from person to person. That particular poison, the *malaria*, is of this kind.

Of inorganic substances, some are taken into the blood and emerge again from the body, unaltered, with one or more of the ordinary secretions; chiefly with the urine. They may induce changes in the body as they pass; and if these changes be salutary, the substances so inducing them become medicinal. If the changes be destructive or injurious, the substances are strictly poisons.

Other of the inorganic poisonous substances do not find so ready an exit from the body. They enter into permanent chemical union with the constituent tissues of particular organs. In this way, to use the words of Liebig, they deprive the organs of the principal property which appertains to their vital condition, viz. that of undergoing and of effecting transformations. If the organs of which the functions are thus destroyed are vital organs, these poisons are fatal.

But the animal poisons, those at least with which we are now concerned, act in a totally different manner. They work or suffer changes in the blood, whereby they are themselves abundantly multiplied or reproduced; and the eruptive disease that ensues seems to be the mode provided by nature for the escape or the expulsion of this newly-formed morbid matter from the system. This is the old-fashioned humoral pathology; founded on bold, unproven speculation: and it is most curious to see these very doctrines, which had sunk into universal discredit and contempt, now again assuming their places, as scientific truths, upon the secure basis of organic chemistry. A wonderful sample of the sagacity of the older physicians—of the despised wisdom of our forefathers.

The ancients attributed various disorders to a fermentation of the animal fluids. The cause of fever, according to Hippocrates, was some morbid matter in the blood. This matter, by a process of concoction, was brought, in a certain number of days, into a state in which it was ready for expulsion from the body. It was then thrown off by hæmorrhage, by sweat, by alvine discharges; or deposited upon the surface in the form of abscess, or cutaneous eruption: and these eruptions or evacuations constituted the crisis of each fever.

The doctrine thus enunciated by the father of physic is very nearly the same with that which Liebig has been teaching in the nineteenth century. This distinguished chemist ascribes the phenomena which succeed the introduction of certain animal poisons into the blood to a process resembling fermentation. Let me try, in a few sentences, to expound to you his views on this deeply interesting subject.

You know that the brewer excites the fermentation of his *sweetwort* by adding to it a small quantity of *yeast*. Wort is an infusion of malt, and contains sugar and gluten, with other vegetable matters, in solution. Yeast, as I showed you in a former Lecture, is a cryptogamous plant or fungus, which undergoes rapid change and development when placed in a solution of sugar. During this development, and apparently in consequence of it, alcoholic fermentation takes place; the elements of the sugar arrange themselves into new and simpler forms; namely, into alcohol and carbonic acid. If there were no gluten in the wort, this would be the whole of the process; during which the added yeast disappears.

But the presence of gluten makes a prodigious difference. The yeast increases enormously at the expense of the gluten, and mingling with the liberated carbonic acid, rises and floats upon the surface of the fermenting liquid. So that, when the process is completed, there has been produced thirty times as much yeast as was originally added to the wort.

Now this, according to Liebig, is but a type of what happens in other fluids under analogous circumstances. He maintains that (I use the words of his translator), 'a substance in the act of decomposition, added to a mixed fluid in which its constituents are contained, can reproduce itself in that fluid, exactly in the same manner as new yeast is produced when yeast is added to liquids containing gluten.'

Thus the virus of small-pox (which virus is formed out of the blood) causes such a change within the blood as gives rise to the reproduction of the poison from certain constituents of that fluid: and whilst this process is going on, the natural working of the animal economy is disturbed: the person is ill. The transformation is not arrested until the whole of that ingredient in the blood which is susceptible of the decomposition has undergone the metamorphosis.

Liebig shows that similar processes may take place in mixed fluids (and therefore in the blood) without the regeneration of the added substance: just as the fermentation of a solution of sugar is effected by the addition of yeast, without any reproduction or multiplication of the yeast, if there be no gluten in the saccharine solution. In such cases, the disease, which results from, or accompanies, the transformations that occur in the blood, is not contagious: the poison is not renewed. It is thus that certain *miasms* produce disorders which are not communicable from person to person.

In order, then, that a specific animal poison should effect its own reproduction in the blood, and excite that commotion in the system which results from the formation and expulsion of the new virus, it is requisite that a certain ingredient (analogous to the gluten in the brewer's sweetwort) should be present in the blood: and this ingredient must have a definite relation to the given poison.

If this ingredient be indispensably necessary to life, the poison, which transforms and destroys it, is inevitably a fatal poison. May not this be the *modus operandi* of the poison of hydrophobia?

Again, if this ingredient be wanting, no reproduction of the poison takes place; nor, of course, any of those symptoms which are consequent upon such reproduction. The poisonous qualities of the animal substance are not developed. It ceases to be a poison.

And this ingredient, if naturally present, is exhausted and destroyed, for a while at least, by the operation of the poison. Hence, for a while at least, the same disease cannot be again produced by the agency of that poison.

Supposing the ingredient to be one which is not essential to the composition of the blood, and to have been thus destroyed or exhausted, it may never be replaced. Or it may be replaced only after a long interval. In some persons it may never exist at all; or it may exist at certain periods only of their lives. It may even be acquired by unnatural or peculiar modes of living.

All this is possible and plausible. A certain number of peculiar substances do certainly exist in the blood of some men which are absent from the blood of others. In childhood and in youth the blood of the same individual contains variable quantities of substances which are not to be found in it at other periods of life.

This hypothesis of Liebig's offers, then, an intelligible explanation of the curious facts, that certain contagious disorders furnish a protection, temporary or permanent, against their own return; that they have a tolerably definite period of incubation, and run, for the most part, a determinate course; that some persons are less susceptible than others of the influence of these animal poisons, or not susceptible at all; and that the same individual may be capable of taking a contagious disease at one time, and not at another.

Mr. Simon, in his admirable 'Lectures on Pathology,' which I earnestly commend to your diligent study—Mr. Simon, while he scouts the notion of any true fermentation in these cases (indeed Liebig could hardly have advanced the example of yeast in wort otherwise than analogically, just as we all speak, now-a-days, of *zymotic* diseases), adopts in full the hypothesis of some material which, pre-existing in the blood, but not being an essential part of it, combines somehow with the exciting virus from without, to cause both the febrile commotion, and, in consequence of the exhaustion of that material, the subsequent immunity from the same disorder. He even indicates the possible identity of the inbred material with certain 'wastes of the tissues.' 'In infancy (he writes), in early age, and till puberty, there are certain waste materials which never afterwards occur: the temporary cartilages have to waste away, the thymus gland has to decay, peculiar changes referable to the sexual system have to be accomplished, and the effete products of these changes have to be eliminated from the system.' He points to the fact that 'the surfaces and organs most prone to affection in the diseases under consideration are those which are eliminative and defecating: those whose normal products can hardly be retained for any time within the body, much less out of it, without undergoing a fœtid decomposition, which sufficiently stamps them with an excrementitious character. Bowels, skin, kidney, tonsils, are the favourite resorts of the several fever poisons, just as they are the surfaces by which naturally the organic waste of the several tissues is eliminated.'

This curious subject is looked at in a different light by Mr. Paget. 'The *maintenance of morbid structures* is (he says) so familiar a fact, that not only its wonder but its significance seems to be too much overlooked. What we see in scars and thickenings of parts appears to be only an example of a very large class of cases; for this exactness by which the formative process in a part maintains the change once produced by disease, offers a reasonable explanation of the fact that certain diseases usually occur only once in the same body. The poison of small-pox or of scarlet fever being, for example, once inserted, soon by multiplication or otherwise affects the whole of the blood; alters its whole composition: the disease, in a definite form and order, pursues its course; and finally the blood recovers, to all appearance, its former state. Yet it is not as it was: for now the same material, the same variolous poison, will not produce the same effect upon it; and the alteration thus made on the blood or the tissues is made once for all: for commonly, through all after life, the formative process assimilates, and never deviates from, the altered type, but reproduces materials exactly like those altered by the disease: the new ones, therefore, like the old, are incapable of alteration by the same poison, and the individual is safe from the danger of infection.

'So it must be, I think, with all diseases which, as a general rule, attack the body only once. The most remarkable instance perhaps is that of the vaccine virus. Inserted once, in almost infinitely small quantity, yet, by multiplying itself, or otherwise affecting all the blood, it may alter it once for all. For, unsearchable as the changes it affects may be; inconceivably minute as the difference must be between the blood before and the blood after vaccination; yet in some instances that difference is perpetuated; in nearly all it is long retained; by assimilation the altered model is precisely imitated, and all the blood thereafter formed is insusceptible of the action of the vaccine matter.

'But it will be said, the rule fails in every case (and they are not rare) in which a disease that usually occurs but once in the same body occurs twice or more. Nay, but these are examples of the operation of that inner yet not less certain law—that after a part has been changed by disease, it *tends* naturally to regain a perfect state. Most often the complete return is not effected; but sometimes it is, and the part at length becomes what it would have been if disease had never changed it.'

Respecting points so interesting and so mysterious, it is scarcely possible to refrain from speculation altogether. I have laid before you some attempts of able and thoughtful men to explain the main facts of the case: namely, the production of the disease by an animal poison; the prodigious increase in quantity of the specific virus within the body during the progress of the malady; and the extinguishment of the susceptibility of its influence in that individual thereafter. Without adopting either theory with implicit credence in its truth, I hold my judgment in suspense, until evidence more convincing shall appear, or until some better theory than either shall be propounded.

To my own mind, indeed, the whole train of events has always seemed analogous rather to a cycle in the progression of vegetable life. We have the visible and tangible seed, the manifest sowing, the hidden germination; then, the outgrowth and efflorescence, the ripening, the mature seed-time, the reproduction manifold of the original specific germ—every stage in the process of development occupying a definite period of time. Lastly—for here the analogy, though weaker, does not wholly fail—we have the total or the partial, the final or the temporary exhaustion of the soil, even by a single crop, for that particular substance. Sometimes (to continue the metaphor) the soil slowly regains the power to grow the same disorder: we see this in the waning protective influence of distant bygone vaccination. Parasites, animal and vegetable, grow, we know, upon and within the human body, upon its external surface, upon its internal surfaces, even in its fluids. The yeast, in Liebig's illustration, is a plant-growth in a fluid. A similar growth in the living blood—a parasitic growth, it may be—constitutes (we may fancy) the specific disease.

You will find this view of the matter suggested in the writings of Dr. William Budd.

The same idea presented itself to Professor Tyndall's acute mind, when I once brought this subject before him. 'A tree (he writes to me) or a grain crop requires for its existence an infinitesimal amount of mineral matter, without which, however rich the soil, it cannot grow. It is perfectly conceivable that a soil may contain this matter in such minute quantity that a single crop may exhaust it; and this without prejudice to the capacity of the soil as regards other crops. Now may there not, prior to the sowing of the virus, be something analogous in the human system, which a single crop of pustules entirely removes? Some such change is certainly wrought, and I would rather express it in terms of matter than in terms of force. If after one attack of small-pox the system ever becomes receptive of a second, this would be equivalent to the restoration of the requisite mineral matter to the soil.'

It is very certain, and it is not inconsistent with these hypotheses, that the diseases of this group, which are all of them blood-diseases, do often leave permanent traces of their agency upon the general health, even when no local damage is apparent. We hear men say, 'I have never been so well since I had the measles, or the typhus fever, in such a year:' and we hear this without much wonder, knowing that local mischief may lurk within, unrevealed by any legible outward signs. It is more surprising, but it is equally true, that the influence is sometimes for good. The system is disencumbered of some previous impediment to its perfect welfare and harmonious working. Of this mysterious ameliorating influence, which is the rarer of the two, I may give you one example.

A servant of a gentleman living in Belgrave Square was constantly ailing and weakly. His master procured for him, without avail, the best medical advice that London could furnish; and at one time put him into St. George's Hospital, with very small resulting benefit, or change in his condition. At length the man caught small-pox, and had it most severely and dangerously, under the care, in his master's house, of the late Drs. Nevinson and Chambers. His life was despaired of. By and by a large mask of scab fell entire from his face, and he recovered, though frightfully seamed by the disorder. From that time for many years he lived in the enjoyment of thorough health and strength, such as he had not known before the attack of small-pox.

The subtle contaminating effluvia which proceed from the bodies of the sick enter the blood of those who catch the disorder, chiefly, I imagine, from being inhaled into the lungs in breathing. The poison may, perhaps, be capable of being absorbed through the skin: and upon this supposition oil has been smeared over the surface with the view of shutting out the infection of the plague. The contagious matter may be swallowed with the food or drink, and so reach the blood through the medium of the digestive organs. Water is often, probably, the fatal vehicle—or milk diluted with infected water—or milk, or other beverage, rendered poisonous by merely standing in a room where fever patients are lying. Dr. Michael W. Taylor and Dr. Ballard have brought forward strong evidence to show that typhus fever, enteric fever, and scarlet fever, have been imparted by the water or the milk drunk by the unsuspecting subjects of these diseases. The virus may gain direct entrance into the blood; we know that it sometimes does so, for we ourselves insert it, in inoculation of the small-pox. Dr. Francis Home imparted measles by engrafting some of the blood of a person ill of that complaint; and subsequent attempts to excite the disease in that manner have been equally successful. Some rash and unfortunate trials have proved that the plague is communicable by inoculation with matter from the buboes.

Endeavours have been made to estimate the distance to which the influence of different volatile contagious emanations extends. The effluvia in small-pox, measles, and scarlet fever, are the most active; operate, I mean, at the greatest distance. In continued fevers they have a less range; and in the plague the diameter of the infectious circle is probably very small. Some have even supposed that the plague is communicable by actual contact only; but the opposite opinion seems the more likely, namely, that you may *touch* plague patients with impunity (as the first Napoleon is known to have done on a memorable occasion) if you avoid inhaling their breath, or the effluvia proceeding from their bodies.

The most important practical result of the experiments made by Dr. Haygarth and others, for determining the absolute distances to which the power of the contagion extends in different disorders, was this, that *where ventilation is complete*—in other words, where the volatile poison is freely diluted with atmospheric air, the sphere of its operation is very limited.

It is an interesting subject of enquiry, worth glancing at for a moment, how far the power of different contagions is modified by differences of temperature. Small-pox is readily propagated either in hot or in cold regions; in Mexico near the Equator, in Greenland towards the Pole. The plague does not spread when the temperature is below 60° or above 90° Fahrenheit. The vaccine matter loses its property of producing the cow-pox if it be exposed for a certain time to extreme cold, or to a heat of 95°. Typhus fever, measles, and scarlet fever, are said to be of rare occurrence in the intertropical regions. Dr. Henry has turned these facts to useful account in proposing to decompose and destroy certain contagions lurking in fomites, by the operation of artificial heat.

There is still one subject which it is proper that I should notice before I proceed to consider, one by one, the various forms of specific fever. It is a subject upon which within the last three or four years much new light has been thrown, and about which a very lively interest has been re-excited: I mean what is called the *thermometry* of disease.

The essence of fever is increased heat of the body. This is expressed in the very word *fever*, which we get from the Latin *ferveo*. The measures of this increase were heretofore altogether vague and imperfect; having been furnished either by the subjective feelings of the sick person himself, or by the objective tactile feelings of the bystanders: both of which are not only wanting in precision and accuracy, but apt to be even fallacious and deceptive. Yet we all know, from the use of that familiar instrument the thermometer, that degrees of heat are capable of being expressed numerically, and therefore with absolute exactness. What was so long needed for giving a scientific value to the varying temperatures of the human body in fevers, was some method whereby the exact degree of its heat from time to time might be ascertained and recorded. And such a method has now been realised. You may carry in your pocket a small self-registering thermometer, and assure yourself, in a few minutes, of your patient's actual internal temperature. I do not describe here the rules and cautions to be observed in gathering this information. They will be best learned by practising them and seeing them practised at the bedside in your clinical lessons. We owe this great service in the study of disease to Dr. Wunderlich, of Leipsic, whose 'Manual of Medical Thermometry,' translated into English by Dr. Bathurst Woodman, has been published during the present year by the new Sydenham Society. You will find ample proof in Dr. Wunderlich's book of the value of what may fairly be called his invention. Analysis of the records obtained by these means brings to light a series of fixed laws by which the varying degrees of bodily temperature are regulated: laws which are seen at once to be profitable for the diagnosis, the prognosis, and the treatment of febrile disorders. You may thus learn to what species of fever a given case belongs; the stage which it has reached; whether it is running its natural course; to what mode of termination it is tending; may test the efficacy and the value of therapeutic measures; and discriminate between real and only seeming convalescence. The importance of such information in many cases is great and obvious; and (unlike the methods of auscultation and palpation) this method demands no previous or special education of the senses: all that is requisite being carefulness, patience, and a clear eye-sight.

It is a very remarkable thing that the temperature of our bodies in health should vary so little, should be so nearly a constant quantity. The amount of variation at different periods of the day and night, and under varying circumstances, scarcely exceeds one degree of Fahrenheit. $98\frac{1}{2}$° may be reckoned the standard heat. For all practical purposes we may neglect the division of a degree into tenths, and assume the standard temperature, or *blood-heat*, to be 98°.

Take a few more broad and general rules. If the temperature rises to 100°, we know simply that our patient is feverish. The heat of 105° and upwards denotes the existence of a dangerous degree of fever. There is danger also when the temperature sinks to the other extreme, that of collapse—to 95°, for example, a point which is not necessarily of mortal omen, but below which there is always great peril.

Speaking generally, we may say that we have before us a moderate fever when the temperature varies from 101° in the morning to 103° in the evening; of much fever when from 103° to 104°; of high fever when the varying temperatures are above these. Wunderlich calls a temperature of 107° hyperpyretic, and says that it mostly imports a fatal event.

I shall supply some of the teachings of this method as I proceed, in the next Lecture, to a more particular account in succession of the diseases grouped together under the title of Exanthemata. And I shall begin with *continued fevers*; because, although they do not afford the best-marked examples of the collection of symptoms that compose Cullen's definition of the order, yet a right understanding of the practical points concerned in the management of these febrile diseases, will assist us materially towards a just conception of the modifications of treatment that may be required by the rest.

LECTURE LXXVIII.

Typhus Fever. Phenomena of the first and of the second week; Delirium, Mulberry Rash: of the third week; Recovery, or death in the way of Coma, of Apnœa, of Asthenia. Symptoms that precede and usher in those modes of dying. Typhoid Fever; points of distinction between it and Typhus in respect of symptoms, of modes of attack. Rose-coloured spots. Ulcerations of the Intestine. Relapsing Fever. Further differences between these three Fevers.

We hear continually, both in and out of the profession, many different species of fever spoken of. By the public, typhus fever, brain fever, bilious, putrid, low, nervous. And systematic writers are to the full as particular; mucous fever, ataxic, adynamic, gastro-enteric, typhoid, and so forth. Now this complexity of nomenclature is puzzling to the student, and misleads the public mind. Friends and parents ask anxiously what sort of fever the patient has: and medical men themselves often find it difficult to answer them. The subject is really more simple than at first it may seem to be; yet it is not quite so simple as I formerly supposed. For a long time I held, in common, I believe, with most English physicians, that no absolute distinction could be drawn between the various forms of continued fever met with in this country. I was indeed aware—it was impossible to be long engaged in hospital practice without becoming aware—that different epidemics presented very striking features of diversity and even of contrast—so as to suggest the frequent suspicion that the maladies composing them might be specifically distinct. Still, taking into account their strong general resemblance—finding that these like, yet varying, forms were often more or less intermixed—fancying also that other distempers fluctuated in type, that within the last twenty years all acute disorders had assumed, in this town at least, an altered and a more asthenic character—I conceived, and I taught, that the differences to which I have adverted in the aspect and phenomena of continued fever, depended more upon what is called the epidemic constitution—that is to say, more upon an acquired disposition of the human body produced by some obscure general influence, and therefore affecting the entire London community—than upon any essential difference in the nature of the disease itself, or in the virus from which (as I believed) it sprang.

But I think so no longer. The Jenner of our time, with patience and sagacity worthy of the great name he bears, has traced out plain lines of division between two or three forms of continued fever, and especially between two forms which had been chiefly confounded together, and which we now call respectively *typhus* and *typhoid* fever. In the affinity of these names is still implied the similarity of the two disorders; but Sir William Jenner has shown, by evidence which quite satisfies my mind, that they differ notably and constantly in their symptoms and course, in their duration, in their comparative fatality, in the superficial markings which respectively belong to them and which warrant our classing them among the exanthemata, in the internal organic changes with which they are severally attended, and (what is the most important, the most conclusive, and the most difficult point to determine of all) in their exciting causes. For some reasons it may be regretted that names so much alike should have been given to diseases which are really distinct; yet even this may be not without its advantage, if it keep the attention of the observer on the alert to discriminate between the two kindred disorders. For my own part, I could have wished that typhoid fever had been named, as Dr. William Budd names it, *intestinal* fever. The synonyme *enteric* fever is now becoming common.*

A third form of continued fever, called the *relapsing* fever, is readily distinguishable by well-marked features of its own, when once its separate existence has been realised.

In sketching the main phenomena of continued fever I shall keep in view *typhus* as its typical form—and afterwards point out the characters which chiefly distinguish this from the *typhoid* disorder. In this way I hope to spare you the tiresomeness of listening to a detailed description of each of two diseases, which, after all, though perfectly distinct, have very much in common.

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* It is a bare act of justice to record that so early as the spring of 1840, a paper was read before the Parisian Medical Society, by Dr. Alexander P. Stewart, in which the main distinctions between typhus and typhoid fever were clearly set forth. And in 1842 Dr. Bartlett's excellent book on 'Fever' testifies to the same distinctions, as he had noticed them in the United States. The question of identity had also been raised, and answered in the negative, by Louis, in 1841.

Typhus fever does not always commence in the same way. It may happen that for several days before the disease assumes its peculiar and proper aspect, and before the patient is rendered unable to pursue his usual occupations, he is affected with certain morbid symptoms which may be considered premonitory of the fever; so that it is sometimes difficult to mark the precise beginning of the very disease. These preliminary symptoms result apparently from an altered condition of the *nervous system.* M. Claude Bernard regards fever as a purely nervous phenomenon—a transient and incomplete paralysis of the sympathetic system, which, according to him, is the sole vaso-motor apparatus. The expression of the patient's countenance alters; he becomes pale, languid, and abstracted. Those about him observe that he is looking very ill. He is feeble, and soon tired; reluctant to make any exertion of mind or body; listless, dejected, and apprehensive often of some impending evil. He loses his appetite; his tongue becomes white and inclined to tremble; his bowels are irregular, often confined, rarely affected with diarrhœa; his senses lose their natural delicacy. He has uneasiness or wandering pains in various parts of the body; and occasionally there is some giddiness: drowsiness perhaps during the day, and unsound and unrefreshing sleep at night. To collect all this into one expressive word, the patient evidently *droops.*

Much more commonly, however, these preliminary movements are altogether wanting: the disease sets in suddenly. Its regular onset is marked, very frequently indeed, by a *shivering fit.* Another common phenomenon at the period of the invasion is severe *headache*; pain or aching across the forehead, rarely in the temples, never at the back of the head. The pain is apt to begin during the night, or in the early morning. But this is not constantly the case; nor is the pain a constant symptom. Sometimes there is a sense of heaviness and vertigo rather than headache. You will perceive also, even when there have been no premonitory circumstances, that symptoms arise, even thus early, which belong to the nervous system, and which denote some disturbance and alteration in the functions of sensation, thought, and voluntary motion. They are comprised under the general phrase '*febrile oppression,*' and they are different from what we notice when pyrexia supervenes upon inflammation. You will obtain a clearer notion of what this term, febrile oppression, means, by watching at the bedside of a single patient in this disease, than by any description that I can give you. There is great inaptitude for exerting the power of thought or of motion. The expression of the face is dull and heavy, absent, puzzled; its hue thick and dusky. The patient presents very much the appearance of a person made stupid by drink; and he staggers a little when he attempts to walk. The muscular power is sensibly enfeebled: sometimes he will struggle against this; but in a few hours, or in a day or two at furthest, he takes to his bed.

In order the more clearly to portray the course of continued fever, I shall divide it, as others have done, into periods—weekly periods. Not that there is any such period of seven days allotted to particular symptoms; but that in the simplest forms of the disease, when it runs its course most evenly and favourably, and therefore, we may suppose, the most *regularly* also, there is a succession of different *sets* of symptoms, which occupy each *about* that space of time; nearly enough to allow of my taking it as a help to the better *describing* the disease.

Many of the symptoms which occur during the first stage of the disorder—during the first week, we will say—are such as belong to the sanguiferous system. The pulse becomes more frequent than in health, there is increased heat of skin, and thirst; headache, and throbbing of the temples. The pulse varies considerably in different cases. Generally, I say, its frequency augments; but sometimes it is even slower than is natural. The acceleration of the pulse is greatest (*cœteris paribus*) in those constitutions which are the most irritable. In young persons, in females, and in weak and delicate males, it will often rise, soon, to 120: while in stronger adults, it does not so early attain its maximum of frequency, and perhaps does not exceed 100 throughout the whole course of the disease. Should the pulse in any instance reach 130 or 140, the disease is severe: and the majority of such cases prove fatal. The absolute frequency of the pulse is not, however, of so much importance, in typhus fever, as its steadiness. If it shift often from one number to another, *that* affords a worse prognostic even than its being very frequent, provided it keeps at the same standard. It is almost always soft, soon becomes weak, and gradually gets weaker. Sir William Jenner affirms that in *typhus* fever, running an uncomplicated course, the pulse rises slowly in frequency to a certain point, preserves that rate of frequency for a variable period, and then as slowly falls: while in *typhoid* fever it rises and falls in a most irregular manner, to-day 120, to-morrow 90, the next day 120—without appreciable cause or consequence. A sudden and considerable increase in the frequency of the pulse should therefore, in typhus fever, suggest the probability of some inflammatory complication. The skin, during this period, is generally hot and dry, and it *feels* to a bystander very hot and *pungent*. According to Wunderlich, the *thermometric* temperature of the body in typhus fever rises suddenly, especially when the disease commences with a rigor, to about 104° Fah. the first evening; and gradually ascends, with slight morning remissions, to 105° or 105½° by the fourth evening; when, or on the next day, or the next day but one, a turning point is reached, marked by a trifling decrease in the temperature, which continues to diminish on the seventh or eighth day. After this it rises afresh for a day or two, and begins again slowly to descend. On the twelfth day a more decided fall takes place, and the normal temperature is reached about the fourteenth day.

The period of the disease during which the temperature begins its progressive decline is called the period of *defervescence*. Wunderlich states, that in this disease it most commonly occurs between the thirteenth and seventeenth days; less frequently between the twelfth and thirteenth; and still more seldom at an earlier date: it is generally rapid.

What I have now said applies to typhus when it runs a smooth and favourable course. In severer cases the temperature may be much higher, even early; even above 106°, according to Wunderlich; and the transient remissions at the end of the first week are wanting.

In fatal cases the temperature often rises just before death.

The thirst in typhus is usually troublesome for the first few days. The tongue becomes clammy or dry; sometimes it is clean and smooth; more often furred: its edges and tip may, perhaps, be red, while a white fur either covers entirely the central part of the tongue, or is divided by a straight brown streak which occupies its middle portion. This brown streak is often the first step to dryness and blackness of the tongue.

Slight and transient tenderness of the abdomen is not uncommon during the same period. Sometimes the belly is full and resonant, without being sensibly distended; sometimes it is even concave; most frequently of all it retains the natural qualities of health.

There is evidence, not seldom, of a slight affection of the membrane lining the air-passages, from nearly the first: some notable quickness of respiration, and some diffused rhonchus and sibilus, audible through the stethoscope.

And among all the indications of derangement in the circulating system, the symptoms that relate to the nervous centres remain conspicuous. The aspect of the patient is characteristic: the features are fixed and inexpressive; or expressive merely of apathy and indifference. If spoken to briskly, he responds; and although his sensibility seems blunted, his answers are, as yet, rational, and to the purpose. Delirium does not come on, in general, till towards the end of the first week. The muscular power is greatly depressed. The patient lies on his back, motionless; he sleeps but little, waking often; and the short snatches of repose which he seems to get are disturbed, apparently, by uneasy dreams; and he *fancies*, and perhaps says, that he does not sleep at all: or he lies with his eyes open, evidently awake, but insensible to all that is going on around him. To this condition Sir William Jenner applies the term *coma vigil*. It is much more common in typhus than in typhoid fever, if it be not peculiar to the former: and it is of most evil omen. Sometimes, even during the first stage of the disorder, the prostration of strength is so great, or the tendency to stupor and indifference is so marked, that the stools are passed under him as he lies in bed, without any apparent endeavour on the part of the sick person to prevent it; and without any notice of his wants being made to his nurse. The urine, during the same stage, is scanty, high-coloured, and ill-smelling often: and not unfrequently it contains albumen. It is seldom, except in very malignant forms of typhus fever, that death takes place during this its primary stage. Towards the end of the first weekly period, the eruption which is peculiar to typhus fever commonly begins to show itself: but this is sometimes postponed to the next stage; and I shall describe it in connection with other changes that are apt to occur in the second week of the disorder. These are as follows:—The pulse becomes more frequent, weaker, and more compressible. The tongue grows drier and browner. More sordes, and of a darker colour, accumulate on the teeth and lips. But the symptoms that relate to the nervous system are often still the most prominent. The patient generally loses his *headache*. His voluntary movements, however, become very much weakened, and are sometimes exercised irregularly. The posture which the patient in this stage almost always assumes is significant of this weakness; he lies on his back, and he *sinks down in the* *bed*, slips towards the foot of the bed. He is unable to make or bear that degree of voluntary exertion which would be necessary to place him upon his side. Hence we hail it as a good omen—because it is an indication that the patient still retains some strength—if we find him lying on his side, or even on his back with his knees drawn up. Other proofs of muscular debility, approaching to palsy, are apt to present themselves. The voice becomes feeble; the patient can scarcely utter an audible sound. Perhaps he is unable to swallow. This is a very bad symptom, though it is one that has been recovered from. Sometimes it seems that the power of deglutition is not lost, but the sick man is too listless to try to swallow: or the dry and parched state of his tongue and throat render it difficult and painful for him to attempt to do so. He lies with his mouth open; and breathing thus through the mouth tends to dry the tongue. Hence it is well to desire him to swallow a mouthful or two of water, and so to moisten his tongue, before you decide upon the state of that member, or upon his facility of deglutition. Often, especially in bad cases, there are little convulsive startings of the tendons (*subsultus tendinum* is the technical name of the symptom), and other irregular and involuntary actions of the muscles: tremulous movements, especially of the tongue and of the hands; and sometimes the sick person is unable to put out his tongue at all. There are two symptoms which, in the majority of instances, present themselves most obviously towards the end of the first or in the second week of the fever, and which deserve your particular attention: I mean delirium, and the eruption which belongs to the disease.

The delirium is peculiar. The patient wanders, at first, in the night only; and the delirium commonly shows itself on his awaking from disturbed sleep. Sometimes he is desirous of getting up, and talks incessantly and earnestly in a loud voice, and can only be kept in bed by the imposition of some restraint. Usually, however, his rambling is of a tranquil kind, and without agitation. His mind seems elsewhere: he is inattentive to all that passes around him; but he lies still, muttering disjointed words or sentences, like a man talking in his dreams. From this state of *typhomania* the patient may sometimes be roused by loud speaking addressed to him, or by the sight of a strange face; so that, though incoherent and delirious just before, he may become collected when his medical attendant enters the room. But he presently relapses. During the delirious state there is a great deficiency of sensation, and insensibility to impressions. The patient is deaf. This deafness you may hear spoken of as being a good omen, or favourable sign; but it is only so by comparison: it indicates a condition of brain less perilous than its opposite, in which the sense of hearing is morbidly acute. Imperfection or loss of vision is much rarer, and much more alarming, than deafness; yet the eye is generally dull—unlike the brilliant eye of acute phrenitis; it corresponds with the expression of the countenance, which is perplexed rather than wild. Sometimes, however, as the disease advances, black spots, like flies on the wing, *muscæ volitantes*, appear before the patient's eyes; in consequence, it is presumed, of partial insensibility of the retina. The patient attempts to grasp or catch these in the air, or to pick them from the bed clothes. This is called *floccitatio*. After these symptoms recovery is not common. 'After I saw him *fumble with the sheets*, and play with flowers, and smile upon his fingers' ends, *I knew there was but one way*,' says Dame Quickly, speaking of Falstaff's death. The mouth and tongue are dry; yet the patient no longer complains of thirst. The taste, the smell, the sense of touch, are all impaired. One trivial yet expressive mark of this dulness of the senses, mentioned by a recent author, is that the flies crawl, unnoticed, over the patient's face. Even external ulceration may occur, especially about the hips and sacrum, and go on to gangrene, without eliciting any complaint of pain from him. He seems altogether careless about the issue of his disorder. If, at this period of the fever, you ask him how he does, he will probably declare that he is quite well. This also is of bad omen. I have already alluded to the involuntary passage of the fæces: this may depend, in part, especially in the advanced stages of the disorder, upon debility or paralysis of the sphincter muscles. The urine also dribbles away frequently; and these are points which must always be looked after; first, for the sake of keeping the patient as clean and dry as possible, the irritation of the urine and fæcal matters tending to produce sloughing ulceration; and, secondly, with the view of preventing the bladder from becoming unduly distended. Retention of urine, and all its bad consequences, may otherwise occur. It is a good general rule, therefore, to examine the hypogastric region every day with the hand: and also to ask to *see* the urine, not for any purpose of prognosis, but to ascertain that it is regularly discharged.

The eruption which is peculiar to and distinctive of typhus fever is called by Sir William Jenner the *mulberry rash.* I follow his account of it with perfect confidence in his fidelity as a describer. It commences usually from the fifth to the eighth day of the disease: sometimes later; occasionally earlier. After the third day of the eruption no fresh spots appear. In this particular it resembles, as we shall see hereafter, the eruption of small-pox. It disappears in the course of the third week of the disorder.

The characters of the rash vary with its age. It is never papular, but consists, at first, of very slightly elevated spots, of a dull crimson colour. Each spot is flattened on its surface, irregular in outline, fades insensibly into the hue of the surrounding skin, and disappears completely under the pressure of one's finger. The larger spots, more irregular than the smaller, appear to be formed by the coalescence of two or more of them.

In two or three days these spots undergo a marked change. They are no longer elevated, become darker, dingier, and rather more defined; and now they fade only, without disappearing, under pressure. From this condition the spots, in most instances, grow paler, pass into faintly marked reddish-brown stains, and finally vanish. Sometimes a third stage is reached. The centres of the spots take a dark purple colour, and remain unaltered by pressure, although their circumferences fade; or the entire spots change into true petechiæ—i.e. into spots of a deep crimson or purple colour, quite flat, with a well-defined margin, and unaffected by pressure. These petechial spots result from a minute extravasation of blood beneath the cuticle. They occur most frequently on the back, at the bend of the elbow, and in the groin.

The spots or stains composing this mulberry rash are generally very numerous, set closely together, and sometimes they almost cover the skin. They are usually spread over the trunk and extremities, occasionally over the trunk only, now and then they are seen on the face. Each spot remains visible till the whole rash disappears. To this rule there is one exception. The eruption sometimes shows itself first on the backs of the hands, and leaves those parts within twenty-four hours. When numerous, the spots have not all the same depth of colour; some being paler than others, and appearing as if seen beneath the cuticle. Hence the surface has a mottled look. Our wards at the Middlesex Hospital in 1838 were full of this form of fever. Not a case, I believe, presented itself without these spots. We spoke of it familiarly as the *spotted* fever; or (from the resemblance the rash bore to that of measles, hereafter to be described) as the *rubeoloid* fever.

The spots which I have been describing, those at least which had reached their second stage, have been found to remain visible upon the surface of the dead body, when death has ensued before the natural time of their disappearance. The petechial spots also are persistent. And after death, as well as during life, the spots on the undermost parts of the body are the darkest in colour. This difference seems to be owing to their depending position.

Finally, with respect to the mulberry rash, Sir William Jenner states that in patients less than 15 years old it is mostly either absent, or pale in hue, and scanty in quantity. And the mortality from typhus at this early age is proportionally trifling, not more than 2 or 3 per cent.: while in persons more than 50 years of age it is about 56 per cent., and in them the rash is always present, and ordinarily dark and abundant.

There is another eruption described by the French as *occurring* in this disease without being *peculiar* to it. In this country it is now rare; but when the hot plan of treatment was in vogue, it used to be very common indeed here, in various febrile complaints: and it was, and is, apparently connected with copious sweating. *Sudamina*, the vesicles composing the eruption are called. They are small, hemispherical, transparent elevations of the cuticle, containing a clear watery fluid. The vesicles are from a quarter of a line to half a line in diameter; they have no red bases; and they are so perfectly pellucid, that when you look upon them in a direction perpendicular to the skin on which they stand, they may readily elude observation. Viewed sideways, they present bright surfaces, and look like so many drops of water, and you may feel with your fingers that they *roughen* the part affected with them. These sudamina are mostly met with on the thorax, along the sides of the neck, and about the axillæ. By degrees, the limpid fluid disappears, and they shrivel up; the cuticle becomes wrinkled, and dries into a whitish powder.

Sir William Jenner is of opinion that age has something to do with the occurrence of these miliary vesicles; and that they are rarely seen on persons who are more than 40 years old. They remain after life has departed.

It is in the course of this second week of the disease that, in typhus fever, death is most apt to take place. Among 25 fatal cases noted by Sir William Jenner, 9 deaths only occurred after the 15th day; not one after the 20th.

As the disorder approaches its fatal termination, symptoms which are not unfitly called *putrid* very often show themselves: a peculiar fœtor is exhaled by the patient's body; his tongue becomes dry, black, and fissured; and he is perhaps unable to protrude it when asked to do so; his teeth are covered with dark sticky sordes; sloughs form from the mere pressure of the bed on which he lies; in extreme cases the toes have mortified; and Dr. Roupell relates one terrible instance in which both legs rotted away to the bones, which it became necessary to saw through: yet this patient recovered.

I should have told you that, besides the fœtor just mentioned, which is probably caused by commencing decomposition of the bodily tissues, typhus fever, after the first week, has, like small-pox, a characteristic odour of its own, by which nurses learn to know it. It is likely, as Dr. Murchison suggests, that the vapour which imparts this smell, imparts with it the typhus poison.

During the third week of typhus fever, the patient's chance of recovery improves; unless, indeed, some local mischief which pre-existed, or which has sprung up during the course of the fever, shuts out or obscures this more favourable view.

When the disorder is about to end favourably, the more formidable of the symptoms diminish and abate; often almost suddenly: so that, as Dr. Stewart remarks, we might almost say of the patient, in Scripture phrase, 'at such an hour the fever left him.' After perhaps a prolonged and quiet sleep, he wakes up an altered man; begins again to attend to questions that are put to him; the air of stupor which has hung over his countenance clears away; he once more shows an interest in what is going on around him; the temperature of his skin becomes more natural; the tongue moist and cleaner at its edges; appetite returns; and the frequency of the pulse subsides. The evacuations from the bowels gradually regain their natural qualities; and the patient is aware when the necessity for passing them or for emptying his bladder arrives, and he gives notice, or asks for assistance. Generally, at the same time with these tokens of improvement, the emaciation which has taken place becomes remarkably conspicuous; perhaps it is the more observable on account of the patient's resuming a more natural expression of countenance.

In many instances, however, the amendment is so gradual that we can scarcely say when it begins. Sometimes the favourable crisis is preceded by an aggravation of most of the former symptoms, and a marked increase of the general distress. This is a very curious circumstance: and it did not escape the notice of our great dramatist:

> Before the curing of a strong disease,
> Even in the instant of repair and health,
> The fit is strongest. Evils that take leave,
> In their departure most of all show evil.

Certain evacuations are also sometimes observed to accompany or to be connected with the favourable change; and the most common of these is the evacuation of sweating.

When convalescence has once fairly begun, it goes on rapidly, and recovery from typhus fever is mostly both early and complete.

On the other hand, when the disease is about to terminate in death, that event may take place in different ways; in either of those modes, in short, which I took some pains to distinguish in the earlier part of this course of lectures. I told you then that I had been taught the importance of studying the tendency to this or that mode of dying, in reference especially to fever, by Dr. Alison. My own experience has since sufficiently approved to me the wisdom of his teaching. Cullen inculcates the necessity of 'obviating the tendency to death.' To do so, we must ascertain the direction of that tendency. We do not so much *cure* these exanthematous maladies, as keep our patients alive while they are recovering. If we would prevent their dying, we must know in what manner they are in danger of dying.

The most common mode of death in typhus fever is certainly that of *coma*. The organic life survives the animal life. The muttering, half-conscious, dream-like stupor, from which the patient may be roused for a while, becomes, by degrees, more profound, and death begins at the head. This mode of death, occurring in the second or third week of the fever, is associated, frequently, with the symptoms of putrescency already described. But as the stupor deepens, the pulse generally grows weak, and the extremities become cold. So that death does not come purely in the way of coma; but we have a compound of coma and asthenia, in which the coma takes the lead.

Now coma may result from at least two different kinds of cause. One cause is pressure, which is mechanical. Another, which is probably chemical, is the circulation of some noxious or narcotic substance (such as opium) in the blood. And there are, doubtless, many physical conditions of the nervous mass itself which are capable of arresting the cerebral functions, and producing coma. To which kind of cause are we to ascribe the stupor that supervenes during the progress of typhus fever? That is an interesting, and in reference to practice, an important, question.

Physicians have diligently attempted its solution, by examining the dead brain. I cannot tell you how often I have looked, and looked in vain, for some palpable disorganisation, or some effusion implying pressure. All who are familiar with the dead-house of a hospital are aware that this fruitless search for some physical explanation of the comatose state which had preceded death by fever, is of very common occurrence.

The unnatural conditions that have been sometimes noted are—slightly diminished consistence of the substance of the brain; congestion of its blood-vessels, marked by red points on its cut surface; undue fulness of the vessels of the dura and pia mater, with, occasionally, thin coagula or films of blood in the cavity of the arachnoid; scanty effusions of thin watery liquid in the lateral ventricles, or in the meshes of the pia mater; and diminished cohesion between the membranes and the surface of the convolutions, such as admits of their separation with unusual facility. Now, to what conclusion do these facts lead us? Why, plainly to the conclusion that those pathologists are in error who maintain (as Dr. Clutterbuck did, for whose experience and talents I always entertained a sincere respect) that the essence of continued fever is *inflammation of the brain*. Not only do we fail to discover, in many instances, any traces of inflammation, upon inspecting the dead brain, but we find that, during the life of the patient, measures which would be likely to aggravate any inflammatory mischief—strong stimulants, for example, wine or brandy—do actually and obviously, in cases innumerable, relieve the comatose symptoms, and benefit the patient. The inference seems unavoidable, that the coma, in such cases, has some other cause than that mechanical pressure which arises sometimes from the effusion of fluid upon the surface of the brain, or within its ventricles; and that other cause is supplied by the poisoned blood. Here again we may adopt the pathology of Shakspeare:

> The life of all his blood
> Is touched corruptibly: and his pure brain
> (Which some suppose the soul's frail dwelling-house)
> Doth by the idle comments that it makes
> Foretel the ending of mortality.

In some malign epidemics the nervous system is overwhelmed at once, in the very outset, by the force of the poison. The patient becomes stupid or bewildered; his surface is cold, clammy, purplish, and his pulse feeble: the coma rapidly augments, and death may ensue within twenty-four hours. We sometimes see this fearful train of symptoms in small-pox; and still more often and more strikingly in the worst forms of scarlet fever. I believe that in these cases there is no deviation, cognisable by our senses, from the healthy texture and appearance of the parts within the skull.

When the coma is associated with a scanty secretion of albuminous urine, it may be of uræmic origin.

Nevertheless, there sometimes is, in these fevers, actual inflammation of the brain or of its membranes: but this is an incidental complication. We conjecture that, in addition to the influence of the poison upon the nervous system, there may be a low degree of inflammation going on within the head, when we find it externally hot, when the patient has flushed cheeks, and a vascular eye, and when the dull headache, instead of ceasing, as it ordinarily does upon the supervention of delirium, continues, and is complained of. And occasionally we recognise still more distinctly the outward signs of encephalitis—severe pain in the head, high and fierce delirium, intolerance of light and of sound, with much heat of skin, and a hard pulse. When coma succeeds such symptoms as these, we naturally ascribe it, in part at least, to the effects of the inflammation: and rightly, for we find traces of inflammation after death; considerable serous effusion into the cerebral ventricles; shreds of coagulable lymph upon the membranes; and more rarely suppuration. I suspect that genuine encephalitis, which is of course attended with pyrexia, is sometimes mistaken for continued fever with intercurrent inflammation of the brain. Great attention, and some skill and judgment, are required for discriminating those cases of fever in which such inflammation occurs, and for directing the appropriate treatment.

The death in typhus fever by *apnœa* is certainly much rarer than that by coma: yet it is not very unfrequent. It often mingles itself with the death by coma. From the earliest period of the fever we may, in most cases, notice some increased quickness of respiration, which is not entirely owing to the mere fever, or to acceleration of the circulation; for the ear, when applied to the parietes of the chest, discovers rhonchus, and sibilus, at least. Frequently there is considerable dyspnœa for some hours, or for a day or two, before death: and this may be apparent only, in consequence of the stupor; or it may be real, and proceeding either from more or less of pulmonary collapse, or from a low form of pneumonia, which, by interfering with the due arterialisation of the blood, may aggravate, or even give rise to the coma: and such pneumonia is apt to be masked by the fever; declaring itself by none of the ordinary symptoms of cough, rust-coloured sputa, or pain in the thorax. The inflammation, thus latent, is discoverable, however, by the sense of hearing.

What account does dissection give us of the condition of the lungs after death from continued fever? Why, the most notable, and probably the commonest morbid state is that kind of condensation which indicates collapse of the lungs; and it is met with chiefly at their posterior part and in its diffused form—sometimes, however, dispersed through them, and in its lobular form. M. Louis found this change, which he calls carnification, in 19 out of 45 instances of death from typhoid fever. It was observed by Dr. Wm. Gairdner in the lungs of many of those who fell victims to the Edinburgh epidemic of 1847. It occurred in each of the three species, but more rarely in the relapsing than in the typhoid and typhus fevers.

As the bronchial tubes are very apt to be clogged with viscid mucus, while the muscular power of the patient is so much reduced by the disease as greatly to impair the force and effectiveness of the acts of inspiration, you will not be surprised that pulmonary collapse should often accompany the course, and aggravate the danger of continued fever. Another unnatural appearance met with in the pulmonary substance is engorgement: a state similar to that which occurs in the first stage of pneumonia. But here the engorgement is probably in a great measure mechanical, and takes place during the last few days of the patient's life. As the vital powers diminish, the laws which govern the physical world resume their empire. The fluids, and the blood especially, accumulate in the most depending parts of the viscera; and the lower and hindermost portions of the lungs in particular become loaded. But besides this, it is not unusual to find large portions of the lungs in a state of hepatisation, and even infiltrated with pus. Less frequently, and in those cases principally in which the putrid symptoms have been most marked, the lung passes into a gangrenous state.

The mode of death by *asthenia* or syncope in typhus fever is not very common *as existing by itself*; but it is *often* combined with one or both of the other two modes. The muscular tissue of the heart is not unfrequently softened. Its sounds, during life, especially the first sound, are apt, for that reason, to be feeble. Death beginning at the heart is, however, more frequently seen in the *typhoid* disease; to which I may now turn.

The main features, as I have endeavoured to paint them, are the same, or very similar, in both of these two allied diseases. It remains for me therefore to dwell on those only which chiefly serve to discriminate them.

In the first place, then, typhoid fever commences more often insidiously, and with premonitory symptoms—more gradually—than typhus. It is often difficult to specify the exact day on which the illness began. The patient is later in taking to his bed.

The countenance of the patient in typhoid fever, though heavy and oppressed, differs remarkably from that of the sufferer under typhus: its flush is less dusky, it expression less dull and stupid, more anxious, less apathetic; and sometimes, on the occurrence of delirium, it is even vivacious. The delirium comes on later, and is decidedly more active; and the patients are more disposed to try to get out of bed. Sometimes, however, there is no delirium from first to last.

In typhoid fever, pain of the head is almost always one of the earliest symptoms; not acute pain, but dull, lasting several days, and ceasing if delirium or stupor ensue. Unless questioned about it, patients seldom *complain* of this pain.

A striking characteristic of typhoid fever, and very important in relation to its treatment, and to an ulcerated condition of the intestines which I shall presently describe, is the prevalence of diarrhœa. Often this is an early symptom; sometimes it is postponed to the latter part of the first, or the beginning of the second week. It is either spontaneous, or it continues after the operation of a dose of purgative medicine.

Pain in the abdomen frequently precedes and accompanies it. Whenever it occurs, the stools are for the most part loose, frequent, and fœtid; and either dark in colour, or of a yellow-ochre appearance, like pea-soup somewhat. They are alkaline also, whereas in health the fæces are always acid. If you make pressure upon the abdomen, you will find it unnaturally hard and resisting, as though its walls were made of pasteboard, tympanitic, often very much distended; but, whether large or not, Sir William Jenner states that 'its shape is invariably the same, and somewhat peculiar. Its convexity is from side to side, and not from above downward. The patient is never pot-bellied, but tub-shaped; the cause probably being that the flatus occupies the colon, ascending, descending, and transverse.' Frequently, uneasiness is manifested when pressure is made on the belly, particularly over the cæcal region: and another symptom, not commonly met with in other diseases, is usually noticeable in the first stage of this, viz. a slight gurgling movement, evidently from the intermixture of liquid and gas within the bowel, which movement becomes audible, or palpable to the hand, upon pressing the same region. This symptom is still more common in the more advanced stages of the disorder. It is of rare occurrence in *typhus* fever.

As the disease proceeds, so does the diarrhœa; from three to six stools, or even more, occurring daily. When they take place involuntarily, when they are passed into the bed without notice on the part of the patient, they add materially to his danger by the irritation and the sores which are apt to result from their contact with the skin. There is seldom much pain of the abdomen now complained of by the patient; but if you make pressure, especially about the situation of the cæcum, you may often remark that he winces, or that a transient expression of suffering passes across his features. The character of the evacuations remains the same, and is in itself distinctive of the disease; thin, yellowish, ochrey, like pea-soup. When, in fever, such stools persist day after day, and several of them every day, you may safely infer that there is ulceration of the bowels, although there should be no pain complained of even when the abdomen is pressed.

And the same conclusion will become still more certain when *hæmorrhage* from the bowels occurs, as it is apt to do, in this stage of the fever. It often takes place unexpectedly, sometimes in considerable quantity, and rapidly exhausts the patient; or it recurs at intervals to a smaller amount, wasting his strength as surely, though more slowly. The bleeding is probably owing, in general, to the division or opening of some of the mesenteric bloodvessels by the ulcerating process which I shall more fully describe by-and-by. This is not, however, a necessary consequence of the ulceration; for the vessels are usually obliterated previously to their erosion. Sometimes blood may be thus poured into the bowel without being voided. Andral relates a case in which a man died suddenly and unexpectedly at an advanced period of typhoid fever. Large clots of black blood filled the lower two-thirds of the small intestines, which were crowded with patches of ulceration. No part of the blood had passed the valve of the cæcum.

Hæmorrhage from the bowels may occur in continued fever in another way: in connexion with other putrid symptoms, petechiæ, purple spots, bruiselike blotches, and extreme depression of the vital power. In these cases the hæmorrhage is strictly of a passive kind, and it is a symptom of the worst augury. Like those effusions of blood from the same parts that happen in scurvy and purpura, it depends upon a morbid condition of the blood. This is no matter of speculation, for by this time the sensible qualities of the blood are manifestly changed; its natural tendency to coagulate when withdrawn from the body is diminished, the crassamentum is large and loose, and fills the cup, and sometimes is rather an incoherent sediment than a clot. But hæmorrhage from this cause belongs rather to *typhus* fever, and in that species of fever it rarely happens from any other cause. In typhoid fever it is not so discouraging a symptom. Some writers have affirmed that the hæmorrhage may even be beneficial. Whenever it is at all copious, it is pregnant with danger. Dr. Murchison says, 'Although I have known patients recover after profuse hæmorrhage, I have never observed the slightest benefit from it.'

Another distinctive mark between typhoid and typhus fevers, is the character of their respective eruptions. That of the latter I have already described. It is strikingly in contrast with the eruption of the former.

The typhoid eruption is papular, or pimply. It consists of little circular or lenticular spots of a bright-rose colour, which fades insensibly into the hue of the neighbouring skin. They are slightly elevated, with round heads which seldom or never become vesicular, nor petechial. From first to last these spots disappear completely under pressure, and reappear when the pressure is taken away. Each papula lasts about three days.

Others follow. Ordinarily the number present at one time is from six to twenty. Occasionally there is only one. Sometimes there are more than one hundred. They occur on the breast and belly, rarely on the limbs or face.

These spots begin to show themselves, generally, during the second week of the disease; and fresh spots come out every day or two till the third week, in the course of which they cease to appear, except in cases of relapse, when they also may recur with the other symptoms. This eruption of lenticular spots in successive crops is highly diagnostic of typhoid fever. Sir William Jenner holds that this species of fever is over by the thirtieth day, since, under ordinary circumstances, no fresh spots are seen after that day. Of course the *illness* may continue much longer—protracted by the effects of the fever, or by pre-existing local complications.

The spots peculiar to typhoid fever do not remain visible on the dead body.

It may not be superfluous to caution you against mistaking *flea-bites*, which are common to nearly all our hospital patients, for this specific eruption which is peculiar to fever patients. The round red stain, with a dark point for its centre, sufficiently distinguishes the mark of the insect from the rose-coloured spot of the disease.

It is stated by Dr. Murchison, whose book on fevers is a rich mine of learning, and of practical experience, that the pupils of the eyes are contracted in typhus and dilated in typhoid fever.

There are certain differences that require to be noted in regard to the appearance of the *tongue* in the two diseases. It is oftener moist throughout the disease in typhoid than in typhus fever; and when dry, more frequently red, and as it were glazed. Generally, if brown at all, it is of a yellowish instead of a blackish brown. Sir William Jenner says, that 'the small dry tongue, with red tip and edges, smooth, furred of a pale brownish yellow, and fissured, the surface seen between the fissures being of a deep red—may be considered differentially as a diagnostic sign of typhoid fever.'

With respect to the comparative *duration* of the two diseases, the same observer found that the average duration of the fatal cases of typhoid fever seen by him was twenty-two days, of typhus fever fourteen days. 'Half the cases of typhoid fever survived the twentieth day of the disease. Not a single case of typhus fever survived the twentieth day.'

And it is in the more protracted cases of typhoid fever that death is chiefly apt to occur in the way of *asthenia.* In one most painful case witnessed by me, the first alarming sign was a sort of hissing respiration, accompanied, at the base of both lungs, with slight fine crepitation which gradually ascended, invading more and more of their substance. In some instances death seems to take place from mere *debility of the heart*, there having been no pulmonary embarrassment, and the head having remained clear. Dissolution, in such cases, is preceded by those symptoms of debility which have been already described. The pulse becomes small, and weak, like a thread; the patient lies on his back, and sinks down in the bed; the features sharpen; the eyes are hollow, and dim as though glazed; the sphincter muscles fail to contract; the extremities grow cold; cold sweats appear on different parts of the body; and at length the heart ceases to beat, and the patient to exist.

Death occurring in this manner does not, I say, occur *early*. It happens at an advanced period of the disease. It is noticed sometimes in persons who have been largely bled, or too actively depleted at the commencement of the fever; and in those who have suffered a good deal from *diarrhœa.* In short, as death in continued fever in the way of coma and in the way of apnœa, is connected often with morbid conditions of the head and chest respectively, so death, in fever, occurring purely or *chiefly* by asthenia, connects itself with morbid conditions existing within the belly.

And in typhoid fevers we discover within the abdomen vestiges of mischief so constant and definite, and so different from what we meet with in typhus, as to have led first to suspicions, then to a closer scrutiny of facts, and finally to what I deem full proof that these two disorders are as distinct the one from the other, as scarlet fever is distinct from small-pox. To these morbid conditions, then, so striking, so constantly to be noticed within the intestines of those who die of typhoid fever, and so uniformly absent after death from other causes, I have now to invite your attention.

I scarcely need remind you that the intestinal canal is largely furnished, on its inner surface, with glands, or follicles, which consist of little more than crypts, and of which the precise office has not yet, I think, been accurately determined. Some of these glands are sprinkled (one might almost say at random) over the whole tract of mucous surface. These are accordingly called *solitary glands.* Till I was better instructed by Professor Todd, I used to speak of them as being the glands of *Brunner*;

whereas the glands discovered and described by that anatomist are limited to the duodenum. Other glands or follicles are collected into groups, and are named *glandulæ agminatæ*, or often the glands of *Peyer*, who has given a capital description of them. Now the arrangement of these glands of Peyer is peculiar; and in reference to the morbid anatomy of typhoid fever, very necessary to be known. They are met with in the ileum alone: the groups are, mostly, oblong in form; and they occupy that part of the bowel which is opposite to its mesenteric attachment. They are largest, and most numerous, and consequently most thickly set, in the lower end of the ileum, which, in some instances, is almost entirely covered with them: they are found also upon the ileo-cæcal valve; but beyond that they do not go in that direction. Ascending from the cæcum towards the jejunum, these groups or patches become smaller, shorter, more circular, and less numerous; they are separated by longer and longer intervals, till at last they cease to be visible at all. Where there are valvulæ conniventes, there the situation of these patches is very obvious; for the valvulæ conniventes are interrupted, and never run across them. This fact has sometimes led to curious mistakes. I have seen in the Museum of the College of Surgeons a preparation put up by John Hunter (but not labelled I presume by him), professing to be an example of the destruction of portions of the valvulæ conniventes by ulceration. It was nothing more than one of those natural patches, rather more plainly developed than usual.

These glands, in their healthy state, are much more conspicuous in some bodies than in others. You may see, if you look at them attentively, that each patch is made up of a congeries of mucous follicles, of which the *orifices* are obscurely apparent. Now the main alterations met with in the abdomen after death, in typhoid fever, are alterations of these very glands: of the solitary glands, to wit; and still more constantly and remarkably of the agminate glands.

The changes which these glands undergo are of the following kind. In the first place they become enlarged, and more *perceptible* than they are in their natural state. They then present a greyish transparent surface, dotted over with black points; which black points mark, I conceive, the excretory mouths of the several follicles. This appearance, however, which has been likened to the beard of a recently shaven chin, is not necessarily associated with disease. Then, as the inflammation, for such it is, advances, the patch becomes reddish, perhaps; and the follicles burst, or ulcerate, or slough away; not altogether, but partially and by piecemeal: so that an irregular ragged ulcer is generally left, having thickened edges. Sometimes, however, the follicles disappear without there being much redness or thickening: the mucous membrane immediately adjacent, and even the remaining part of the patch of follicles, being pale, and level. Sometimes the patch puffs up into a sort of fungous swelling, in which all trace of the follicular structure is lost. The colour of the ulcerated surface is various, as well as its form and appearance. Sometimes it is pale and grey; sometimes red; oftentimes yellow, as if the exposed cellular and other tissues were stained by the ochrey fluid which had been poured from the bowels during life. What I have hitherto stated relates to Peyer's glands; but the solitary glands participate, usually, in the change. They become, in the first place, large and hard, and present a whitish coloured projection from the surface, which, by a mistaken analogy, has been sometimes called a pustule. At length a loss of substance takes place in these also; beginning at the summit of each, and producing a small, but sometimes a deep ulcer.

And of these changes it is further to be observed, that they are more common, more numerous, more extensive, more advanced, in proportion as we approach the cæcum. It is natural that we should meet with *more* ulcers near the cæcal valve, because there are more glands there; but undoubtedly the ulcers are (in general) *further advanced* there than higher up in the bowel.

Going along with this ulceration of the mucous glands of the intestines, and above all of the aggregate glands of the ileum, you will find inflammation (i.e. redness, hardness, and swelling) of the corresponding *mesenteric* glands. This is a consequence of the inflammation and ulceration of the mucous glands. The affection of the mesenteric glands is secondary; and bears the same relation to the ulcers seen in the bowel, as a bubo in the groin bears to a chancre on the glans penis.

These alterations are extremely interesting, because they afford a reasonable explanation of many of the symptoms of typhoid fever. They account for the diarrhœa. They account for the commonest form of hæmorrhage from the bowels: in one instance Sir William Jenner found that water, thrown into the superior mesenteric artery, welled forth freely from the edges of a ragged ulcer not far from the ileo-cæcal valve. They account also for the uneasiness or pain which is experienced when the abdomen is pressed, and teach us why that uneasiness is greatest in the situation of the cæcum. Nay, we can even understand why, although these ulcerations exist, there may be *no* pain occasioned by them. This may be partly owing to the general insensibility to impressions and sensations of all kinds produced by the stupor; but partly, also, it probably depends upon the depth to which the ulceration goes. The mucous tissues are possessed of but little sensibility even under inflammation; but if the muscular and peritoneal coats become involved in the inflammatory process, then pain begins to be felt. I remember, a few years ago, attending a young lady ill of typhoid fever, with a very well-informed practitioner, but one of the old school, for he had not turned his attention much to the state of the intestines in that disease. One day we learned that our patient had had hæmorrhage; from the uterus her friends supposed; but when I saw the discharge, I was made certain, by its appearance, and by its odour, though it was not mixed with any fæces, that it had come from the bowels; and I stated my conviction that there was ulceration in the lower portion of the ileum. But she had no pain in the abdomen. You might press any part of it without exciting the smallest uneasiness. So, distrustful of my opinion, they called in an eminent accoucheur, who also pressed and examined the belly: but neither could he detect any tenderness or undue sensibility. He next examined the uterus *per vaginam*; but could discover nothing wrong there. Some few nights afterwards the general practitioner was called out of his bed to this patient. He was told that the hæmorrhage (or flooding as they called it, for they persisted in believing that it was uterine,) had returned; and before he arrived at the house the patient was dead. We next day opened the body together. The uterus was perfectly natural; there was no vestige of discharge or bleeding in the vagina; but the ileum, for about a foot above its entrance into the cæcum, was in a state of superficial but ragged ulceration, and universally red and besmeared with blood. I mention this as a strong fact in illustration of the possibility of there being much disorganisation of the inner surface of the intestines, without any pain to reveal it.

Now the alterations I have last been sketching—the thickening, redness, tumefaction, and ulceration or sloughing of the glands of Peyer, and also of the solitary glands, are met with, almost constantly, in some epidemics of continued fever, and are absent, almost constantly, in others. The kind of fever with which I was conversant in London for ten years before the first arrival of the cholera in this country, I now know to have been *typhoid* fever. The antiphlogistic regimen was indispensable at the outset of the disorder: in many instances topical bloodletting seemed to be required, appeared to do good, and certainly was well borne: in clean, well-ventilated wards the disease showed no strong tendency to spread: the mortality was very moderate. About the rose-coloured spots upon the skin I can say nothing, for I had not learned to look for them: but the glands of Peyer, according to my own experience of the fatal cases, were invariably affected. Subsequent epidemics, I may specify particularly that of 1838, offered a marked contrast in all these points. A large per centage of those who contracted the fever died: the disorder was propagated from hospital patients to nurses and students: after death, we could not detect any disease of the agminate or other glands of the intestine: the peculiar mottled rash scarcely ever failed to show itself: we were taught by experience to refrain as much as possible from abstracting blood: and almost from the beginning, or quite, we found it necessary to sustain our patients by a liberal allowance of strong animal broths, and even of wine, or brandy. An epidemic of which I witnessed a portion in Edinburgh in 1820, was without intestinal ulcers. Chomel, during five years' investigation of this matter in the *Hotel Dieu*, never met with an exception to the general rule of their occurrence, or of some degree or form of that sort of alteration of the mucous glands, of which the ulcer is the final stage: and the experience of Louis is to the same effect.

Dr. Carpenter has shown it to be probable that the natural office of the glandulæ agminatæ is to eliminate decomposing and noxious matters from the blood, and to discharge them into the intestinal canal: and Dr. C. B. Williams advances the reasonable suggestion, that the ulceration so constantly met with of those glands in typhoid fever may result from the continued operation of the poison of that disease, thus escaping.

This intestinal condition, then, while it throws an important light upon some of the prominent symptoms of the disease, furnishes also evidence the most cogent against the identity of the several forms of fever. In regard to the typhoid disorder, the argument is thus forcibly put by Dr. William Budd: 'These are changes which have the twofold distinction of being *always* present in this disease, and *never* present in any other; changes therefore specific in the highest degree. . . . Take the diseased intestine away, and it becomes impossible to distinguish the body of a man dead of this disease from that of a man killed by any other septic poison. Take away the body, but leave the intestine, and by the marks upon it, death from this fever is at once distinguished from death from every other cause. This affection of the intestine is as much a specific character of this fever as a peculiar eruption on the skin is a specific character of small-pox. It has always appeared to me, that if the affection of the intestine which is characteristic of one of these fevers had been a thing open to view instead of being hidden from it, the question of their identity would never have arisen.'

When these internal ulcers do occur, is the disease, as is popularly supposed, always or necessarily fatal? By no means. We know that whereas these glands are affected in every patient having this species of fever, a large proportion of such patients get well. It *may* be fatal, in more ways than one. It may lead to death by exhausting diarrhœa, in the way of slow asthenia. The ulceration may kill by laying open a large mesenteric blood-vessel, and so producing copious hæmorrhage and mortal syncope; it may, and often does, destroy the patient, by *perforation* of the bowel: the ulcer penetrates the mucous and muscular coats, and reaches the peritoneum; and sometimes that membrane gives way, and sometimes it does not. In Louis' experience, one-fourth of the fatal cases of typhoid fever were attended with perforation of the small intestine. Upon the consequences of the rupture or perforation—the escape, I mean, of the contents of the bowel, and the supervention of intense and incontrollable peritonitis—I need not again dwell. But the ulcers may, and doubtless often do, *heal*; and the *scars* which they leave behind them are frequently to be seen. The ulcerated surface seems to clothe itself afresh, by degrees, with a new mucous membrane, which is thin, however, and adherent to the subjacent tissues, and does not slide over them when pressed between the finger and thumb, as the healthy portions of the coats of the bowel will slide upon each other. And in the seat of the cicatrix there is usually to be seen a slight amount of puckering, and a number of little wrinkles or lines, radiating from a common centre. According to Rokitanski, with whose opinion Sir William Jenner's concurs, the formation of these cicatrices never leads to any diminution of the calibre of the bowel.

But it is plain, and most worthy of consideration, that the existence of these ulcers is likely to prolong the illness of the patient after the fever itself has ended; to protract his convalescence; to hinder his recovery; and even to endanger his life, though he may seem to be getting well, by causing hæmorrhage, or perforation of the bowel.

In the earlier period of typhoid fever the temperature takes a tolerably uniform upward course; alternately increasing from morning till evening and decreasing from evening till morning, with a daily rise on the whole, up to the evening of the fourth day, when it reaches 104° or 104½°. Except that the temperature does not start so high all at once, this course thus far differs little from that of typhus fever. Any marked deviation from this course may be taken as a proof that the case in question is not a case of typhoid fever. The temperature of this earlier period has no prophetic bearing upon the mildness or the severity of the disease in its subsequent course.

During the second half of the first week and the first half of the second week, the range of temperature is nearly uniform: its maximum, 104½°, 105°, or more, being reached on the fourth or fifth day, but sometimes later. At that maximum it generally remains for one day only, sometimes for two days, rarely for three. In the vast majority of instances the disease, even when its progress is mild and favourable, does not manifest decided defervescence before the twenty-first day.

The temperatures of the third week differ in a striking manner from those of typhus fever at the same period. In typhus defervescence occurs, and the normal temperature is regained about the fourteenth day. In typhoid, on the other hand, the fever is prolonged into the third week; so that while the daily difference between the morning and the evening temperatures is sharply marked, amounting to from 3° to 5° or 6°, the latter gradually decrease till, in the course of the week, the temperature, descending in a zigzag fashion, reaches its natural standard at the end of the week, and convalescence commences.

When the higher temperatures are protracted longer than this, the case is a severe one. It becomes dangerous, or at best has a tedious recovery, if the temperature attain 106°. Complete recovery can never be announced till the evening temperature shows perfect freedom from fever.

It may be well for you to bear in mind, for the satisfaction of early anxiety as to the diagnosis, that in all probability the disease is *not* typhoid fever—

1, if the temperature on the first three evenings, or on two of them only, is the same;
2, if on two of the first three mornings the temperature is alike; or,
3, if the temperature on the first two days rises as high as 104°.

Among numerous minuter points of difference between typhus and typhoid fever, observed and recorded by Sir William Jenner, there are yet two or three which demand mention in the sketch which I am now giving you. In typhoid fever that facile separation of the pia mater and arachnoid from the convolutions of the brain is seldom noticed, which I told you was common in typhus. Again, ulceration of the pharynx was discovered in one-third of Sir William Jenner's fatal cases after typhoid—in no single instance after typhus fever. Slight epistaxis occurs sometimes exclusively, I believe, in typhoid fever.

The spleen, after death, in both these forms of fever, is very frequently found altered in size and texture: enlarged, of a dark colour, and of soft and sometimes almost rotten consistence. The enlargement, which is more constant in the typhoid form, may often, by means of palpation and percussion, be detected in the living patient.

There remains yet another species of fever sufficiently remarkable and frequent to require a brief description.

It would be difficult to give you a more graphic outline of this disorder than has been drawn from nature by Dr. Christison, who, upwards of half a century ago, and on subsequent occasions, watched its phenomena and behaviour in Edinburgh. 'It was characterised (he says) by its very abrupt invasion, often rendering the patient prostrate within an hour; by the continuous and urgent suffering from febrile anxiety, restlessness, burning heat, rending headache, and irritability of the organs of sense throughout the whole period of perfected fever; by its abrupt departure, often in two hours, with free sweating, between the fourth and seventh days, most generally on the fifth; by a complete apyretic intermission succeeding, so that in a few days the patient may be out of bed, or even walk out of doors, or sometimes actually travel to some distance without difficulty; by an abrupt relapse, ushered in with severe rigors, taking place on the fourteenth day from the beginning of the primary attack, and not to be averted by any precautionary management; and finally by a second abrupt sweating crisis on the third day of relapse, leaving the patient greatly prostrate, and with a slow convalescence to pass through, but without any vestige of fever after the expiry of the few brief hours of critical sweating.'

The second convalescence is generally permanent. The relapse or repetition of the symptoms may, however, happen three, or even four times.

In another place, Dr. Christison says, 'It is far from being a deadly fever; but it causes great suffering, and debility so lasting that it makes a working man unfit for labour for two months, first and last. Strange to say, in this city (Edinburgh) at least, where it has been better studied than anywhere else, it is never seen but as an epidemic. I have known four such visitations of it, in 1817–20, 1827–28, 1841–42, and 1847–48: but I never saw it in the intervals, nor have any of my medical brethren. Hence, at every first appearance, it is at first taken for a new fever. It is met with in the labouring population alone; never in the easy ranks of society, unless through very decided exposure to infection.'

The history of the epidemic of 1841–42 has been given by Dr. Alison, by Dr. Henderson, by Dr. Cormack, and in great detail by Dr. Wardell. It was thought by these physicians, and by others who witnessed it, to have been a new and distinct pestilence. It was, in fact, what is now well known by the name of *relapsing fever*. Later research has established this to be, indeed, a *distinct*, but by no means a *new* form of fever. In the following paragraph Sir William Jenner traces for more than a century the existence of a disease having the peculiar symptoms and course of relapsing fever.

'Writing on the weather of 1741, Rutty says "there was frequently a fever, altogether without the malignity of the disease already described, of six or seven days' duration, terminating in a critical sweat (as did the other also frequently); but in this fever the patients were subject to a relapse, even to a third or fourth time, and yet recovered." In 1800 and 1801 there was an epidemic in Ireland of a fever generally terminating on the fifth or seventh day by perspiration, and when that happened, very liable to recur. Barker and Cheyne's Reports, and Dr. Welch's book on Blood-letting, prove the existence of a similar fever in 1816, 1817, 1818, 1819, and 1820, in Ireland and Scotland.

In the year 1847 a great outbreak of relapsing fever took place in Manchester: evidently, says Dr. Tweedie, the consequence of a sudden large importation of Irish immigrants. Dr. Ormerod has given us a sketch of this kind of fever as it fell under his observation in the same year in the wards of St. Bartholomew's Hospital. The subjects of this also were chiefly Irish persons newly arrived in London. It was remarkable for the occurrence, in the majority of instances, of *sudamina*, or miliary vesicles: so that Dr. Ormerod speaks of it under the name of miliary fever. In Dr. Muirhead's later experience the sudamina were mostly confined to the second crisis.

I may add to Dr. Christison's rapid sketch, that relapsing fever begins, for the most part, with sudden rigors and a very frequent pulse, rising sometimes to 140 or 150 beats in the minute. The tongue is covered with a thick, moist, whitish fur. Epigastric tenderness, nausea, and vomiting are common among the early symptoms. There is much aching of the limbs and joints. The functions of the bowels are not materially disturbed. In many cases, probably in one-fourth of the whole number, yellowness of the skin occurs, amounting to jaundice, with vomiting of matters looking like coffee grounds, and sometimes as black as ink. Now, Sir William Jenner declares (and my own experience is in agreement with his) that he never saw jaundice in typhus or in typhoid fever. Circumscribed petechial spots—'minute hæmorrhagic points'—never elevated, are observable upon the skin in a large proportion of these patients.

The rate of mortality in this species of fever is low: and death usually takes place, if at all, before the seventh day of the disease. In the fatal cases jaundice has been observed to be a frequent, but not a constant symptom: the surface becomes cold and livid, the pulse very feeble as well as very frequent, a low form of delirium arises, with drowsiness, which deepens into unconsciousness, and so life departs.

It is stated by Dr. Wardell that in pregnant women affected with this fever, abortion is sure to happen, whatever may be the period of gestation.

The only remarkable change discovered in the bodies of those who have died of relapsing fever is very considerable enlargement of the spleen.

Neither early and extreme frequency of the pulse, nor sudden and great variations in its rate of beating, are, of themselves, prognostic of danger. In this respect, therefore, the disease differs remarkably from both typhus and typhoid fever, in either of which a pulse of 130 or 140, and in the former of which rapid fluctuations of the pulse, always indicate great peril. In relapsing fever it may mount to 150, or even higher, and upon the breaking out of perspiration it may drop, in the course of two or three hours, to half that number of beats, without warranting any alarm for the patient's safety.

With these great and sudden leaps of the pulse there are, as you may well suppose, corresponding abrupt changes in the thermometric temperature, which in the height of the febrile paroxysms rises to 106° or more, and sinks in the crises all at once to a degree or two perhaps below the natural standard.

Among the many points of diversity which exist between the three species of fever that we have been considering, one striking and obvious difference is to be found in their respective *duration*. This is apparent even to the observation of the vulgar, who have thus drawn rude distinctions between different epidemics, before they were recognised or acknowledged by the scientific physician. They talk of the one-and-twenty-day fever (typhoid), and of the fourteen-day fever (typhus), according as the disorder 'takes the turn' in three weeks, or in a fortnight. In like manner the relapsing fever was called in Ireland the five-day fever; although as it has its crisis very often on the seventh day, it might properly enough be termed the seven-day fever. We should thus have the natural duration of the three fevers marked by periods of *weeks*.

The average *mortality* in the three species of fever, as deduced from tabular statements compiled on a large scale by Dr. Murchison, is in typhus one in 5 or 6; in typhoid one in 6; in relapsing fever very much less. In one epidemic it has been computed at one in 25, in another at one in 50. We may safely put it at about three in the hundred.

Again, there are some differences between the three in respect of the average *age* of the patients. Typhus is a disease of adult age. Dr. Murchison states the average of 3,456 cases during ten years in the London Fever Hospital to have been 29·33 years; or about four years above the mean age of the total population. It is most common between the ages of 15 and 25.

Typhoid fever is chiefly met with (says the same author) in youth and adolescence. The mean age of 1,772 cases during ten years in the same hospital was 21·25. Sir William Jenner, who omits no opportunity of examining Peyer's glands, tells me that, at the age of 50, or thereabouts, the patches begin to shrink and fade: and he conjectures that this may have to do with the infrequency of typhoid fever in advanced life. About the same time the spleen also grows smaller, and the lymphatic glands waste. Old age has not, however, as I can testify, the privilege of being absolutely exempt from this disease. At a meeting of the Pathological Society, Dr. Wilks showed the ulcerated ilium of a woman dead of typhoid fever, who was more than 70 years old.

Relapsing fever, like typhus, attacks all ages; but the proportion of the young to the aged is greater than in typhus.

Another point of diversity to which Dr. Murchison was, I believe, the first to direct special attention, is this. While relapsing and typhus fevers are both eminently *epidemic* diseases, raging at one time like a general conflagration, and at other times extinct, or merely smouldering, typhoid fever, on the contrary, displays an *endemic* character, is always among us, with a tolerably uniform prevalence year after year. Uniformly, too, it is most rife during the autumnal months.

Typhus fever appears to be much more common in the great towns of England and of Scotland than it is in Paris, where typhoid fever is the predominant, if it be not the only, form. Relapsing fever has prevailed to a very great extent in Ireland, whence it was probably first imported into Great Britain, and into America. I look upon it as emphatically an Irish fever.

LECTURE LXXIX.

Exciting cause of Continued Fevers. Their treatment.

It is of vast importance to think rightly of the *exciting cause* of continued fevers. Upon this subject there has been, and there still is, a confusing and pernicious contrariety of opinion among medical men.

The two great questions about which we have to make up our minds are these :—

1. Are the three diseases, typhus fever, typhoid fever, and relapsing fever *contagious* diseases; communicable, I mean, from one who has the disease to another who has it not?

2. If contagious, do they ever arise except from contagion?

My own conviction, respecting everyone of the three, is, that the first of these questions must be answered in the affirmative; the second, in the negative.

The facts and reasonings which in former courses of these lectures were brought forward in proof of the contagious character of continued fever generally, have lost something of their determinate value since the distinction of continued fevers into definite species has been firmly established. The disbelief in contagion has also much diminished. I shall, therefore, both rearrange and abbreviate my former observations on this subject.

When the same disease attacks many persons in the same house or neighbourhood, at about the same time, the popular suspicion soon arises that the disease is catching. Yet you know that disorders may be widely prevalent without being contagious. Agues, for example, engendered by malaria; ordinary catarrhs and sore throats, produced by vicissitudes of the weather. When an epidemic malady affects large masses of the people suddenly and at once, it is presumably not contagious; at any rate it must have some other source besides contagion. When, on the contrary, it begins in a certain spot, and gradually spreads thence as from a centre, the presumption is in favour of its propagation from person to person. In investigating this subject, if we trace the fever among persons who have had intercourse with the sick, and more frequently in proportion as that intercourse has been close and continued;

and if we find that other persons, living in the same place, and under precisely the same circumstances, except that they have had no known communication with the sick, do escape the malady; we have in these facts convincing evidence that the disease has been spread by such intercourse; in one word, that it is contagious.

Again, when a person having a given malady upon him is transferred to some distant spot previously free from that malady, and the malady begins thereupon and thenceforward to spread among other persons, as from a centre, we conclude that the malady is contagious.

Evidence of this latter kind, especially when it is multiplied, is peculiarly cogent and conclusive: for we have to take into account, not simply the extension of the disorder to a few, but the previous freedom from it of the many. It is in questions of this kind that statistics speak with the greatest force.

That *typhus* fever is a rankly contagious disorder is doubted now, perhaps, by none. We find, even in hospitals, where cleanliness and ventilation are prized and enforced, that this fever attacks most of the persons who come oftenest and most closely in contact with those already sick with it: chiefly the nurses, next the clinical assistants and the most assiduous of the students, and the medical officers; and too frequently other patients in the same ward. 'Every physician (writes Dr. Tweedie, whose testimony relates to typhus rather than to any other species of fever) connected with the London Fever House, with one exception, has been attacked with fever; and three out of eight have died of it. Also the resident medical officers, matrons, porters, laundresses, domestic servants not connected with the wards, and every female who has performed the duties of nurse, have one and all, invariably been the subjects of fever. And to show that the disease is capable of being conveyed by fomites or clothes, the laundresses, whose duty it is to wash the patients' clothes, are so invariably attacked with fever, that few women will undertake the loathsome and disgusting office.'

Dr. Murchison quotes from a statement of Dr. Stewart's, that during the winter of 1837–38, an isolated case of typhus in one of the medical wards of the Glasgow Infirmary, communicated the disease to most of those in the same ward, and several died.

In the year 1866, a widow woman was living with one of her daughters, in the small village of Scorton, in Lancashire. This was considered a model village; it contained no public-house or beershop, and its inhabitants were quite healthy and free from fever. Another of the widow's daughters fell ill of typhus fever at Wigan. The daughter from Scorton went to Wigan to attend upon her sister, and was herself attacked. Shortly afterwards, the mother went to their assistance, and she, too, caught the malady. The daughter who had left her home died; the other returned with her mother to Scorton, both of them being then in a convalescent state. On arriving at Scorton, they were helped out of the conveyance which took them from the railway station to their house, by a man who soon afterwards was attacked with the fever and died. The disease then appeared in the house of a man named Wells, for whom the widow used to wash. Three of Wells' family were taken ill, and one of his daughters died of the disease. Gradually the epidemic spread from house to house, till of 36 families living in the village, 23 were more or less affected with the fever. Eighty cases occurred in and around the village, and ten of them were fatal.

The cook of Trinity College, Cambridge, living in a street called the Petty Cury, had a daughter in London who fell ill with typhus fever, and who insisted upon going home. At that time there probably was no case of fever in Cambridge: certainly none in the Petty Cury, as Dr. Haviland (who gave me this account) satisfied himself by inquiry. The girl was very ill indeed after she reached her father's house; but she ultimately recovered. Every inhabitant of that house, except an old, seasoned nurse, became affected with the fever; and three or four of them died. But no fever existed in the other houses of the same street. When one of the sick persons was convalescent, it was thought that her recovery might be accelerated if she were put into a lodging at Trumpington, a small village two or three miles from Cambridge, in which there certainly was then no fever. Here she and the old nurse were waited on by a servant belonging to the Trumpington House. That servant soon sickened of the fever, and was sent to Addenbrook's Hospital, where she died.

Take one more instance to the same effect, related by Dr. Alison. 'Some years ago, at a time when there was no great number of fever cases in Edinburgh, I met with a case in the son of a shoemaker, who was lying in a room in which his father and two apprentices were at work. I could not prevail upon the father to remove his son to the hospital, although I stated the danger of the apprentices being affected. Within two or three weeks after, I found that the two apprentices were lying ill of fever in their own houses: one of them two hundred yards, the other half a mile distant from the workshop, and widely distant from each other. These young men likewise lay at home during the fever; and each of their cases was speedily followed by a succession of others in the inhabitants of the rooms which they occupied, and of those immediately adjoining, who had never been at the workshop. In one of these houses seven, and in the other twelve, were thus affected. Now on the supposition of the fever being contagious, all this was to be expected, and all corresponded to the predictions which were hazarded on that belief. But on the supposition of such succession of fever cases depending on local miasmata, (which had been suggested) there must have been at least two, more probably three, separate and accidentally concurring miasmata to explain the phenomena here observed; one at the workshop, and one at each of the houses of the apprentices; and there must have been this extraordinary coincidence, that at each of these last the malaria sprung up just at a time when a patient was lying ill there of fever, which he had apparently contracted elsewhere. Further, the three houses in which these successions of fever cases were observed, are in situations very different from one another; and all of them have been, to my knowledge, perfectly free from fever for years together, both before and since that time, notwithstanding that fever has been much more generally prevalent, and that they have been inhabited by successive families. What probability is there (continues Dr. Alison) that three separate miasmata should have arisen in these three houses, just at the time when their presence was required in each to produce an effect which had been foretold as the consequence of another cause undeniably operating on all?'

These are samples of the vast stock of evidence that we possess of the contagiousness of *typhus* fever.

Dr. Welch, in his account of *relapsing* fever as it was seen in Queensbury House, in Edinburgh, which was opened for the sole use of fever patients during the prevalence in that city of the epidemic of 1817–1819, has the following statement:—

'In this hospital, since it was opened (which was the year before the time when Dr. Welch was writing), my friends Messrs. Stephenson and Christison, the matron, two apothecaries in succession, the shopboy, washerwoman, and thirty-eight nurses have been infected, and four of the nurses have died. With the exception of but two or three nurses, who have been but a short time in the hospital, I am now the only person who has *not* caught the disease, either here or at the infirmary, within the last eight or ten months.'

That the same disorder has been brought over from Ireland, and diffused itself rapidly and widely in Manchester and elsewhere, the physicians who have seen and treated the disease in the infected places have the profoundest belief.

If we contrast facts such as I have been advancing, with other cases, in which all the circumstances appear to have been precisely the same, *except* the presence of the alleged cause of the disease—making our observations always upon as large a scale as possible—we approach, as nearly as the subject will admit of, to a demonstrative proof that typhus and relapsing fevers spread by contagion.

The reports which were made by the accredited physicians, to a Committee of the House of Commons, respecting epidemic fever in Ireland, contain abundant and valuable evidence on this point also. The fever was either typhus or relapsing. Dr. Cheyne states that the farmers and householders in some parts of Wicklow, who would not harbour or admit into their houses strolling persons, nor go to wakes or funerals, remained free from the disease. In Ballytore a committee was formed of persons who took pains to instruct the inhabitants as to the precautions to be observed against infection: such as refusing admission to wandering beggars, absenting themselves from wakes and other assemblies; and even, under certain circumstances, from places of worship. These precautions were so effectual that not a case of fever occurred. Four villages in the neighbourhood of Lismore are stated by Dr. Barker to have been preserved from the fever, chiefly by the exertions of some Roman Catholic clergymen, who persuaded the inhabitants to avoid all communication with Lismore, and with another town in its vicinity, where the fever was rife.

It was observed also that bodies of persons collected together, and fenced about by barriers which precluded intercourse between them and places infected with the fever, remained exempt from it; children in charity schools, soldiers in barracks, and even prisoners in the jails. In the same parliamentary report you may see a letter from Dr. McDonald, who had established a fever hospital at Belfast, which hospital alone, at one period, contained 190 cases of the disease. Very near the hospital was a school, containing 700 or 800 young persons; a poorhouse with 300 inmates; and a barrack with 1,000 soldiers. These places were never more free from fever than at that time.

Facts to the same purpose abound in the medical reports of the army and navy. Those of the navy are especially valuable and instructive in this matter, because the whole of the circumstances in which the patients are placed come under the certain and immediate cognisance of the medical officers superintending them. I cannot go into particulars here, but must content myself by referring you to the writings of Dr. Lind, Sir Gilbert Blane, and Dr. Trotter. You will there find that ships which had, for a great length of time, been quite free from fever, have had that disease spread rapidly from one individual, recently imported, so as to affect almost all the crew. This was often the case when raw recruits were drafted from the receiving ships. It appears, from reports made to Dr. Trotter by different naval surgeons, and published in his *Medicina Nautica*, that the fever was conveyed to a great number of vessels forming the Channel Fleet, from the receiving ship called the *Cambridge*.

It is, moreover, found, that when persons ill of fever are taken away from their own close and crowded houses, and when means of purification are employed, the fever ceases to spread in those houses. This well-ascertained fact it is which gives to fever hospitals their greatest, nay almost their only value. They would otherwise, as we have already seen, be detrimental or dangerous to all concerned with them, by concentrating the poison that produces the fever; without equivalent benefit. As it is, they cause, indeed, a certain amount of disease and of death; but by affording opportunities for clearing an infected neighbourhood of the seeds of the fever, and by so preventing its diffusion among a large and healthy community, they save many more lives than they sacrifice.

The evidence of the contagious property of *typhoid* fever requires, and will repay, a somewhat closer scrutiny. It is a subject of the deepest interest. As we see this fever, we who live *in a city*, its contagious property is much less manifest than that of the other two fevers. It is very rarely found to pass from one person to another in the wards of a well-conducted hospital, or in a private dwelling-house. Dr. Murchison tells us, that during his own experience, in the London Fever Hospital, of 1,048 cases of this typhoid fever, there were but two which appeared to have originated in the hospital;

whereas, the number of typhus patients being 2,581, thirty-six of the attendants and patients contracted the disease. Many of the continental physicians, and not a few in this country, doubt whether this fever be contagious at all. Dr. Murchison himself says, that 'although it is communicable, it is not so certain that it is contagious in the strict sense of the term.'

On the other hand, *in rural districts*, in the hamlet and the farm, both here and abroad, the disease in numberless instances has been seen to spread from person to person in the same house or neighbourhood, with a readiness scarcely less remarkable than that of typhus: so that the strange conclusion has been adopted that 'typhoid fever is almost invariably contagious in country places, and only rarely so, and by exception, so to speak, in cities and large towns.'

This subject has been handled with singular clearness and force by Dr. William Budd, in a series of communications to the 'Lancet,' in the years 1856, 1858, and 1859.

He enjoyed unusual and peculiar advantages for studying the country habits of this cruel disease. Born and brought up in the village of North Tawton, in Devonshire, he was personally acquainted with every one of its inhabitants; and being in almost exclusive possession of the practice there, nearly every one who fell ill, not only in the village itself, but over a large area around it, came immediately under his care. 'For tracing (he says) the part of personal intercourse in propagating disease, better outlook has rarely fallen to the lot of an observer.'

He gives some striking examples of the spread of the fever through the household, when once introduced.

In one family a young woman was the first subject of it. Her mother, brother, and sister were soon laid up with the same fever. The father, who had already in former years had the disease, and a young infant, were the only inmates spared. In another house, four out of six persons were successively attacked; in another, three: and so on. There were few houses in which, having once appeared, it did not further extend itself to one or more members of the family.

All this might, possibly, have been the effect of some local miasm.

But the other test of contagion was equally conspicuous and even more conclusive.

The evidence was of this kind.

In North Tawton, A and B lodged next door to a house in which typhoid fever existed. The two houses had a common privy. Both of these men took the disorder, and becoming too ill to work, returned to their respective homes in the parish of Morchard, seven miles off. A took at once to his bed, and at the end of five weeks was dead. Ten days afterwards his two children were laid up with the same fever, and had it severely. B, a single man, lodged with two aged persons. After a long struggle he recovered. When at the worst, a friend, C, who had called, was asked to assist in raising him in bed, and was overpowered by the smell from the sick man's body. Ten days afterwards, in another part of the village, C also was 'down' with the fever. Before he was convalescent two of his children sickened with the disease, and also a brother who lived at a distance, but had repeatedly visited him. The two aged persons escaped infection.

The houses occupied by these four men lay some way apart. Except under their roofs there was at that time no fever in that part of the country.

Again, D, drooping with the disease, went from North Tawton to a house in Chaffcombe, seven miles distant. Nine persons in that house took the fever.

One of these nine moved in her early illness to Loosebeare, a hamlet four miles off, and became the focus of a little epidemic which gradually extended to the whole hamlet. Scattered over the country side were some twenty or thirty other hamlets, the precise counterparts of this. 'Two or three farm-yards, and a few labourers' cottages clustered round them, made up (says Dr. Budd) in each case the little community. In each of these there were the usual manure-yard and the inevitable pigsty; in each there was the same primitive accommodation for human needs. The same sun shone upon all alike through month after month of the same fine dry autumnal weather. From the soil of all, human and other exuviæ exhaled into the air the same compounds, in about equal abundance. And yet while at Loosebeare a large proportion of the inhabitants were lying prostrate with intestinal fever, in not one of the twenty or thirty exactly similar places was there a single case.'

The tests are complete. You may take it as absolutely certain that the three fevers are all of them contagious.

In all cases the effluvia which proceed from the lungs and from the skin doubtless contain and carry the peculiar poison. But in typhoid fever the contagious power belongs chiefly and especially to the discharges from the bowels, to that thinnish, yellow fluid, unmixed with mucus, and having an alkaline reaction, which I have already described as peculiar to that disease. This peculiar flux comes—as Dr. William Budd believes and argues, and in the same belief I unhesitatingly share—from that specific *internal eruption*, from those ulcerating follicles of the intestinal glands, which characterise the disorder; just as surely as the pustules on the skin which characterise small-pox are pregnant with the specific contagion of that disease.

And this being so, the apparent discrepancy between the town and the country experience respecting the contagious quality of typhoid fever is accounted for at once.

In the country—under the present neglect of sanitary care—the poison of this disease, and of some others, *infects the ground*. The alvine discharges 'accumulate day by day on the open soil which surrounds the patient's dwelling, until they envelope the whole household, and often the neighbour's also, in a fever-miasm, which is incomparably more virulent than the atmosphere of the sick chamber itself.' 'Among the poor (I am still quoting from Dr. Budd), when this fever breaks out in a family, the discharges from the bowels are thrown either into the common privy, or, as I have seen a hundred times in rural districts, are cast upon the dung-heap, or into the open gutter. From this point, following the line of watershed, this pestilent stuff often makes its way to considerable distances, where, appearing now under the guise of an endemic miasm, it may carry disease and death into many an unsuspecting household.'

On the other hand, in country mansions, in cities, and in large towns, the alvine excretions—the principal vehicles of the poison—are for the most part discharged through water-closets into the drains and common sewers, and so cease to be dangerous *to the houses in which they are thrown off from the body of the sick person*; and the disease *seems* to be but slightly contagious.

But, according to Dr. William Budd's teaching, the danger thus carried *from* the houses of the sick is transferred *to* other houses more or less distant. During the prevalence of typhoid fever in a large town, a vast quantity of the dejections peculiar to that disease must be daily and hourly poured into and through the drains and sewers, impregnating the sewer gases with the specific poison. In our present faulty arrangements, these gases, so infected, enter many a house the inmates of which are unsuspicious of such a source of fearful peril. Mr. Rawlinson, the well-known engineer, has stated in print that, in the year 1859, disinfectants were freely used in some of the London main sewers, and 'the smell of the disinfectants was found to pervade all the houses in the district connected by drains with the sewers; showing to demonstration that such houses must at all other times be pervaded with diluted sewer gases.' 'Wherever (writes Dr. Budd) the alvine discharges from typhoid fever patients travel—wherever exhalations from them penetrate—there the most specific of all the exuviæ of the sick body are in operation. *The sewer which is their common receptacle is the direct continuation of the diseased intestine.*'

To show that typhoid fever may be communicated by drinking water containing the specific poison, I may quote one conclusive instance from Dr. Budd.

Richmond Terrace, Clifton, is built in the form of a horseshoe, and contains 34 good houses. Of these 13 were supplied with drinking water from a pump at one end of the terrace. In the latter end of September the taste and smell of this water suggested that it was tainted with sewage; and subsequently an examination of the well revealed the actual leakage. Early in October intestinal fever broke out, nearly all at once, in all the 13 houses in which the tainted water had been drunk. In almost every house of the 13, two or three persons were laid up, and in some a much larger number.

These houses were many of them far apart from each other in the length of the terrace; and their inmates, for the most part, were not in the habit of personal intercourse. Meanwhile the other families on the terrace, living side by side with these, but not drinking water from the tainted well, remained perfectly free from fever.

Moreover, each of the three species of fever perpetuates itself, and no other. Typhus undeviatingly reproduces typhus, and never typhoid or relapsing fever; any more than small-pox propagates scarlet fever or measles; and so of the rest.

Upon this point we are indebted to Sir William Jenner for the most satisfactory evidence that has yet been collected. He noticed, as others have done, that whichever of the three species was, in its turn, prevalent in epidemic abundance, cases of the other two were intermixed; and that these retained without change or modification their characteristic features. There were no transition forms; nothing like graduation of one species into another. He also took pains, during the years 1847, 1848, and 1849, to track the in-patients of the Fever Hospital to their several homes; and he found that where more than one fever patient came from the same family or the same house, they all presented the same species of fever. He thus traced 208 patients from 75 different sources of infection, with one exception only to the rule just stated: and this sole exception admitted of an explanation so reasonable, that it could scarcely be said to militate against the general inference.

You may read his evidence in full, in the 33rd volume of the 'Medico-Chirurgical Transactions.'

This great truth attests conclusively the specific difference of the three diseases. It had suggested itself to other minds, but had never been so successfully worked out. Dr. Alison, in his description of the epidemic relapsing fever of 1833–34, says, 'Patients with two kinds of fever, not to be clearly distinguished from one another in the early stage, are daily admitted; and each of these forms of fever appears to be easily *communicated* in the hospital itself, to *convalescents from the other*.' Dr. Henderson, writing in the 61st volume of the 'Edinburgh Medical and Surgical Journal' upon the same epidemic, states that in nine instances the same persons had exhibited the epidemic (or relapsing) form, *and* typhus, within a very short time. He concludes 'that the two forms of fever do not acknowledge a common origin; that they are not propagated indifferently from the same sources of infection; that the one will not produce the other.'

In corroboration of the same conclusion, Dr. Bartlett adduces the remarkable, and otherwise inexplicable fact, 'that for the long period of the last thirty or forty years, in the city of Paris, and throughout New England, where this subject has been most carefully studied, only one of these forms of disease has prevailed, to the entire and absolute exclusion of the others.'

Again, each of the two fevers, enteric and typhus, illustrates the law of which small-pox furnishes so distinct a type, in rendering the subject of it thenceforth, as a rule, intangible by the same contagion. The instances in which the same person suffers either of these fevers twice, are probably not more or less numerous than those in which small-pox itself, measles, or scarlet fever repeat themselves. Sir William Jenner has never known the same individual to be affected twice with typhus fever. Dr. Murchison, however, has twice undergone that disease. A second attack of the typhoid form I believe to be quite as rare. But the rule appears to be broken in the case of *relapsing* fever. Of examples of this, known to myself, I may mention that of Dr. Christison, who suffered three separate attacks of this singular malady within the short space of fifteen months.

And here we come face to face with the second of the two great questions which were put in the beginning of this lecture.

The facts which I have set before you, and the analogy of the typical disorder, small-pox, lead directly to the opinion which I have always held, and which has been argued so convincingly by Dr. William Budd, that it is unphilosophical as well as erroneous to attribute the three contagious fevers in question to any other source than to specific contagion. This is not a new opinion. It was held and forcibly expressed by Dr. Bancroft, who maintained that 'no combination of dead animal or vegetable matter, whether produced fortuitously, or by human art and contrivance, will ever be able to generate any one of these specific contagions, any more than it will be able to produce a horse, an eagle, or an orange tree.'

The very fact that the cooperation of the human body is requisite for the manifested agency, and the multiplication, of the poison would seem conclusive.

Dr. Murchison, however—any of whose deliberate judgments must command our respectful consideration, and who draws after him, I suspect, a large portion of the profession—believes that *typhus* fever may be generated spontaneously, i.e. independently of contagion. 'The poison (he says) is generated by the concentration of the exhalations from living beings, whose bodies and clothing are in a state of great filth.' 'Typhus is due to a specific poison.' 'The poison is also generated *de novo* by overcrowding and bad ventilation.'

'The poison of *enteric* fever is contained in the emanations from certain forms of putrefying organic matter.' 'Pythogenic (typhoid) fever is often generated spontaneously by fæcal fermentation. It is occasionally communicated by the sick to persons in health.'

'Although *relapsing* fever is undoubtedly contagious, it is highly probable that it can be generated *de novo*.' He imagines it to be the result of destitution alone, and he justifies the names of *famine* fever and *hungerpest*, which have been given to it in Ireland and in Germany.

It appears from these quotations that Dr. Murchison, believing all these fevers to be contagious, believes also that each of them may originate independently of contagion.

The main argument for this view is the frequent occurrence of each of these diseases, when no exposure to contagion can be traced or easily imagined. But the same argument would be equally valid in favour of the spontaneous origin of small-pox, which scarcely anyone, I suppose, would now maintain. 'The evidence (Dr. Budd truly observes) is negative only, and consists solely in our inability to trace with the eye the continuity of a chain whose connecting links are known to be invisible. To conclude from this that no chain exists is palpably absurd.' There are a thousand unsuspected ways in which the invisible contagion may be conveyed. It may lurk in a hackney coach; you may catch the complaint from your neighbour in an omnibus, at a theatre or a concert, at church, or in a casual jostling crowd; your linen may be impregnated with it in the house of your laundress; or your coat may bring it from the workshop of your tailor; nay, I have heard it affirmed that the contagion of small-pox has been carried in a letter.

Again, we know that, in instances innumerable, typhus fails to spring up where the effluvia constantly proceeding from the human body are accumulated and condensed by the crowding together of many persons in close, dirty, and ill-ventilated places, where it could scarcely fail to appear if Dr. Murchison's theory were true. Dr. Bancroft instances the natives of the arctic regions, who, in order to shelter themselves against the extreme cold of their climate, live during the greater part of the year in close subterraneous dwellings, from which the fresh air is studiously excluded, and of which the atmosphere becomes so offensively foul as to be scarcely endurable by a stranger, yet continued fever is not known among them. A similar exemption from that disease is observed within the tropics, in the African slave ships, where 'the poor wretches are crowded together below the deck, as close as they can possibly lie, in a sultry climate, barred down with iron, to prevent insurrection.' Although many of them die from suffocation, and from fluxes, yet Dr. Trotter, who was himself, at one time, surgeon to a slave ship, declares that 'contagious fevers are not their diseases.' Dr. Bancroft quotes also a narrative of the sufferings of 193 Europeans, who, during the time of the first French revolution, were 'deported' to Cayenne in the 'Decade' frigate. They were crowded, and even squeezed together, in so small a space, and for so long a time (no less than 96 days), that the sentinels, who were placed at the hatchways to guard them, and who were thus exposed to the hot and fœtid air which came from their hole of confinement, demanded that their period of this offensive duty might be shortened. Yet none of these miserable persons perished, nor did fever, properly so called, arise among them. Yet here were collected together all the alleged causes of typhus fever, contagion excepted—crowding, want of ventilation, filthy clothing, unwholesome and corrupting food, anxiety and dejection of mind.

Typhus fever used to infest our English jails; but that it was always imported, and never engendered there by filth and defective ventilation, and by the accumulation of human effluvia, may be concluded from the fact that the benevolent Howard, when he visited the prisons on the continent, found, to his great surprise, that they were free from fever, although they were no less close, crowded, and impure than our own. He brings the result of his observations and inquiries concerning the cause of the jail-fever, to this pointed conclusion:—'If it were asked,' says he, 'what is the cause of the jail-fever, it would in general be readily replied, the want of fresh air and cleanliness; but as I have found in some prisons abroad, cells and dungeons as offensive and dirty as any I have observed in this country, where, however, this distemper was unknown, I am obliged to look out for some *additional cause* for its production'—which additional cause can be no other than the contagious poison emanating from the bodies of those who have the fever. It is true that fever is most frequently met with, and most rapidly propagated, where men are crowded together, in jails, or in close and ill-ventilated places: but this affords no reason for supposing that it is ever *generated* there, any more (to use a homely illustration of Dr. Bancroft's) than the general prevalence of lice and other parasitic vermin in such places, proves that these vermin are *generated* by filth, by pent-up human effluvia, and by want of ventilation, instead of being merely *fostered* thereby.

Again, continued fever has been attributed, with great confidence, to a vitiated state of the air, from the putrefaction of *dead* animal and vegetable substances. Dr. Bancroft deals with and demolishes this error also; showing that neither the putrid atmosphere of dissecting-rooms (respecting which *you* must have some personal experience), nor the noisome effluvia from full and ill-conducted burial-grounds, nor those to which tallow-chandlers, soap-boilers, glue and cat-gut makers, and the melters of whale-blubber, are exposed, nor the mere foul air of sewers and privies, have ever been known to produce anything like continued fever. In some parts of Essex, near the coast, where the farmers are in the habit of manuring their fields with shoals of sprats, I have seen large tracts covered with these fishes in a state of putrefaction. The stench they occasion is horrible; but no disease results. In the slaughter grounds at Buenos Ayres, Mr. Hinchcliff found the stench very powerful; but it was asserted by the natives to be peculiarly wholesome. Dr. Chisholm, in a paper to which I can only refer, but which I would recommend you to look at, in the sixth volume of the 'Edinburgh Medical and Surgical Journal,' brings forward other and very satisfactory instances, to the same purpose: from a bone manufactory, near Bitton, in Gloucestershire; from an establishment (now relinquished) on the banks of the Avon, for converting the flesh of dead animals into adipocire; from manufactories for refining sugar, where the blood of slaughtered animals, obtained from butchers, is kept for that use; from the leather-dressing business;—all tending, I say, to the conclusion, that air, contaminated by the decomposition of animal substances, is not necessarily even noxious to life; still less productive of that specific disease which we are now considering. The old belief, therefore, was unfounded, that the exhalations from the dead and putrefying bodies of men and horses, lying unburied on the field of battle, are sufficient and likely to produce a pestilential fever. Many instances to the contrary are on record: one, of an early date, is thus stated by Diemerbroek:—Anno 1642, in agro Juliacensi maxima strages facta est, et ad minimum 8000 militum occisi fuerunt, præter majorem adhuc famulorum, rusticorum, aurigarum, puerorum et mulierum numerum, atque equorum copiam innumerabilem: corpora inhumata sub dio computruerunt, nulla tamen pestis insecuta est.'

Dr. Murchison holds that *typhoid* fever is often generated by *fœcal* fermentation. But they who have had much experience of latrines in most parts of the continent will be able to testify that the emanations from human ordure may be both intense and permanent without ever generating enteric fever. The vulgar notion that stenches breed fevers is altogether unfounded. Neither does the absence of stinks certify the absence of contagion. Yet the two may be, and often are, companions.

Mind, I neither deny nor doubt that filth, foul air, and the gaseous products of animal and vegetable decomposition, are things hurtful to health; or that they are capable, especially when abundant and concentrated, of causing serious disease, and even death. What I do doubt and deny is that, of themselves, they ever produce a contagious fever. I agree with Dr. Guy—whose interesting report upon the health of nightmen, scavengers, and dustmen is well worth your perusal in connexion with this subject—I agree with him in believing that 'filth is rather the *nurse* than the *parent* of fever,' but I am not persuaded of the correctness of his final conclusion that 'in extreme cases, fever may be bred of filth.' In old communities the *seminium* of each of these diseases is doubtless always dormant somewhere—as that of small-pox must be—ready to rouse into wide-spread mischief upon the first return of the mysterious influences which awaken or renew its epidemic power.

There never was perhaps a more signal illustration of much that I have been saying than what took place in this metropolis during the hot months of 1858 and 1859. 'For the first time in the history of man (says Dr. W. Budd) the sewage of nearly two millions of people had been brought to seethe and ferment, under a burning sun, in one vast open cloaca lying in their midst.' The Thames stank so horribly that the Courts of Law in Westminster were broken up by the invasion of the abominable odour. Parliamentary committees sat behind window-blinds saturated with chloride of lime and other disinfectants, and even thus could scarcely endure the foulness of the entering atmosphere. The river steamers lost their traffic, and imminent pestilence was everywhere prophecied. Yet no epidemic came, and the death rate for those seasons was below the average.

That *relapsing* fever may be produced by destitution alone, and without contagion, is inferred by Dr. Murchison from the same kind of negative evidence as, I humbly conceive, has misled him in the case of enteric fever. That the prevalence of contagious fevers among the poor is mostly in direct proportion to their state of physical destitution may be granted. The association of pestilence with famine is proverbial. But we do not find that continued fever is ever created by the mere want of nutriment. In persons who have sought to starve themselves to death, among sailors who have of necessity been kept upon very short allowances of food, in cases of isolation by snow-storms, or by the accidental closure of a mine, we find, indeed, that disease is produced by the privation of nourishment; but it is not relapsing fever, nor continued fever of any kind. The condition into which the sufferers are brought is more akin to scurvy. So that, though the want of sufficient aliment may be, and doubtless is, a powerful auxiliary in promoting the effect of the contagious poison, there is no ground for supposing that it ever primarily or solely occasions fever.

For myself, I firmly believe that in illustration of the genesis of all the diseases now under our consideration we may use the playful language of a statesman satirist of the last generation—

> "Like genders like, potatoes tatoes breed,
> Uncostly cabbage springs from cabbage seed,"

and from nothing else.

And to look at the question under a more serious light, of what unspeakable value and importance is this truth, if it be a truth. All the circumstances, contagion excepted, to which the generation of fevers has been ascribed, are predisposing circumstances only; and, although these may in some measure be combated, they are for the most part beyond our absolute control. In an old and fully-peopled country like our own, we cannot hope to prevent or remedy extreme and extensive destitution, nor to separate it effectually from its concomitants of crowding, filth, and despondency of mind. But we may by timely diligence—by well-devised and strictly-enforced sanitary measures—root out the specific contagion, or confine it within narrow bounds; and thus reduce within its smallest possible dimensions (even if we might not wholly extirpate) that enormous and awful amount of misery, disease, disablement, and death, to which these contagious fevers annually give rise.

The *treatment* of continued fevers has been, at all times, a stumbling block to young practitioners; and a subject of dispute even among physicians who have built it upon their own experience. Before I attempt to trace out any plan, or to lay down any principles for your guidance, it may be useful to inquire how it happens that the practice in these and in analogous diseases has been so fluctuating and unsettled.

In the first place, then, it is very difficult to estimate the value and efficacy of any particular plan of treatment, and still more of any particular remedial substances, in these exanthems. Continued fever, of whatever species, like other disorders which run a definite course, and have no direct or necessary operation in spoiling the structure of vital organs, has a strong natural tendency to terminate in health. We see this tendency when the disease is left entirely to itself, and it exists no less when remedies are employed to regulate its course, or in the hope of abbreviating its duration. No one can doubt, who has had much experience in fever, that this tendency is sometimes thwarted by the *nimia cura medici*; and that patients get well in *spite* of the well-meant but mischievous interference of the Doctor. This tendency to recover is a constant source, therefore, of fallacy in our observations upon the behaviour of these diseases under different plans of treatment; and upon the effects and utility of remedies. It leads us, too often, into the danger of ascribing to drugs what is really due to the workings of nature; of confounding antecedents and sequences with causes and effects; of counting recoveries as cures. And this danger is increased by the circumstance that continued fevers, although they observe a certain definite course, are nevertheless liable, even when left altogether to themselves, to sudden and remarkable *changes in the symptoms*, sometimes for the worse, and sometimes for the better; and often we cannot perceive any obvious reason for these fluctuations. But if this happen when no medicine is given, so also will it happen when the disease is submitted to treatment; and it requires more than a little care and discretion to avoid attributing the changes which so occur, to the remedy which was last employed. For example, the abatement or cessation of *headache*, after a few days have elapsed, is a natural phenomenon; whereas an inexperienced or a careless person might easily persuade himself that it had yielded to his method of treatment, and that it was a favourable omen: neither of which conclusions would, however, be warranted by the circumstance upon which it was founded.

There is yet another source of difficulty connected with the subject. I have shown you that not only individual cases of fever, but different epidemics, vary much in their character: so that a plan of treatment which was well suited to one epidemic, may be improper and even hurtful if indiscriminately applied in another.

These considerations may serve in some measure to teach us how it has happened that so many *different*, and sometimes *opposite* remedies and modes of treatment have been recommended by different practitioners for the cure of continued fevers. The distinctions established by Sir William Jenner, while they somewhat disturb our previous confidence in the teachings of past experience on this subject, will render all future experience more exact, and probably more uniform also.

The natural tendency to a termination in health was very plainly visible in the epidemic fever in Ireland, to which I have more than once referred; and which apparently was *relapsing* fever. The mortality among the patients who were placed in sheds upon straw, and left with very little medical care, and even without any great personal attention from others, was very small indeed. No one can form even an approximate judgment of this tendency, who has not seen the disease under several varieties of practice. Doubtless one rule which we derive from a clear perception of the same tendency is, that we should *not* interfere *unnecessarily*. *Ἀσκεῖν περὶ τὰ νουσήματα δύο* (says Hippocrates,) *ὠφελέειν, ἢ μὴ βλάπτειν.* Two objects are to be kept in view in the treatment of diseases: first, that we do the patient *good*; secondly, that *at least* we do him *no harm*. In all these exanthemata, he must be reckoned the safest and the best practitioner who knows when to abstain from acting, as well as when to act; in other words, who has learned when, and to what extent, the case may be left to the salutary processes of nature.

However, there is an opposite error to that of mischievous activity. The tendency to recovery which manifests itself under different modes of treatment, and even in spite of opposite modes, has induced, in some minds, a degree of scepticism as to the utility of any remedies, that may easily be carried too far. It does not follow, because the majority of patients under continued fever would at length emerge into health, although no remedial measures were employed, that the disease ought therefore to be abandoned to what Cullen calls the *vis medicatrix naturæ*. It is not quite correct to say, with the older pathologists, that the whole disorder being merely an effort of nature to throw off something noxious to the system, it is *therefore* not to be interfered with. The true view of the matter I apprehend to be that which a toxicologist might take. The disease is produced by a poison of which the injurious impression upon the animal economy at length ceases, or passes off, of itself; in the same manner, only more slowly, as the influence of a dose of opium will spontaneously pass away. But during the natural course of the fever, as in many other cases of poisoning, morbid processes are apt to be set up, which, if suffered to proceed unchecked, would inflict irreparable injury upon important organs, and which are fairly within the scope of curative management. Tendencies accompany, or conditions survive the fever, which remedial measures, opportunely and judiciously applied, avail to oppose and to control. Our object must be, when the fever is once established, to conduct it to a favourable close; to 'obviate the tendency to death.' Upon this point I agree most entirely with Pitcairn, who, being asked what he thought of a certain treatise on fevers, declared, 'I do not like fever curers. You may *guide* a fever; you cannot *cure* it. What would you think of a pilot who attempted to quell a storm? Either position is equally absurd. In the storm you steer the ship as well as you can; and in a fever you can only employ patience and judicious measures to meet the difficulties of the case.'

When some immediate change ensues in the symptoms or in the feelings of the patient upon the administration of remedies that are generally followed by sensible effects, we are warranted in ascribing the change to those remedies. But even here comes in the fallacy already noticed, arising from the sudden and *spontaneous* changes that are apt to occur in fever: and this fallacy is to be got over only by multiplying our observations.

After all, the best guide that you can have in determining upon the general principle of treatment in a given epidemic, or even in an individual case, is that which Dr. Alison has so ably enforced in his lectures and in his writings upon this subject. I mean the observed tendency to this or that mode of dying. The manner and circumstances of the deaths, are of more practical importance than of the recoveries.

It was once a favourite practice with physicians to attempt to *cut short* the fever at its outset: and the two expedients which were chiefly relied upon for that purpose were *emetics*, and the *cold affusion*. They have both of them, in this country, gone very much out of fashion. In truth, neither reason nor experience encourages us to look for such a result from such measures. If fever depend upon a poison in the blood, it is not to be dislodged by the act of vomiting, nor washed out by the forcible descent of cold water upon the skin: and in the few instances in which the one or the other of these remedies may have *seemed* to arrest a fever, or to check its progress, that effect has always occurred at the very commencement of the complaint: so that we cannot be sure (and the probability lies the other way) that these were really cases of specific fever at all, or that they would not have ceased even if nothing had been done for them. Or they may have been instances of the merely ephemeral fever which is spoken of as *febricula.* Perhaps emetics may, in the present day, be too much neglected. I have no notion of their *stopping* the fever; but when given early, especially if gastric disturbance be a prominent symptom, they are sometimes followed by a marked abatement of many morbid sensations. 'It is astonishing,' says the observant Sydenham, 'how it happens that a vomit, which does not produce either a large or a morbid discharge from the stomach, should so materially relieve the nausea, restlessness, anxiety, and furred tongue of the patient.'

The cold affusion is not more effectual in cutting fever short than the treatment by emetics; and it has this great disadvantage, that it fatigues and alarms the patient: and when the vital powers are naturally feeble, or are much depressed by the disease, the very shock of the affusion may be attended with injurious consequences.

A modification of this expedient is, however, often of great use in abating the morbid heat, and soothing the uneasy feelings of the patient. I mean the practice of cold or tepid *sponging* of the surface. This is one of the remedies which, when the symptoms appear to indicate it, ought to be tried; and the propriety of continuing or discontinuing it may be determined by a very simple test, namely, the feelings and wishes of the patient himself respecting it.

Where choice can be made, a large apartment should be selected for the sick person. Unless the weather be very hot, there should be a fire in the room, for it acts as a ventilator. The air of the chamber should be kept fresh by having a window or a door (according to the weather) always open; or both window and door. Bed and window curtains, carpets, and all superfluous articles of furniture should be removed. Great diligence should be used in keeping the patient clean, by the requisite ablutions, and by frequently changing his sheets and his body linen; and these should be immersed at once in water, in which Condy's fluid, or chloride of lime or of zinc, or carbolic acid, has been mixed; and all discharges from the sick person's body—and especially the alvine dejections in enteric fever—should be received into a vessel containing some disinfecting fluid, and carried instantly out of the room.

All unnecessary intercourse with the patient, by his family and friends, should for his sake as well as for theirs be forbidden. As life advances the susceptibility of the typhoid disease at least seems to diminish, for which reason the nurses and other personal attendants of the patient should not be very young. And it is obviously desirable, whenever it may be practicable, to select such persons for those offices as are known to have themselves already passed through the particular species of fever that they are required to tend. All who approach the sick bed should take care to avoid as much as possible inhaling the patient's breath, or the emanations which proceed from his person. They should, therefore, in nautical language, keep to leeward of him. After attending upon him, they should not eat with unwashen hands. Friends who visit the apartment at intervals only should never enter it *fasting*.

In none of the three species of fever can venæsection be requisite or defensible. But there are incidental complications in which topical bleeding to a small amount may be employed with advantage to the patient.

I am always desirous that his hair should be cut off. The mere removal of it is often attended with benefit; the headache and confusion of thought are relieved, and the patient is calmed. We can then also, with much greater convenience and effect, apply cold washes to the head. Patients sometimes demur to this shaving of their heads; but they generally consent if you explain to them that their hair will at length fall off, in consequence of the fever; and that the head, if on that account only, had better be shaved at once. The head and shoulders should be somewhat raised, and thin strips of linen, kept constantly wet with some cold lotion, should be constantly applied upon the forehead and scalp. It should be the business of one person to attend to this. You would be surprised at the rapidity with which the cloths sometimes dry.

Now, with regard to this remedial measure you need not have any difficulty. It will do good, and should be steadily employed, so long as it is *grateful to the feelings* of the patient; and it *will*, generally, be pleasant and agreeable to him so long as the head remains morbidly hot. If the temperature of the scalp be not above the natural standard, and especially if the cold application make him shivery or uneasy, and give him annoyance instead of comfort, then it must be at once discontinued.

If the patient complained of intense headache, and his face were flushed and the heat of the surface great, and he were wildly delirious, and his pulse were hard, I would put leeches to his temples, or behind his ears, or take a few ounces of blood from his neck by means of cupping-glasses, and at the same time apply assiduously the cold lotion. The combination of headache with delirium warrants the suspicion that inflammatory mischief may be going on in the brain. Sir William Jenner observes, of all these species of fever, that 'after the patient becomes delirious he never complains of headache, and rarely admits its existence even when questioned concerning it; while in cases of intracranial inflammation headache is constantly and even loudly complained of, after delirium has commenced.'

In the outset of the disease, and while its species may as yet be uncertain, if the bowels have not been already purged by nature or by art, it will be right to give a couple of grains of calomel at once, and to follow up this dose by a warm rhubarb draught. Except in this way, mercury has not been found a serviceable remedy in these diseases. The common saline draught will generally be useful and refreshing to the patient; who may be allowed to drink toast and water also, or barley-water, in small quantities, as often as he wishes. As the disorder goes on, if the rose-coloured spots declare it to be of the typhoid species, and if diarrhœa arise, the state of the abdomen should be carefully investigated; and when tenderness is found to exist in the cæcal region, with gurgling perhaps felt under gentle pressure with the hand, a light poultice may be applied over the tender spot; and should the tenderness be extreme, the poultice might be preceded by a few leeches. Should the diarrhœa persist, and become profuse, a small quantity of Dover's powder, or of the extract of poppy, or some mild astringent, may be added from time to time.

But if the fever should prove to be typhus, and to exhibit a strong and early tendency to depression of the vital power, with a signal loss of muscular strength, a confused and dusky countenance, a mottled state of the skin, simulating the eruption of measles, a dry dark-brown tongue, and a feeble pulse—under these circumstances you must begin very early to give the patient a full allowance of good beef tea, or milk; and these may be thickened with arrowroot, or with the broken up yolk of an egg: and if the symptoms of prostration become more pronounced, you may add ammonia in full doses, Hoffman's æther, or what is much the best of all, wine: and you must omit the mercury.

Under this kind of management the patients will often go on, in a doubtful state, for some days, and at length begin to recover. Many of them, in both species of fever, but especially in typhus, sleep heavily, as the disorder passes slowly off.

There is one point in the treatment of fever, of exceeding importance, and of some nicety: I mean the use of *opiates*. When they are given inopportunely, they are apt to puzzle and perplex the case. You do not know how much of the disposition to coma is owing to the disease, and how much is the consequence of the remedy. Again, you may easily *augment* the natural tendency to somnolence, and lull your patient into a fatal stupor. But, when judiciously administered, opium will often save a patient who would inevitably sink without it.

It is in that form or phase of fever which the French call the *fièvre ataxique*,—when the patient is affected with delirium, restlessness, wakefulness, and spasm, and the disturbance of the nervous system outruns the disturbance of the sanguiferous system,—that opium is so beneficial. The condition of the patient resembles that of a person in *delirium tremens*. It is said that these symptoms occur most commonly among patients in those ranks of life that are above the lowest ranks; and it probably is so: but they are apt to take place in any patients, high or low, rich or poor, who have had the mind overwrought, and the nervous system unstrung, whether by dissipation and intemperance, or by anxiety of any kind. Sydenham was quite aware of the existence of this particular set of symptoms, and of the remedy for them. Of all these symptoms, *sleeplessness* is the most urgent. Dr. Grattan and Dr. Latham have both written in praise of the same opiate treatment, under such circumstances, as was recommended by Sydenham. Dr. Grattan observes, with great truth, that two or three nights spent in restless delirium are followed by the worst consequences; and that patients who pass *three* nights in succession in that way, almost invariably die. If the symptoms be well marked, the best mode of proceeding is to give a tolerably full dose of opium in the evening; one-third of a grain of acetate of morphia is a common prescription with me. The amendment of the patient, on the following day, is often very striking. Unless the same symptoms recur, it is better, I think, not to *repeat* the anodyne. But, as Dr. Latham cautions us, ‘there are cases, where the indications for the employment of opium are *doubtful*. Wild delirium, and long wakefulness, and a circulation weak and fluttering, seem to call for a considerable dose of opium. Yet, withal, there is a certain jerk in the pulse, so that we cannot help *suspecting* that the blood-vessels have something to do with the sensorial excitement. Under such circumstances, I have certainly seen (says he) twenty minims of laudanum produce tranquil sleep, from which the patient has awoke quite a new man. But I have also seen the same quantity produce a fatal coma, from which he has never been roused. Now (continues Dr. Latham) since it is a fearful thing to strike a heavy blow in the dark, where the alternative is of such magnitude, it is the safest and best method to administer a small dose, at intervals of an hour or two: so as to stop short of actual mischief at the first glimpse of its approach, or to be led, by a plain earnest of benefit, to push the remedy to its full and consummate effect. Many doses may be required for this purpose; but we shall see, after the first or second, whether to go on or to desist.'

I think it probable that the newly-discovered remedy, chloral, may prove even more suitable than opium for these cases.

When, as sometimes happens, the stress of the disorder falls upon the thorax, and there is much dyspnœa, with the sounds that denote inflammation of the bronchial membrane, or of the pulmonary substance, leeches or cupping-glasses may be applied to the chest; and, in milder cases, a blister, or a mustard cataplasm.

These remedies—cold to the shaven head; the local abstraction of blood wherever there happens to arise unequivocal evidence of local inflammation; a purge at first, and mild aperients afterwards, if the bowels are confined or sluggish; moderate astringents, if there be much or urgent diarrhœa, a few grains of Dover's powder, for example, or of the extract of catechu; opium in a more efficient dose, when the nervous symptoms are prominent, particularly sleepless delirium and restlessness; and in many cases, *early* support by animal broths, and even by wine—these remedies, adapted to the particular circumstances of individual patients, form the staple of the treatment of continued fever, according to the best of my judgment and experience.

After what has been said, I need scarcely again admonish you to study carefully, not merely the symptoms of any particular case to which you may be called, but the general character of the fevers that are at the same time prevalent, and the manner of dying in the fatal cases. If you find that they who die, die chiefly in the way of asthenia, that will be a strong reason for caution with respect to any removal of blood, and for the early employment of beef-tea, egg, milk, and other means of support. If plenty of beef-tea do not suffice, you must give the patient wine, and that sometimes to a very large amount, or even brandy; the egg-flip of the Pharmacopœia for instance, the *mistura vini gallici.* The object is to keep him alive, to keep the heart in motion, until the depressing influence of the exciting cause of the disease shall have passed by. If the wine should flush or excite him, or render the pulse hard, it must be diminished in quantity, given less frequently, or omitted altogether. If there be indications of local inflammation—pain (for example) in the cæcal region, increased by pressure—leeches may be used, and wine given at the same time. There is nothing inconsistent in such mixed practice. We seek to remove the local inflammation by unloading the capillary blood-vessels in or near the part, while we uphold the general powers of the constitution which are ready to sink. The great art of getting a fever patient through a bad attack, is to have him judiciously and perpetually watched, by night and by day. The remedy that is proper one hour, may do harm if pushed during the next. And there is another reason, which I have mentioned before, why the sick person should never be left alone, even for a moment. I have heard of more than one or two instances of patients, in the delirium which attends the disease, getting out of bed, and out at the window, during the temporary absence of their nurse, and perishing from the fall.

When the patient relishes and wishes for the beef-tea, or the wine, that is no equivocal warrant of the propriety and usefulness of its administration.

A word or two more may be proper, even in this cursory sketch, respecting certain incidental points of practice that are apt to arise.

Great comfort and soothing may generally be obtained for the patient, when the abdomen is uneasy, by the application of a large warm linseed-meal poultice; or of the epithem called *spongio-piline.* In the latter periods of typhoid fever it is not uncommon, even when pain is no longer excited by pressure made upon the belly, for the diarrhœa to persist, and for a troublesome degree of *meteorismus* to come on; a tympanitic distention of the intestines with gas. Under these circumstances, a large *blister*, laid over the abdomen, has often very happy effects, upon both the diarrhœa and the meteorismus. Clysters containing a drachm of the confection of rue are commended for the same purpose. Do not, however, be too anxious about the diarrhœa. If it be very urgent and profuse, it may be moderated by opiate or astringent enemata. But be careful not, by frequent or paralysing opiates, to lock up within the intestine its morbid secretions, until they decompose, and so *produce* gaseous distention, and even increase the risk of rupture of the ulcerated bowel.

Again, if the patient sink into profound coma, a blister should be applied to the shaven scalp. The sick man will sometimes awake from deep stupor while the blister is still rising.

It is always a matter of importance, as I stated before, to inquire carefully, every day, into the condition of the bladder of these fever-patients. In the state of stupor and indifference in which they often lie, they appear not to be sensible to the want of passing the urine, which collects in the bladder, and distends it enormously; not only increasing the present hazard of the patient, but laying the foundation, sometimes, of future disease of the kidneys, in case he recovers from the fever. You must not take the assurances of the nurses upon this point. They will often tell you that the patient has made plenty of water, when in fact the urine has been dribbling away from him, overflowing, while the bladder is stretched to the utmost. Feel, therefore, with your hand, and percuss the hypogastric region, as well as that of the cæcum, at every visit.

It is requisite, too, that the under surface of the patient's body should not only be kept scrupulously dry and clean, but be *looked at* every day, or twice a day. If the projecting points, the hip bones, the sacrum, the shoulders, the elbows, should become *red*, that is a sign that they are likely to slough or ulcerate. This evil consequence of continued pressure upon parts of which the vital power and healthy tone are lowered, may often be prevented by washing the erythematous spots with brandy. Dr. Murchison recommends painting the surface with a solution of sheet gutta-percha (one drachm) in pure chloroform (one ounce), and thus placing a protecting film over it. Should the skin be already broken, the place may be covered with soap plaster; or with the *amadou* plaster, lately introduced by Mr. Wetherfield, which does not wrinkle or ruck up. An adjustment of pillows and of posture—or the water-bed—or the water-cushion—may sometimes supersede the necessity of these local expedients.

When ulceration of the mucous follicles perforates the bowel, that catastrophe does not *always* so distinctly declare itself in fever, as in other cases. I have seen such perforation, and its resulting peritonitis, when no complaint of pain had been made by the patient, so great was his insensibility. In general, however, the rupture of the gut is denoted by symptoms which cannot be mistaken. When it does occur, there is but little chance of the patient's recovery; and that little will be best husbanded by the adoption of the plan of treatment which I formerly laid down: opiates, to check the peristaltic movements of the intestine; and a rigid adherence, for some days, to the horizontal posture. Sir William Jenner, indeed, concurs with Rokitanski in believing that perforation of the intestine in typhoid fever, even though the passage of its contents into the cavity of the peritoneum may be for a while prevented by adhesions, is always fatal at last; that the adhesions are never permanent. It is well to bear in mind the risk of hastening, or even of causing, this fearful accident by careless or heavy pressure of the hand upon the abdomen, in a late stage of the disease.

I have seen a few instances, in which an œdematous swelling of one leg and thigh has occurred in the advanced stage of typhoid fever, like that which is incidental to parturient women, and depending upon the same cause—inflammation and obstruction of the great vein that returns the blood from the limb towards the heart. I believe that the inflammation extends itself, in such cases, from some of the smaller veins of the mesentery, which have been injured by the intestinal ulceration. Fomentation of the affected limb comprises all the remedial treatment which this accident requires or admits of.

The management of the patient during convalescence is scarcely of less importance than during the progress of the fever. The chief danger is, that his desire to be allowed to get up, and his wish to eat animal food, should be too soon indulged. The latter of these errors is more frequently the cause of a relapse than any other circumstance; and relapses are often more perilous and difficult to remedy than the original malady. You must be prepared, therefore, to withstand the solicitations of the patient and of his friends, who think that if strength be wanting, strong drinks and plenty of meat, are the things to impart it. Until the tongue is quite clean and moist, and of its natural colour, and the pulse has lost all its undue frequency, and the skin its excess of heat, the patient must be kept to broths, jellies, puddings, and preparations of the well-known farinaceous articles of food. Then he may begin with some boiled white fish, and so gradually eat his way, through chicken, and a mutton chop, to his ordinary diet again. I have already told you that the convalescence from typhus fever is generally rapid, from typhoid often lingering.

Such, I say, is the general plan of treatment which some observation of this disorder has persuaded me is the best. Summarily expressed, it consists in the exercise of incessant vigilance, and the adoption of the proper remedy at the proper moment. It lies between a timid or sceptical abandonment of all known resources, and a meddlesome rashness in applying them. The flame of life may be suffered to expire for want of timely succour and tending, by the practitioner who folds his arms, and looks on; as it may be rudely extinguished by a restless or routine interference which has no definite or intelligible purpose. Boerhaave, in the preface to his 'Aphorisms,' professes that he knows of nothing which can be fitly termed a *remedy*, 'quin solo tempestivo usu tale fiat.' In fevers the wisdom of this maxim is eminently conspicuous. The rational objects of treatment are, to mitigate the urgency of symptoms that cannot be wholly subdued, to redress (so far as art may redress) those dangerous complications which are incidental but not essential to the disease; and to aid the conservative efforts of nature, when these manifestly languish and fail.

Dr. Robert Williams held that enemata of warm water and syrup of poppies would do all that could be done beneficially. For some time he lost only one patient out of sixty-three thus treated. This was a most encouraging result. But then, when the fever changed in species, and typhus began to prevail, as it did about the period of the arrival of the epidemic cholera, he lost one in every four or five: a very large mortality. These facts illustrate, in a strong manner, the necessity, which I have so often endeavoured to inculcate, of taking into account, when we would estimate the value of a particular remedy or plan, the difference which obtains in different epidemics, whether bad, good, or no treatment whatever be adopted. A far surer method is to compare (if you would experiment at all) two or more different modes of practice in different cases of the *same* epidemic. Thus Dr. Latham, finding during one season that his wards were full of fever, while yet its type was so mild that scarcely any died, thought this a favourable opportunity for trying whether mercury had any beneficial operation upon the disease. Accordingly, he treated half his cases with small doses of the *hydrargyrum cum cretâ*; and the other half with the *liquor ammoniæ acetatis,* and so forth, and no mercury; and he fancied that the patients in the first of these classes were, on the average, convalescent sooner than those in the last. Chomel is of opinion, from some trials, that the chlorate of soda is a useful remedy, in addition to the general plan of management. I have myself been in the habit of giving to all my fever patients a drachm of the chlorate of potass dissolved in a pint of water, as a daily drink. Dr. Murchison states that he has tried the mineral acids, and especially the nitro-muriatic acid, in many hundreds of cases, and without ascribing to them the wonderful virtues which some writers have done, he believes them to be superior to any other general method of treatment. I can well believe this, and shall prefer to use these acids in any future opportunities of practice.

It may be right that I should notice briefly a method of treating continued fevers which has of late been strenuously advocated by Dr. Dundas. Led, apparently, by his own experience of its admirable effects in the malarious fevers of Brazil, which often run into the continued form, Dr. Dundas maintains that quinine, in large and frequently-repeated doses, is a specific cure for continued fevers as we see them here: to use his own words, that '*cinchonism* will be found to control generally the continued fever of this country in all its forms, in all its stages, and in all its complications.' The condition here called 'cinchonism' is marked, as you already know, by the occurrence of giddiness, deafness, and a sense of buzzing or some kind of tinnitus in the ears. Large and frequent doses of quinine exercise also a remarkable influence over the pulse, rendering its beats weaker, and slower.

This plan, thus strongly commended, has been tried with varying results by sundry medical practitioners. In some hands it has signally failed; in others it has seemed to prosper. Favourable reports are made of it from Dublin, from Drogheda, from Cork; from Liverpool, which is full of Irish poor; and from Manchester, which probably is not less so. In this town it has not succeeded, nor in Edinburgh. Dr. Barclay of St. George's Hospital, Dr. Peacock of St. Thomas's, and Professor Hughes Bennett of Edinburgh, report severally, as the upshot of numerous trials, that large and repeated doses of quinine neither cut short the fever, nor in any way favourably influenced its progress. If in any of our three species, I should have looked for success in the relapsing fever; which in some of its habits resembles the malarious fever, and which is much more common in Ireland than elsewhere, and among Irish emigrants to our large towns. But of the cases in which this quinine treatment is reported to have effected cures, some are expressly described as instances of maculated typhus, and others as instances of typhoid fever.

LECTURE LXXX.

Small-pox. Inoculation. Vaccination. Their comparative advantages. Treatment of Small-pox.

I PASS, without pausing, from the consideration of continued fever, to that of *small-pox* or *variola*; a disease, fortunately, less common in this country than it used to be, yet still sufficiently frequent and formidable to require that we should acquaint ourselves with the phenomena it is accustomed to present; and very rife and fatal here, as it happens, at present (1871). I have already mentioned, by anticipation, several points in its history.

This frightful disease sets in with smart febrile symptoms: rigors, followed by heat and dryness of skin, a hard and frequent pulse, pain in the epigastrium, with nausea and vomiting, and headache. Sometimes wild delirium, sometimes convulsions, attend its outset. Then, to use the words of Cullen's definition, 'tertio die incipit, et quinto finitur, eruptio papularum phlegmonodearum, quæ spatio octo dierum, in suppurationem et in crustas demum abeunt, sæpe cicatrices depressas, sive foveolas, in cute relinquentes.'

When small-pox is fully formed, it cannot be mistaken for any other complaint: but it is of some importance to recognise it at its very commencement, for the force of the impending disorder may sometimes be lessened by judicious measures adopted at that early stage. The symptoms, however, that mark the outset of all febrile diseases are necessarily very much the same. If the pyrexia set in when small-pox is prevalent in the neighbourhood, if the person in whom it occurs be an 'unprotected' person (i.e. one who has neither had that disease, nor been vaccinated previously), and especially if he be known to have been exposed, within from nine or ten days to a fortnight, to the contagion of variola, we may well suspect that the disease will turn out to be small-pox, and act upon that suspicion.

Nevertheless there are some symptoms which, being common in the commencement of variola, and not common at the beginning of the three continued fevers just considered, or of the other exanthemata, may assist the early diagnosis. Vomiting is one of these; pain in the back another. When these symptoms are violent, they usually usher in a severe form of the disease. The same may be inferred from a continuance of the nausea and vomiting, after the coming out of the eruption; which is very unusual. Heberden noticed that acute pain *in the loins* was almost always followed by a severe disorder; that pain higher up, between the shoulders, was of better augury; and that it was to be reckoned in all cases a good sign, if there were no pain in the back at all. Early delirium, stupor, or convulsions, announce severity in the subsequent course of the malady. Yet not always, especially in children. Within the last month I was asked to see a child which had been suddenly attacked with convulsions, followed by coma. In due time the eruption of variola appeared, and the disease ran a mild course, with little aid from medicine, although the child was previously unprotected. Children do not shiver. Convulsions in the outset of a febrile attack are in them what rigors are in adults.

The peculiar eruption almost always begins to show itself on the third day of the fever. The earlier it comes, the severer generally does the disorder prove. In judging of the date of the eruption, you must bear in mind that parents and servants are apt to state its accession to have been later than it was in reality: for the spots are at first so minute that they often escape observation. They also frequently begin to come out in the night; and the morning of the second day of the eruption is then called erroneously the first day.

The eruption comes out first on the face, then on the neck and wrists and on the trunk of the body, and lastly on the lower extremities. Such is the rule; so that (as is specified in the definition) it does not cease to come out till the fifth day: and it keeps a-head, in that order, throughout the disease. There are, indeed, some exceptions to this rule. Occasionally the spots appear first upon the extremities, but this is very rare. In some instances straggling papulæ continue to spring up after the main crop is fairly completed; but these stragglers seldom attain the same size with the others.

The pimples, or papulæ, ripen gradually into *pustules*, the suppuration being complete by their eighth day; and on that day the pustules generally begin to break, and crusts or scabs to form. In four or five days more the scabs are falling off. There are some variations in all this also. In children, the crusts are sometimes visible on the seventh day: and in adults, when the disease is severe, they sometimes do not begin to form till the ninth day. In all cases some of the pustules are liable to be prematurely broken, by accident, or by the patient's scratching; and these will crust over earlier than they otherwise would have done. So that in fixing the period of incrustation, you are to regard those pustules only, of which the natural progress has not been interfered with.

All that I have hitherto been saying applies, with more or less exactness, to the disease in all its varieties. But its severity differs exceedingly, as I have already hinted, in different cases. Its severity, in truth, is almost always in direct relation to the *quantity of the eruption.* The number of pustules indicates, in the first place, the quantity of the variolous poison which has been reproduced in the blood. In the second place, it is also a direct measure of the extent to which the skin suffers inflammation. Sometimes there are not more than half a dozen pustules; sometimes there are many thousands. If all these were collected into one, it would be an enormous abscess. For both these reasons the system suffers commotion, distress, and peril, in proportion to the quantity of the eruption.

When the pustules are very many, they run together: when they are few, they are separated from each other. And this affords a broad line of distinction, which can neither be overlooked nor mistaken, into the variola *discreta*, and the variola *confluens.* In the one, the pustules are distinct, and of a regularly circumscribed circular form. In the other they coalesce, and their common outline becomes irregular. Now the discrete form of the disease is scarcely ever dangerous; the confluent form is never free from danger. The distinction therefore is of the highest importance and interest. For its full estimation, each form must be considered separately.

In the discrete variety, in which the disorder may be presumed to run its most natural course, the eruption is at first, according to the phraseology of Willan, *papular.* The pimples gradually increase in magnitude, but it is not till the third day of their appearance that they begin to contain a little fluid on their summits. For two days after this they increase in breadth only, and a depression is observable in the centre of many of them. The cuticle is bound down there somehow, for a time, to the cutis vera. It is the eighth day *of the disease*, or the *fifth* day *of the eruption,* before the pustules become perfectly turgid and hemispheröidal. During the time in which they are thus filling up, the face swells; often to so great a degree that the eyelids are closed; and the natural aspect suffers a complete and hideous change. The skin between the pustules on the face assumes a damask red colour. About the eighth day of the eruption, a dark spot makes its appearance on the top of each turgid pustule, and at that spot the cuticle breaks, a portion of the matter oozes out, and the pustule dries into a scab. When this crust at length falls off, it leaves behind it either a purplish red stain, which is still very characteristic of the disease, and which very slowly fades; or a depressed scar, which is indelible; and ultimately white. In the latter case the patient, or more properly his skin, is said to be *pitted* with the small-pox, or pock-marked. The swelling of the face begins gradually to diminish after the eruption has become thoroughly pustular.

This is the course which the eruption pursues on the face, where the pustules, even in the discrete form of the disease, are usually thicker set than on any other part of the surface. And it pursues the same course, only two or three days later, upon the extremities, where it also begins later. The feet and hands swell just as the face swelled, but they begin to tumefy as the features begin to subside. Some of the pustules, especially on the extremities, do not burst at all, but shrivel up.

In this, the distinct variety of the disorder, the fever generally ceases entirely upon the coming out of the eruption: the headache, the pain of the back, the vomiting, the restlessness, abate and disappear, the pulse resumes its natural force and frequency, and the skin its natural temperature. About the seventh or eighth day of the eruption there is commonly for a day or two a recurrence of fever. This is called the fever of maturation.

You are to observe that we judge of the eruption as it appears on the *face*. The disease is of the confluent kind, when the pustules are confluent there, whether they are so or not upon the trunk and extremities. Sometimes they are neither strictly confluent nor strictly separate, but stand just thick enough to touch each other, without absolutely coalescing; every pustule preserving its circular outline. In that case the disease is said to be of the *cohering* form. When the pustules are confluent over the whole body, their number is often prodigiously great, and their progress is less regular than in the discrete and milder variety of the complaint.

In the first place the eruptive fever itself is usually more violent and tumultuous in the confluent disease: the disturbance of the sensorial functions is more common and more decided, the sickness more distressing, the pain of the back and loins more severe; the temperature higher. The eruption comes out earlier, and more confusedly; the pimples being at first very minute, and crowded together in patches, and not seldom preceded and accompanied by a rash like that of scarlet fever, or erysipelas: whereby the diagnosis, in so far as it depends upon the appearance of the skin, is rendered for a while uncertain. I have at present in the Middlesex Hospital a patient in whom the papulæ of small-pox were, at the outset, so intermingled with the appearances and sensations of urticaria, that I doubted, for twenty-four hours, what the true character of the eruption might be. It is sometimes like that of the measles; but the similarity and the uncertainty are soon at an end, for the pimples—which from the first are much harder than those of measles, and feel like small shots under the skin—soon begin to exhibit a fluid on their summits. They do not, however, as they advance, and pass into pustules, fill up so completely as in the distinct form; they are flatter, less plump, more irregularly depressed, and even of a different colour; being at first whitish, and then of a brown tint, and seldom of the yellow purulent hue which is seen in the variola discreta. Sometimes they are even bluish, or purple. In the confluent form there is commonly some abatement of the febrile distress upon the coming out of the eruption, but the remission is much less decided than in the discrete. About the fifth or sixth day fresh rigors are apt to occur, marking the fever of maturation. Most of these points of distinction between the two varieties of the disease are well set forth in Cullen's definitions. The distinct form he defines thus: 'Variola (discreta) pustulis paucis, discretis, circumscriptione circularibus, turgidis; febre, eruptione factâ, protinus cessante.' And of the confluent kind his definition is, 'Variola (confluens) pustulis numerosis, confluentibus, circumscriptione irregularibus, flaccidis, parum elevatis; febre post eruptionem perstante.'

But the most important difference between the two forms is in what is called the *secondary fever*, which sets in about the eleventh day of the disease, or the eighth of the eruption, just when the maturation of the pustules is complete, and they begin to desiccate. This secondary fever is slightly marked in the distinct small-pox, and very intense and perilous in most instances of the confluent. It is at this period of the disorder, that death, in the fatal cases, oftenest occurs. Of 168 such cases, recorded by Dr. Gregory, the deaths happened in twenty-seven (nearly one-sixth of the whole) upon the eighth day of the eruption. That, therefore, is the most perilous *day*, as the second is the most perilous *week*. Thirty-two died in the first week, ninety-nine in the second, twenty-one in the third. The early occurrence of death denotes a peculiar malignancy in the disease. The nervous system appears to be overwhelmed by the force of the poison. During the second week the disorder proves fatal chiefly in the way of apnœa; from some affection of the respiratory passages. After that period the characters of asthenia commonly predominate. The patient sinks under some casual complication, or the powers of life are gradually worn out by so much irritation of the surface, and so large an amount of suppuration.

So much for the ordinary course of small-pox, and of the symptoms that are essential to it in its distinct and in its confluent form. I have yet to mention some other circumstances that are very frequently to be noticed in connexion with the disease.

Both kinds are accompanied by *sore throat*; the tonsils and fauces are tumid and red: and with this sore throat there is associated, about the period when the face swells, sometimes in the discrete variety, and almost always in the confluent, more or less *salivation*, which lasts for several days. At first the discharge is thin and plentiful: but, towards the period of maturation, it often becomes viscid and ropy, and is with difficulty got rid of by the patient. This salivation is of some importance as a *prognostic* symptom. If it cease abruptly, and especially if at the same time the swelling of the face suddenly and prematurely subside, the peril is great. Besides this, Sydenham regarded the ptyalism as a *diagnostic* circumstance; as a mark which identified with true small-pox a fever called by him the variolous fever, the *variolæ sine variolis* of De Haen and others. 'The resemblance (says he) which this fever bore, in its symptoms, to small-pox, induces me to give it the title of variolous fever, which seemed indeed so much the more appropriate, as the fever raged at the same time with small-pox, and got well under the same treatment. The two diseases belonged evidently to one family, and there was no difference between them, saving that in small-pox the morbific matter was directed towards the skin, in the shape of an eruption; while in the variolous fever this matter was expelled from the system by the salivary glands.' Notwithstanding this statement, it is difficult to believe that any such disorder as variolæ sine variolis ever proceeds from the contagion of small-pox.

This affection of the salivary glands does not so often occur in children; but diarrhœa appears sometimes to take its place.

The soreness of the fauces often depends, in great measure, upon pustules there situate. You may see that the tongue, the roof of the mouth, the inside of the cheeks, the uvula, and the velum palati, are thickly studded with them. It is affirmed by some writers that the pustules of small-pox occur in various internal parts of the body, and especially upon the mucous membrane of the intestinal canal. I believe this to be a mistake. The enlarged solitary follicles often put on very much the appearance of pustules. Cotunnius, who has written a good book *De sedibus variolarum*, asserts that pustules appear only upon the skin, and upon those parts of the mucous membranes which are freely exposed to the air. In one solitary instance he met with them in the trachea so low as its third ring. He fancied that previous desiccation of the part was necessary to their formation. He says that none appear on the cornea, while it is kept moist. He denies that they are seen in the interior of the body, or upon the fœtus in utero: but in this last point he is certainly wrong; and this is a strong objection to his theory. The most striking facts which he alleges in support of his opinion of the necessary presence of air—besides the fact of the eruption being more copious on parts usually exposed to the atmosphere, as the face and hands—are, that pustules do not form on the inner surface of the eyelids, except in cases of *ectropium*; that they appear on hæmorrhoidal tumours only when these project beyond the margin of the anus; and that that portion alone of the glans penis is ever affected by them which is uncovered by the prepuce.

Dr. Gregory and Mr. Marson both state that true variolous pustules very rarely indeed form upon the conjunctival membrane, and never upon the cornea: and that the blindness of one or both eyes, so common a result of small-pox, especially in children, is caused by conjunctival inflammation and ulceration of the cornea, which occur at the period of the secondary fever, and rapidly spoil the transparent tissues of the eye.

The pustules are apt to be more copious on parts of the skin which have undergone some recent irritation, on the site of a late blister for example, or on the place of a blow. A patient of mine, who had confluent small-pox a second time at the age of forty-five, having been inoculated in early life, complained, about the sixth day of the eruption, of smarting pain in her feet and legs. On being looked at, both feet and ankles, *up to the usual limit of her boots*, were seen to be thickly set with pustules, while above these lines there were comparatively few.

During the period of maturation, a peculiar greasy, disagreeable odour, quite *sui generis*, proceeds from the body of the patient. If taken into the sick chamber blindfolded, one might name the disease at once by this smell. About the same time also many patients are tormented by itching of the surface; so that they are provoked to scratch off the heads of the pustules; and by so doing they often ensure the formation of pits. In many cases of confluent small-pox this itching seems to constitute the chief part of the patient's suffering.

Sir James Emerson Tennant states that the leopards (or panthers) in Ceylon 'are strongly attracted by the peculiar odour which accompanies small-pox. The reluctance of the natives to submit themselves or their children to vaccination exposes the island to frightful visitations of this disease; and in the villages in the interior it is usual, on such occasions, to erect huts in the jungle, to serve as temporary hospitals. Towards these the leopards are certain to be allured; and the medical officers are obliged to resort to increased precautions in consequence.'

There are various troublesome complications to which, in severe cases of the confluent form, the patients are liable during the secondary fever; erysipelatous inflammation involving the subcutaneous areolar tissue in various parts of the body, and leading to the formation of abscesses; glandular swellings in the groins and axillæ, going on sometimes to suppuration; sloughing sores on the hips and sacrum; phlebitis; and in two or three instances I have seen the large joints, after death, full of matter. One of the most serious symptoms, at this period of the disease, is dyspnœa. The air passages, and especially the larynx, become clogged by viscid mucus, the arterialisation of the blood is interfered with, and the patient is in danger of suffocation. Occasionally life is suddenly extinguished by œdema of the glottis, supervening upon that affection of the fauces which I mentioned just now.

Mr. Marson's experience has taught him that a *variety* of the disease, called variola *corymbosa*, because the pustular eruption appears in *clusters*—which, whether few or many, are mostly distributed symmetrically on the limbs and body—is a very fatal variety.

In one most fearful phase of this always formidable disorder, symptoms indicative of what is called the putrid diathesis manifest themselves—petechiæ, vibices, hæmorrhages from various parts of the body. The pustules, instead of being plump and yellow, are flat, red, purple, or blue; that is, they contain blood, or a sanious ichor, in the place of pus, constituting the *variolæ nigræ* of Sydenham, the *bloody* small-pox of Mead. I believe that these appearances augur in all cases a fatal result. Hæmorrhage from the uterus is not uncommon; and in pregnant women abortion, and then, most commonly, death. Heberden says that he examined, in many instances, the fœtuses so parted with, but never could perceive upon them any traces of small-pox. His experience, therefore, agreed with that of Cotunnius; and we may conclude that infection of the fœtus in utero is very rare. Yet, unquestionably, it sometimes happens; and the circumstances under which it has been noticed are various and interesting. In one instance, related by Mr. Flinders, the disorder was eight or ten days later in the fœtus than in the mother. A woman, near her full time, took small-pox. The pustules were mature about the 10th or 11th of June. On the 18th she gave birth to a full-grown boy, upon whose face and body there were many pustules, discrete, and nearly ripe. The child died the same night. It is a very curious fact that the fœtus has caught the disorder, doubtless through the medium of the mother, although she, having had it previously, was unaffected by the contagion. Dr. Mead relates that 'a certain woman, who had formerly had the small-pox, and was now near her reckoning, attended her husband in this distemper. She went her full time, and was delivered of a dead child. It may be needless to add, that she did not catch it on this occasion; but the dead body of the infant was a horrid sight, being covered all over with pustules.' In the first volume of the 'Medico-Chirurgical Transactions,' Dr. Edward Jenner gives an account of an infant which, upon the fifth day of its age, became indisposed, and on the seventh exhibited the eruption of small-pox; so that the contagion must have been communicated to it while yet in the womb. A few days before her confinement the mother of this child had seen in the street a person covered with small-pox pustules, the smell and sight of whose body had sensibly affected her. I see no reason, therefore, for doubting that the unborn being may pass safely through the disease while in the womb, and derive from that attack the customary immunity for the future. My namesake, Sir William Watson, describes, in the 'Philosophical Transactions,' an instance in which the scars left by the pustules were visible upon an infant at its birth. This child was afterwards inoculated without taking the disease. Its mother, who had formerly had it, nursed, when far advanced in pregnancy, a servant ill of small-pox. Dr. Pearson met with a similar example. Mary Spooner was inoculated by him in her sixth month of utero-gestation, and had the disease severely. Her child was twice inoculated with small-pox matter, but without effect.

Like all these contagious exanthemata, small-pox has its periods of dormancy, and its periods of activity. Every now and then, at irregular intervals—and, as it would seem to our ignorance of the cause, capriciously—it overspreads a district or country as an epidemic. At this moment (1871) it is more prevalent and more fatal in London, and in many parts of England, than it has been known to be for many years past. In general, when epidemic, it is also more than ordinarily severe; although different epidemics vary much in that respect.

There is no contagion so strong and sure as that of small-pox: none that operates at so great a distance. Dr. Haygarth states, 'that during his long attention to this subject, not a single instance had occurred to prove that persons liable to the small-pox could associate in the same chamber with a patient in the distemper, without receiving the infection.' It is readily communicable in every way; by inoculation, by breathing a contaminated atmosphere, by the contact or vicinity of fomites. 'A single breathing (says Mr. Marsden) of the air where it is, is enough to give the disease.' The same writer believes it to 'be communicable from the moment when the initiatory fever begins. It may be given by the breath of the patient before the eruption has appeared on the surface of his body. It continues infectious so long as any of the dry scabs resulting from the original eruption remain adherent to the body.' Nay, it may be caught from the dead body. Mr. Cæsar Hawkins has recorded an interesting example of this. The body of a man, who died of small-pox, was brought into his dissecting-room in Windmill Street; and four students took the disease from that source. Of these, one only had *touched* the body.

Clothes and furniture, especially woollen furniture, unless diligently purified, retain the infection for a very long time.

In all these diseases, it seems probable that the infecting power may be almost indefinitely prolonged in substances secluded from the air: although it is difficult to give credence to the statement which Southey somewhere quotes from Dr. Franklin, that during the last century 'several medical men who assisted in London at the dissection of a mummy died of a malignant fever, which it was supposed they caught from the dried and spiced Egyptian.'

There is one appearance which I think curious, although perhaps it has not any great practical interest; and which I omitted to notice when describing the course of the eruption. Without going minutely into the anatomy of the pustules, you may distinctly see, if you closely examine them when they are about five or six days old—you may see, at least, in many of them—two colours, viz., a central whitish disk of lymph, set in, or surrounded by, a circle of yellower puriform matter. In truth, there is, in the centre, a *vesicle*, which is distinct from the pus. You may puncture the vesicle, and empty it of its contents, without letting out any of the pus; or you may puncture the part containing the pus, and let *that* out, without discharging the contents of the vesicle. The vesicles have even, by careful dissection, been taken out entire; and they are said to consist of several little cells. It is most probable that the lymph contained in this separate vesicle is the purest part of the variolous poison.

Before I say anything of the measures to be adopted during the progress of small-pox, I have to bring under your notice two expedients of still greater interest and importance; the one of them contemplating a mitigation of the disorder, the other its total prevention. You anticipate that I am about to speak of *inoculated* small-pox in the first instance, and of the *vaccine disease* in the second.

I have many times stated, and all the world knows, that small-pox may be imparted to a healthy person by inserting beneath his cuticle a minute quantity of the matter taken from a variolous pustule. This, perhaps, is not very surprising; but it is surprising that the disease, so received, should be much milder than if it had been contracted in what is called the 'natural way,' by breathing an atmosphere charged with the contagious poison. Why it should be so it is difficult to conjecture. The fact is sometimes expressed by saying that the disease is milder when the virus is admitted through the cutaneous, than when through the mucous tissues. But I am not at all sure that the hypothesis involved in this proposition is true. No attempts have been made, that I know of, to introduce the poison artificially through a wound in any mucous surface. I should rather guess that the small quantity of the poison conveyed by inoculation into the blood may make the difference. But whatever the explanation, the fact is unquestionable, and obviously of the highest importance. By what accident it was first learned (for it evidently could not have been reasoned out) we do not know. The Chinese claim to have been in the habit for many centuries of *sowing* the disorder, by putting some of the crusts into the nostrils. But this is a different thing from inoculation, the surface being entire, and the effluvia from the crusts being drawn into the lungs by the act of inspiration. It is said that a true engrafting of the virus has been in use by the Brahmins in India, time out of mind. It certainly was practised in Turkey at the very beginning of the last century, and perhaps somewhat earlier. In 1713, Dr. Emanuel Timoni, an Oxford graduate, who had settled at Constantinople, wrote to Dr. Woodward, in London, giving him an account of the new process, and testifying to its success. This account was communicated to the Royal Society, and published in its 'Transactions' the following year. In 1715, Mr. Kennedy, an English surgeon who had travelled in Turkey, gave similar information to the English public in his 'Essay on External Remedies.' And in the 'Philosophical Transactions' for 1716 you may see a notice of the same process, as described by M. Pylarini, the Venetian consul at Smyrna. But these statements were neglected, or had no practical result. We owe the actual introduction of the practice of inoculation into Great Britain to the good sense and courage of an English lady, whose lively epistles have taken their permanent place in our country's literature. Lady Mary Wortley Montagu, the wife of our ambassador at the Ottoman Court, writes thus, from Adrianople, in the year 1718: 'The small-pox, so fatal and so general amongst us, is here entirely harmless by the invention of *engrafting*, which is the term they give it. Every year thousands undergo the operation; and the French ambassador says, pleasantly, that they take the small-pox here by way of diversion, as they take the waters in other countries. There is no example of any one who has died in it; and you may believe I am well satisfied of the safety of this experiment, since I intend to try it on my dear little son. I am patriot enough to take pains to bring this useful invention into fashion in

England.' In fact, she recommended it by her own example. The first person inoculated with the small-pox in England was her daughter. Then a child of a physician, Dr. Keith, who had visited Miss Wortley; afterwards some condemned felons, who were pardoned on condition of their submitting to the experiment; and at length, some of the royal family. But the practice was not thoroughly established, nor properly appreciated, by the English public, until the middle of the century.

Its efficacy in mitigating the severity and danger of the disease, in saving life and preventing deformity, was signally great. The mortality in the natural small-pox was estimated at one in five. It is really higher. Mr. Marson infers, from the records of the Small-pox Hospital (where, however, the mortality is likely to be above the average), that the natural small-pox destroys about one-third of all whom it attacks.* But Baron Dimsdale, a great inoculator, declared that not one in fifteen hundred died of the engrafted disease. Two brothers, named Sutton, who had introduced, or rather revived, a very improved method of treating the disorder, professed to have inoculated 20,000 persons, without fairly losing one. But these, doubtless, were vastly exaggerated statements. Dr. Gregory says, 'the average number of deaths at the Inoculation Hospital was only three in a thousand.' The National Vaccine Board speaks decidedly of 'one in three hundred' as the proportion of the inoculated that 'will surely die' from the operation.

In the inoculated disease the period of incubation is comparatively short; the pustules are seldom numerous, and still more seldom confluent; and the secondary fever is generally slight or awanting.

I may repeat, here, also, that the eruption is not unfrequently preceded by a rash, something like that of scarlet fever, and called by Willan the *roseola variolosa.* It fades in the course of a day or two, and then the small-pox pustules are seen emerging just in the same state that they would have been in, at the same period, if no such rash had appeared. The efflorescence happens oftener in the inoculated than in the casual disease. In the former it is looked upon as rather a favourable sign; in the latter, especially if the rash be of a dark red colour, it is considered unfavourable, and as the herald of a severe confluent disorder.

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* 'Natural small-pox is a most fatal disease at all periods of life: the most so in infancy and advanced life; the least so from 10 to 15 years of age; under 5 years it is 50 per cent.; still greater, however, under 2 years; the mortality after the age of 20 rises suddenly, and increases gradually; at 30 it exceeds the mortality of infancy, and after 60 hardly any escape.'—Marson, in *Med.-Chir. Trans.*

A far superior expedient has since been discovered, in the practice of *vaccination*, which has rendered the inoculation of small-pox not merely unnecessary, but, in most cases, perfectly unjustifiable. Yet circumstances do sometimes arise, even now, in which it may be allowable and right to engraft the matter of small-pox; as when an unprotected person is unavoidably exposed, or has recently been exposed, to the contagion of that disease, and there is no vaccine matter at hand. The advantage of inoculating in such a case is, that the inoculated or milder form gets the start of the natural and severer; the fever commencing sooner than it would otherwise do. To show you the value of the practice in such cases, and the degree of protection which it affords to individuals whom we cannot vaccinate, I may mention a fact which Professor Gregory, of Edinburgh, was in the habit of relating, and which was told him by a naval surgeon. The small-pox was introduced among the crew of a man-of-war, in a tropical climate, where no vaccine matter was to be procured. The men were almost all unprotected. Sixteen of them took the disease in the natural way; and of these, nine, or more than one-half, died. Of 363 who were inoculated, under the disadvantages of a hot climate, and no preparation, not one perished.

That a disorder communicated to the human animal from one of the brutes should protect the former against the contagion of small-pox, is one of the most interesting facts in the whole history of medicine. How glimpses of a truth so remarkable were first revealed to the casual observation of certain peasants, and how the result of this chance observation was gradually 'matured into a rational and scientific form by a mind deeply imbued with the best principles of sound philosophy,' I have not leisure to tell you in detail. And it is the less necessary that I should do so, as you may find the whole subject thoroughly narrated and discussed by Dr. Baron, in his interesting biography of Edward Jenner.

Dr. Jenner found among the great dairy farms in Gloucestershire a popular belief that no person who had had the *cow-pox* (an eruptive vesicular complaint communicated from the udder of the cow to the hands of the milkers) could 'take the small-pox.' Satisfied, by inoculating with small-pox matter several individuals who had had the vaccine eruption, that this was not an unfounded notion, he at length conceived the great and happy idea of propagating the cow-pox from one human being to another, and so preventing, in all cases, the perilous and disfiguring distemper of small-pox, which he hoped might thus be finally expelled from the earth.

By degrees, Dr. Jenner ascertained that some persons, who had had sore hands from milking, were not thereby rendered proof against the contagion of small-pox; but this difficulty was soon cleared up by the discovery that the teats of cows were liable to different kinds of eruption, and he learned, by close observation, which of these was the peculiar eruption that produced in the human frame the protecting disorder.

Dr. Jenner set himself to trace, if possible, the origin of the disease of the cow. First, he found that it was peculiar to certain dairies; then, that in those dairies *men* were employed in milking. Following up this clue, he further made out that those men had also the charge of the farm-horses. Next, he learned that the teats of the cows generally began to exhibit the specific eruption at that time of the year when a complaint called 'the grease' chiefly prevailed among the horses. Hence he concluded, that the malady was conveyed to the cows by the hands of the men who had been dressing the heels of horses affected with the grease. Subsequent inquiries have, however, shown that this conclusion was not strictly correct.

Another difficulty which lay in Dr. Jenner's way, and which his patience and sagacity surmounted, was this. He found that some who were casually infected from the true complaint in the cow were not protected. This depended, as he afterwards ascertained, upon the period of the disease in the cow, at which the virus was communicated to the milker. The thick matter proceeding from the vesicle late in its progress produced indeed a severer local sore than the thinner matter of its earlier state, but it did not confer the desired protection. The same thing is observed in respect of small-pox. If the matter used for inoculation be taken from a fully matured pustule, it does not so surely excite the disease as when taken from a more crude one.

The next important step in this most interesting investigation was to determine whether the vaccine disease could be transmitted, by engrafting, from one human being to another, and whether, if so transmitted, it retained its protecting power. The 14th of May, 1796, was the birthday of vaccination. 'On that day, matter was taken from the hand of Sarah Nelmes, who had been infected by her master's cows, and inserted by two superficial incisions into the arms of James Phipps, a healthy boy of about eight years old. He went through the disease apparently in a regular and satisfactory manner; but the most agitating part of the trial still remained to be performed. It was needful to ascertain whether he was secure from the contagion of small-pox. This point, so full of anxiety to Dr. Jenner, was fairly put to issue on the 1st of the following July. Variolous matter, immediately taken from a pustule, was carefully inserted by several incisions, but no disease followed.'

It is scarcely necessary for me to notice the objections which were made to the practice of vaccination. Some of them were merely foolish—as, that it was unnatural and impious to engraft the disease of a brute upon a Christian. Others were untrue—as, that it introduced into the system new, unheard of, and monstrous disorders, distinct from the cow-pox itself. It triumphed over all these cavils: and in six years from its first promulgation the discovery was known in every region of the world.

It was soon found, however, that some, who had apparently had the cow-pox by inoculation, were nevertheless not incapable of taking the small-pox; and that these failures were, many of them at least, attributable to the mistakes that were made in the time or manner of performing the operation. It became necessary, therefore, to ascertain precisely the conditions requsite for the production of the genuine disease. And these conditions have been successfully investigated by Dr. Jenner and by subsequent observers.

You will learn to recognise the true vaccine vesicle only by repeatedly examining it for yourselves. Yet a brief description of its characters and progressive changes may be useful to you.

On the second or third day after the insertion of the vaccine matter into the arm, the puncture looks red and inflamed; and on the fourth or fifth day the vesicle becomes perceptible; a pearl-coloured elevation of the cuticle enclosing a minute quantity of a thin transparent liquid. It gradually increases in magnitude till the eighth day, when it should measure from a quarter to half an inch across. Like the pustule of small-pox, it is more prominent at its circumference than at its centre, and it consists of small cells, from ten to fourteen in number. By puncturing carefully one of these cells, a drop of the virus may be let out, the other cells remaining full. Up to the seventh, or eighth, or even to the beginning of the ninth day, the inflammation around the vesicle should extend to only a very small distance from it. After this, it spreads, and what is called the *areola* is formed; a circular red border, which continues to increase during the ninth and tenth days, and begins to fade on the eleventh, passing through shades of blue as it declines, and leaving a degree of hardness behind for two or three days more. By this time, a brown or mahogany-coloured crust has formed over the vesicle, of a nearly circular shape; this becomes gradually harder and darker, and finally detaches itself about the twentieth day. The cicatrix which it leaves should be distinct, somewhat less than half an inch broad, circular, slightly depressed, marked (sometimes) by radiating lines, with a well-defined edge, and dotted with little pits which seem to correspond to the cells of the vesicle.

About the eighth day there is usually some slight febrile excitement manifested, which soon subsides. This is analogous to the secondary fever of small-pox: and it appears to furnish the condition of the desired protection.

Of course it is of much moment to determine whether the cow-pox has run its proper course or not; and it is not always easy to say how far the progress of the vesicle may deviate from that which has just been described, without failing of its protecting influence. A very ingenious *test* of this, free from all ambiguity, has been devised by Mr. Bryce. His plan is this. He vaccinates the other arm, or some other part of the body, four or five days after the first vaccination. If the constitution have been properly affected by the first operation, the inflammation of the second vesicle will proceed so much more rapidly than usual, that it will be at its height, and will decline and disappear, as early as that of the first: only the vesicle and its areola will be smaller. In fact, from the time of the formation of the areola, the second vesicle is an exact miniature of the first. If the system have not been duly influenced by the first vesicle, the second will run its own course, increasing up to its eighth day, and so on. Should this be the case, the second vesicle should be tested by a third.

We find the germ of this criterion in the early history of vaccination. Dr. Jenner vaccinated the children of his friend Mr. Hicks, the first *gentleman* who consented to adopt the practice. This Mr. Hicks became afterwards an expert vaccinator himself, and it was his custom, in a doubtful case, to perform a second vaccination a few days after the first: and he remarked that the second vesicle made 'immense strides to overtake the first.'

After some time it became apparent that Dr. Jenner's estimate of the protecting power of the vaccine disease had been set too high. He had hoped and believed, as others also had, that the cow-pox would in all cases prove a perfect and permanent protection against the small-pox; but those hopes have been disappointed. Doubtless complete protection is the rule; but —how thoroughly and regularly soever the vaccine malady may have proceeded—it is most certain that very many exceptions to this rule have taken place, and are daily taking place around us.

And this fact, which has long been too glaring to be denied or explained away, has depreciated the value of the process of vaccination, in the public esteem, far more than, if rightly considered, it should have done. For it is a remarkable and most important truth that the disease which, in some duly vaccinated persons, follows exposure to the contagion of small-pox, is much milder and shorter even than the inoculated, and *à fortiori* than the natural small-pox. The disorder thus occurring is, therefore, denominated the *varioloid* disease, or (more conveniently, in my opinion) the *modified* small-pox, or *post-vaccinal* small-pox.

The constitutional symptoms of this modified disease are, in general, at the outset, and for several days, much the same with those of the regular small-pox. The eruptive fever is of equal length and intensity. There is frequently much headache, and sickness, and sometimes even delirium. The eruption begins about the third day: it is often copious, and sometimes confluent; and in the confluent cases the eruptive fever does not entirely subside so soon as the crop of pimples has come out.

It is in its subsequent progress that the complaint is modified: in respect both of the appearances presented by the skin, and of the constitutional symptoms.

Three distinct kinds of eruption have been observed—

1. The eruption sometimes approaches in its character and course very nearly to that of the ordinary small-pox. The pustules fill up, have the central depression, and ultimately crust over, and the face swells. But this course is performed in a shorter time than that of the ordinary disease, and the pustules are usually smaller. This is the severest and the least common form of the modified small-pox.

2. Sometimes the papulæ show a little fluid on their tops only, but never fairly suppurate, nor break; but the vesicles dry up, and hard prominences remain, with livid bases and horny summits. When the eruption is thus vesicular in its commencement, the disorder may at first be mistaken for varicella.

3. There are other cases in which a great part of the eruption consists of red pimples, which soon become livid, but contain from first to last no fluid whatever.

In the majority of instances of modified small-pox, all these forms of eruption co-exist. Some of the papulæ go on to suppuration, others become crowned with a horny summit, and others never exhibit any fluid at all.

But the most important characteristic of the modified disease, is the total absence of secondary fever. The constitutional disturbance which, for the first week, may have been as severe as in the ordinary small-pox, generally subsides entirely when the eruption has reached its acme. The patient is convalescent just when, in the unchecked and regular form of the malady, his danger is beginning to be most urgent.

These two circumstances, then—the short duration of the eruption, and especially the absence of secondary fever—furnish the broad distinctions between the regular and the modified small-pox: and almost always, when vaccination has been thoroughly effected, and small-pox occurs afterwards, it occurs in this modified form; and the modified form of small-pox is seldom fatal, though instances of death resulting from it now and then happen.

I have said nothing of the thermometry of small-pox. It scarcely helps us. What we find actually to happen is what our knowledge of the course of the disorder might enable us to predict.

In the natural and in the post-vaccinal form of the disease the temperature is alike at the outset; rising on the first or second day, sometimes at once, sometimes more gradually, to 104° or more. At the period of the eruption it begins to fall again, and in the post-vaccinal variety the defervescence from the third to the sixth day is complete and permanent.

In the natural small-pox the temperature does not quite return to the normal standard; and at the period of suppuration it rises again to 102° or 104°, with morning remissions. In fatal cases it may remain at this elevation; or become excessively high, 107° perhaps.

In relation to the modified or post-vaccinal disease, several questions of the highest practical moment and interest have arisen, which by slow degrees, and under careful and multiplied observation, may now be said to have found their solution.

The first is, whether the protecting influence of cow-pox upon the human frame diminishes by lapse of time, and at length wears out. There is ample evidence to show that, sometimes at least, it does. Certainly in many, but not in all, of those who have gone through the vaccine disease, vaccination repeated at a distant period reproduces, in a greater or less degree, its primary effects. A friend of mine, who was vaccinated in 1799, has a son who was vaccinated at the age of three weeks. Years afterwards both of them were revaccinated. The boy was somewhat affected by the renewal of the operation; the father not at all.

It may well be doubted whether *all* those who are susceptible of some impression from a second vaccination would become infected with small-pox under ordinary exposure to its contagion. That many of them would so contract the disease, and that all of them would be *endangered* by such exposure, is too certain. And a second question immediately presents itself; namely, whether this repetition of the operation of engrafting the cow-pox renews, or adds to, their security against small-pox. Happily, this question may also be answered in the affirmative; and answered by statistics of the amplest comprehension. In his able and most conclusive digest of the whole subject, published by the General Board of Health, Mr. Simon shows that during the five years 1833–7, though small-pox infection had been sixteen times imported into different regiments of the army of Wirtemberg, there had ensued among the 14,384 revaccinated soldiers, one single instance only of modified small-pox. Still more satisfactory experience is that of the Prussian army. 'In Prussia (as in Wirtemberg) the practice of revaccination grew out of the knowledge that small-pox would attack a certain proportion of those who had been vaccinated only in infancy. During the ten years preceding 1831, cases of post-vaccinal small-pox were increasing in number and fatality, and within the three years 1831–33 there had occurred no fewer than 312 *deaths* by small-pox. For the last twenty years the Prussian army has represented an almost entirely revaccinated population. And what has been the contrast? 104 annual deaths by small-pox was the last experience of the former system; two annual deaths by small-pox has been the average for the revaccinated army. Analysing moreover the forty fatal cases of small-pox which during the last twenty years have occurred in the Prussian army, we find that only four of the number were of persons who (it is said) had been successfully revaccinated.'

Take one authentic and crucial instance from among ourselves. Mr. Marson has been the resident surgeon to the London Small-Pox and Vaccination Hospital for the last thirty-four years. He has always made it an imperative rule that every nurse and other servant of the hospital should, on entering the service, be vaccinated. In their case it is generally *re*vaccination; and it is never afterwards repeated. These nurses live in the closest daily and nightly attendance upon small-pox patients; and the other servants are constantly exposed to the profuse contagion: yet in no single instance, during these thirty-four years, has any one of these servants and nurses been affected with small-pox. Surely no stronger proof than this can be imagined, that revaccination in the adult is an absolute protection against small-pox, and need not be repeated.

But, thirdly, is there any ground for supposing that the wished-for protection ever fails to be conferred, because the operation is performed too early? None whatever that I know of. In fact there is unquestionable evidence that, for the full attainment of its defensive purpose, *gratuitous* vaccination at least is, in this country, performed too late. It appears from official tables published under the authority of the Registrar-General, that no less than one-fourth—i.e. twenty-five per cent.—of the whole mortality from small-pox in England and Wales happens in infants less than one year old; and as much as eleven per cent. within the age of four months. Within the fifth year the proportion reaches the enormous amount of from seventy-five to eighty per cent. These facts proclaim the necessity of *early* vaccination. It should be as early as is consistent with the safety of the child. Certainly it should never be delayed, except under special circumstances of excuse, beyond the third, or at most the fourth month after birth. Dr. Arthur Farre informs me that he has seen two cases of death from vaccination performed too early, namely, at the age of eight days. In the young infant, when perhaps the navel has not yet cicatrised, vaccination may produce a frightful sore, and erysipelas. He advises three in preference to two months of age, on account of the greater comparative strength at the greater age. But if the contagion of small-pox happen to be present, the infant should be vaccinated immediately after birth.

A fourth question is how far the frequent failure, in late years, of complete protection, can be ascribed to the circumstance that the vaccine virus has been repeatedly transmitted from one human being to another, and its supply thus kept up, without any fresh recurrence to the cow, the original source of the disorder. Dr. Jenner was, himself, not without apprehension that this might prove a cause of failure. For one year I had a seat, as the Senior Censor of the College of Physicians, at the National Vaccine Board, and I then had opportunities of satisfying myself that lymph which had been transmitted without interruption from person to person ever since the time of Jenner, continued to generate what seemed a very perfect cow-pox vesicle. And it is the expressed opinion of the permanent members of that Board, 'that the vaccine lymph does not lose any of its prophylactic power by a continued transit through successive subjects.' Mr. Simon has, however, stated some strong grounds for suspecting that the 'occasional impermanence of protection may depend upon impairment in the specific power of vaccine contagion—an impairment arising in the transmission of that contagion through many generations of men.' It was alleged by M. Brisset, in France, as early as 1818, that the past ten years had made a marked difference in the visible characters of the vaccine vesicle: that it had become necessary to establish, instead of Jenner's two vesicles, eight or ten points of infection. Dr. Meyer, of Kreutzburg, states that on examining in 1824–25 nearly four thousand vaccinated persons of all ages, he found the older scars much better marked than the recent ones; that, according to the testimony of many vaccinators, the proportion of unsuccessful to successful vaccinations was every year increasing; and that the cicatrices resulting from his own use of lymph recently obtained from the cow, were again after the old normal type. Dr. Gregory and Mr. Estlin, in this country, have adduced similar facts in evidence 'that the vaccine lymph, by passing through the bodies of many persons, loses, in process of time, some essential part of its activity.'

This suspicion gathers force from a very curious result of the experience furnished by the Prussian army. It appears that where the vaccine supply has seldom or never been renewed from the cow, the proportionate resusceptibility of vaccine disease at a given age (and therefore it may fairly be presumed the susceptibility of small-pox also) has undergone a progressive increase: just as post-vaccinal small-pox has undergone a successive increase. 'And (argues Mr. Simon) it is difficult to conceive how the infantine generations of a country could, crop by crop, successively derive less permanent constitutional impressions from vaccination, unless the efficient cause of those impressions—the vaccine contagion itself—had year by year undergone enfeeblement of its powers.'

On this point, as well as on others, the statistical experience of the Prussian army is immense. The revaccination of recruits 'extends annually to some forty or forty-five thousand operations. It is reported upon annually. Its records run back twenty-four years. The subjects are of like age, in like proportions, and under like circumstances. When this system of revaccination commenced in 1833, the proportion of successful results (of those, that is, who again *took* the disorder) was thirty-three in every hundred. Now the annual percentages of successful results, for the whole time during which revaccination has been practised in that army, run thus:—33, 39, 42, 46, 49, 50, 51, 54, 57, 58, 57, 57, 58, 60, 64, 64, 64, 61, 64, 69, 69, 69, 69, 70. *The last proportion of success exceeds the double of that with which the series commenced.*' Supposing the first vaccination to have preceded the second by twenty years, '*the vaccinations of* 1836—tested by eventual resusceptibility to cow-pox—*were not half so stable as the vaccinations of* 1813.'

Mr. Marson's testimony is in unison with this. He says, that in the course of years vaccine lymph, by passing many times through the human body, becomes *humanised*; that the cicatrices, according to his own experience, are not so good as they were formerly; and that the mortality after vaccination, estimated on a large scale, shows a considerable increase; namely, from 6·56 per cent. for twenty years, from 1836 to 1855, up to 9·2 per cent. out of 1,958 cases for the years 1863 and 1864.

In the fifth place, there are yet moot points, respecting the number of vesicles, and the degree of constitutional disturbance, which are requisite to insure, and to prolong, the protective power of vaccination. The constitutional effect will bear some proportion to the number of vesicles; and of these, it would seem, there should be several; and one or two of them, at least, should be suffered to pursue their entire course untouched. Mr. Marson believes that 'vaccination may be relied on, when four or more vesicles have formed which have left good dotted cicatrices.' He deduces, from tabular statistics of his own, that among persons taking post-vaccinal small-pox; of those who can show four or more vaccine cicatrices, one in every 200 will die; of those having one indifferent cicatrix, twenty-four; of those having no perceptible cicatrix at all, forty-seven. These numbers are most suggestive as to the propriety of revaccination.

The same gentleman, who tells us that he has vaccinated between forty and fifty thousand persons, asserts that 'one of the principal causes of failure in vaccinating, and of subsequent insecurity of the individual, even when the vaccination does take effect,' is the want of care in the selection of vaccine lymph. 'Lymph for use is in its best state on the seventh day of the progress of the vesicle it is taken from—the day week from the vaccination. It should be taken when the vesicles are plump, and just before the formation of the areola. Under no circumstances should it be taken for use later than twenty-four hours after the areola has begun to form.' If this rule were invariably observed, there would be, Mr. Marson believes, very few cases of severe small-pox after vaccination. 'A serious error in vaccinating is the use of blunt lancets. It is impossible to have a lancet too sharp for vaccinating.' 'The lymph should be introduced by a puncture of a valvular shape, from above downwards, so managed that the lymph at each puncture may gravitate into the wound. In this way, the lymph may be introduced in five punctures—the number I recommend—from half to three-fourths of an inch apart, without recharging the lancet: care being taken that the punctures are not bruised.' 'With good lymph, and the observance of all proper precautions, an expert vaccinator should not fail of success, in his attempts to vaccinate, above once in one hundred and fifty times: yet a large number of those who take upon themselves the duty, think they do very well if they succeed, however imperfectly, five times out of six.'

Mr. Marson has considered and settled a very important question, which sometimes presses for immediate solution. Suppose an unprotected person has been exposed to the contagion of small-pox: may he escape that disease, or obtain comparative safety, by subsequent vaccination? and if so, within what limits of time?

Mr. Marson works out the abstract problem, and declares the result to be confirmed by his personal experience.

After the reception of the poison of small-pox, an interval of twelve days elapses before the manifest outbreak of the disease.

The areola in vaccination is not complete till the ninth or tenth day of the vesicle. If this areola be fully formed before the onset of small-pox, the patient is safe.

He illustrates this rule by an example. 'Suppose an unvaccinated person to inhale the germ of variola on a Monday. If he be vaccinated as late as on the following Wednesday, the vaccination will be in time to prevent small-pox from being developed. If it be put off till Thursday, the small-pox will appear, but will be modified. If the vaccination be delayed till Friday, it will be of no use.

Should the person have been formerly vaccinated, *re*vaccination will be effectual two days later than this; because in revaccinated persons the stage of areola is reached two or three days sooner than in persons vaccinated for the first time.

The limit of safety, you see, is narrow; yet wide enough for the rescue sought, when fitting lymph is at hand.

The following case, related to me by Dr. Johnson, shows that vaccination *may* curb the small-pox, even when it is performed at a later period after the reception of the variolous poison:—

A boy, in a house where a fatal case of small-pox had occurred, was vaccinated, for the first time. About the fourth day after the operation, when the vesicles were beginning to rise, he sickened with small-pox. The initiatory symptoms were severe, and an abundant eruption appeared. About the fourth day, however, the variolous eruption rapidly subsided, and the child was convalescent. Meanwhile the vaccine vesicles pursued their regular course. The vaccination was just in time to arrest the progress of the small-pox.

With regard to a sixth point, the most important of all, we may speak very decidedly; and it is a point concerning which it is of the utmost consequence that medical men should form, and disseminate among the public, correct opinions: I allude to the comparative merits and advantages of *inoculation with small-pox*, and *vaccination*.

The advantages of the practice of inoculation to the individual, supposing him doomed to have small-pox, were great and obvious; to the community at large they were very doubtful. It gave the undoomed individual, for certain, an ugly disease, which was comparatively free from danger, in exchange for the chances, on the one hand, of contracting a very hazardous form, and on the other, of escaping altogether from any form, of variola. We need not inquire which is the most eligible branch of this alternative; we know which was by most men actually chosen. But the practice of inoculation, by carrying the virus and the disease into every village throughout the length and breadth of the land, filled the country with contagion; insured the disease to all who were subjected to the operation, and diminished to all who were not, the chances of escaping it. No doubt the distemper was produced artificially in many more persons than would have caught it naturally, had inoculation never been thought of. So that while the relative mortality, the per-centage of deaths from small-pox, was lessened by this practice, the absolute mortality was fearfully increased. Such at least is the judgment expressed by most who have thought and written on the subject. Dr. Heberden compared the number of deaths ascribed in the London bills of mortality to small-pox during the first thirty years of the last century, with the number during the same period of years at the close of the century, and he found that they had increased from 7·4 per cent. to 9·5 per cent. To be sure, some allowance must be made for the increase in the whole population of London during that interval; but on the other hand we must take into account the deaths (not noted in those bills) which followed the inoculation of small-pox in secluded villages, where but for that practice, the poison might seldom have been found. It is right, I say, that this matter should be steadily contemplated, in all its lights, and with all its shadows, in order that the unspeakable blessing conferred upon mankind by the researches of Dr. Jenner may be fairly set forth, and adequately appreciated. The vaccine virus produces a slight disorder, which is attended with no risk, and which (unluckily I may say) is not communicable except by direct engrafting. It not only does not disseminate a dangerous and deadly poison, but if rightly used, it affords the means of eradicating from a well-regulated community, or at least of confining within narrower limits, the most loathsome pestilence which the world has known. Where vaccination is, the contagion of small-pox need never come. In Denmark, as I told you, variola had at one time disappeared before the defensive influence of compelled vaccination. Chance, and a careless security, engendered by the absence of the pest, have led to its reintroduction there. It is much to be regretted that the vaunted liberty of this country has hitherto rendered it almost impossible to enforce by law a practice which would be so conducive to the public weal. Some good might be done by enacting that no person should be eligible to even any parochial office of trust, honour, or profit who could not produce a certificate that he had been duly vaccinated. Compulsion in some form is clearly defensible and just; and compulsion by *fine*, or by disqualification, is perhaps the least objectionable form. It is well remarked in an instructive report on this subject, by a Committee of the *Epidemiological Society*, that 'though it may be doubtful how far, in this free country, it is justifiable to compel a person to take care of his own life, or of that of his offspring, it can scarcely be disputed that no one has a right to put in jeopardy the lives of his fellow-subjects. The principle of so using one's own as not to injure another's, is one which has always been acted upon in our legislation as regards property and personal nuisances; and it is but an extension of the principle to apply it to questions of life and health.' Statistical returns have shown that the proportionate mortality from small-pox in England and Wales, is considerably more than double what it is in any of those Continental States in which vaccination is more or less stringently enforced.

The benefits which this safeguard, vaccination, confers on the individual are scarcely inferior to those which it is calculated to bestow upon society. It unfortunately does not give complete protection against small-pox to all, but it gives complete protection to many. And you must recollect that small-pox itself is not a universal and absolute assurance against its own return. But the cow-pox relieves all from the necessity, imposed by inoculation, of coming within the sphere of the variolous contagion. It renders many, I repeat, impregnable to that poison, if they do chance to be within its range; and its advantage to the comparative few who suffer the double misfortune of being exposed to the contagion of small-pox, and of being affected by it, is this, that it gives safety, though not exemption; that it takes away the sting and peril of the variolous disease, by curtailing it of the secondary fever. At the very worst, it leaves the individual liable, by a twofold ill luck, to contract a form of small-pox not more dangerous than that which he would voluntarily accept by submitting to the operation of inoculation.

It is not difficult to adduce authentic evidence in illustration of this reasoning; indeed I have already put before you incidentally much and striking evidence of that kind.

The following tables were compiled by the committee just now mentioned, to show the gradual diminution in the mortality from small-pox in London, as compared with the mortality from all causes, since vaccination has been introduced; notwithstanding its hitherto imperfect employment.

Table showing the average of deaths from small-pox out of every 1,000 *deaths from all causes within the bills of mortality during the last half of the last century—the half century preceding vaccination.*

For the 10 years ending	1760	......	100
,, ,,	1770	......	108
,, ,,	1780	......	98
,, ,,	1790	......	87
,, ,,	1800	......	88

Table showing the same during the first half of the present century—the half century succeeding the introduction of vaccination.

For the 10 years ending	1810	......	64
,, ,,	1820	......	42
,, ,,	1830	......	32
,, ,,	1840	......	23
,, ,,	1850	......	16

From the same source I take the following significant statement respecting the prevalence of small-pox *as an epidemic* in London.

The frequency of epidemics in London has been :—

Before protection as 42 ;
During inoculation as 54 ;
During vaccination as 14.

It is shown in Mr. Simon's Blue-book that the fatality of small-pox in Copenhagen is but an eleventh part of what it was before the introduction of vaccination ; in Sweden, little over a thirteenth ; in Berlin and in large parts of Austria, but a twentieth ; in Westphalia, but a twenty-fifth.

An able paper has been published on this subject by Dr. Stark, in the 64th volume of the 'Edinburgh Medical and Surgical Journal.'

He brings forward statements which confirm the fact, already mentioned, that small-pox itself is by no means a sure protection against a recurrence of the same disease.

Small-pox after small-pox is supposed to be much less common than small-pox after cow-pox. But in estimating this proportion we must bear in mind the comparative rarity, now-a-days, of primary small-pox. If, indeed, we could trust to the test of revaccination, we might conclude that small-pox is not a more effectual safeguard against small-pox, than the cow-pox is found to be. In the Hanoverian army, in the years 1837, 8, and 9, revaccination produced the true cow-pox vesicle in 11 persons out of every 100, was partially efficient in 27, and failed altogether in 62. A number of the men who had gone through small-pox, were also subjected to the general vaccination, and, curiously enough, its various effects upon them were in precisely the same proportions, as among those who had previously been vaccinated.

So Heim gives the following comparative view of the results of revaccination in the army of Wirtemberg.

Among 100 men who were vaccinated after having had small-pox, the operation succeeded in 32, produced a modified effect in 26, and no effect at all in 42.

And among 100 men who were vaccinated for the second time, the operation succeeded in 34, produced a modified effect in 25, and no effect at all in 41.

But the most striking part of Dr. Stark's paper is that in which he collects and exhibits evidence, which seems perfectly conclusive, of the immunity conferred, for the most part, by vaccination, from the subsequent occurrence of fatal small-pox.

In a general population like our own, all observation on this point is vitiated and made valueless by the uncertainty that exists respecting the ratio of the vaccinated to the unvaccinated portion of the community. It is different in our armies. Every recruit is closely examined: and if he have not previously undergone cow-pox, or small-pox, he is forthwith vaccinated. Mark now some ascertained facts relating to men thus cared for, of nearly the same age, living in the same place, surrounded by similar external circumstances, and subjected to constant and vigilant supervision.

From the Government 'Statistical reports of the sickness, mortality, and invaliding among Her Majesty's troops' for 20 years, viz., from 1817 to 1836 inclusively, we learn that

In Dragoon Regiments and Guards, with an aggregate strength during that period of 44,611 men, and a total mortality of 627, there were but three deaths from small-pox.

Among the troops at Gibraltar, one death only from small-pox occurred, the aggregate strength being 60,269, and the whole mortality 1,291.

In the West Indies, although several epidemics of small-pox had ravaged the islands within that period, not one person died of the disease among the British or white troops, with an aggregate strength of 86,661 and a total mortality of 6,803: while among the black troops on the same station, with an aggregate strength of 40,934, and a mortality of 1,645, there was not even one *case* of small-pox.

'At Bermuda, Nova Scotia, New Brunswick, Cape of Good Hope, and the Mauritius, not a single death from small-pox occurred during those 20 years; and even the white troops of Western Africa wholly escaped this disease, which was carrying off hundreds of the black unprotected population.'

In Malta, from 1818 to 1836 inclusively (a period of 19 years), the aggregate strength of the British troops was 40,826, the total mortality 665, and the mortality from small-pox 2. Yet in the years 1830 and 1831, small-pox raged there as an epidemic, and destroyed 1,169 persons: for in 1830, there died of small-pox 1,048, the total mortality being 3,407; and in 1831 there were 121 deaths from small-pox, out of an aggregate mortality of 2,583.

Again: in Ceylon three epidemics of small-pox occurred during the 20 years included in the Government reports; namely,
in 1819, when of the natives 7,874 took the disease, and 2,945 died.
" 1830, " " 806 " " 169 "
" 1834, " " 425 " " 94 "

Yet in the same island, during the very same period, there were among the white troops, with a total mortality of 3,000, 4 deaths from small-pox (out of 8 cases); among the Malay troops, with a total mortality of 858, 9 deaths from small-pox; among the pioneer corps, with a total mortality of 647, 1 death from small-pox. And in the last of these epidemics, Dr. Kinnis states that not one instance of the disease appeared among the white or the native troops.

Facts equally conclusive are to be found in the statistics of our armies in India. But I need not go into further detail.

Surely we may perceive—in authenticated statements like these—how inadequately the great preventive remedy of small-pox has hitherto been for the most part applied in this country; how successful might become its thorough application. Surely now that the maxim *salus populi suprema lex* is beginning to be acknowledged by our legislators, any general sanitary enactment must be held defective which does not provide for and compel effectual vaccination, and punish variolous inoculation, and shut up in strict quarantine every case of small-pox as soon as its existence in the community is discovered. By these three provisions—and probably by nothing less than these—the seeds of that dire distemper would gradually become scarce and finally disappear; and the soil upon which they might still chance to light would be made unfruitful of the deadly harvest.

There yet remains a highly interesting, but a less practical question. Dr. Jenner, as I stated before, believed that he had traced the cow-pox to its origin in the heels of the horse affected with *the grease.* It has since been made out that the disease which, in the horse, corresponds with and produces the specific malady of the cow, is a vesicular eruption, having no necessary connection with the grease, but extending sometimes all over the animal's body. Now the question is, whether these two distempers, occurring in the cow and in the horse, are identical in their essence and nature with the small-pox of man. If so (as Dr. Jenner believed, and Dr. Baron strongly maintained), a part of the mystery attending the whole subject vanishes. The protection furnished by the cow-pox resolves itself into the more familiar law, that certain diseases engendered by animal poisons, happen to the same individual but once, and shield the body against their own recurrence. In conformity with this theory, Dr. Baron named the disorders respectively, variolæ, variolæ vaccinæ, and variolæ equinæ.

The notion, you see, was this, that the vaccine disease is in truth small-pox, rendered mild by passing through the system of the cow. The great object of inoculating the small-pox is to produce a benignant form of that disease, by diminishing the number of pustules. The cow-pox diminishes the number to one; and while it reduces the severity of the disorder to a *minimum,* it absolutely takes away its power of propagating itself, except by a direct engrafting of the visible virus. The disease is not sufficiently intense to taint the air with poisonous effluvia. At the same time it affords (*perhaps* somewhat less surely and less permanently) the customary protection. Such was Jenner's theory, which was intelligible and plausible, and supported by strong facts and persuasive reasoning; for all which I may again refer you to Dr. Baron' book.

This plausible and attractive theory has become demonstrated fact. Researches subsequent to Jenner's have made it 'matter of almost familiar experiment' (I state the whole proposition in Mr. Simon's condensed but clear language) 'that the infection of small-pox may, by inoculation, be communicated from man to the cow; that its result is an eruption of vesicles presenting the physical characters of cow-pox; that the lymph from these vesicles, if implanted in the skin of the human subject, produces the ordinary local phenomena of vaccination; that the person so vaccinated diffuses no atmospheric infection; that the lymph generated by him may be transferred, with reproductive powers, to other unprotected persons; and that, on the conclusion of this artificial disorder, neither renewed vaccination, nor inoculation with small-pox, nor the closest contact and cohabitation with small-pox patients, will occasion him to betray any remnant of susceptibility to infection.'

To Dr. Gassner of Günzburg, to Dr. Thiele of Kasan, to Mr. Ceely of Aylesbury, and to Mr. Badcock of Brighton, belong the praise of having worked out, by careful and repeated experiments, this most important truth.

If, as Dr. Heim asserts, there are no less than five kinds of spurious cow-pox, all communicable by inoculation from the teats of the animal to the human body, it follows that, in having recourse, from time to time, to lymph recently obtained from the cow, it should be such lymph only as results from inoculation of the cow with small-pox.

The majority of those who are attacked with post-vaccinal small-pox are between 18 and 25 years old. It would seem that in some way or other the advent of puberty disturbs or lessens the protective influence of the previous vaccination. I advise you to adopt and to recommend the thorough vaccination of infants at the age of three months, and the careful revaccination of the same persons immediately after the period of puberty. Dr. Seaton gives the judicious counsel that in all revaccinations care should be taken to ascertain that some local effect has been produced by the insertion of the vaccine lymph.

A very grave question in respect of vaccination is even now agitated; namely, whether it can communicate any other disease besides, and in addition to, the vaccine disease; especially whether syphilis may be so imparted.

I have no warrant for pronouncing a dogmatic opinion on this painful subject. My strong belief is that no other than the vaccine disorder can possibly be conveyed by inoculation with the pure lymph of the vaccine vesicle: but that if the *blood* of a syphilitic child mingles and is inserted with the lymph, syphilitic disease may be thus engrafted. If this be true doctrine, I need not point out the practical lesson which it involves.

To avoid breaking the thread which connects the different parts of the main subject, I have postponed to the last what I have to say respecting the *treatment* of small-pox.

This, for a long time, was conducted upon an erroneous principle, and was eminently disastrous. The older physicians attempted to force out, through the skin, the morbid matter existing in the blood. The eruption they rightly considered to be the natural and only cure: and adopting the vulgar maxim, that 'it was better out than in,' they did all they could to promote a copious eruption, by a hot regimen, by covering the patient with bed-clothes, by keeping the doors and windows jealously closed, and excluding every breath of air, and sometimes by administering wine and cordials. He was thus compelled to rebreathe an atmosphere loaded with the poisonous emanations from his own skin. The celebrated John of Gaddesden, the author of that curious book the 'Rosa Anglica,' improved even upon this. He surrounded the half-suffocated patient with red curtains, red walls, red furniture of all kinds; everything he saw was to be red; for in that colour there was, John pretended, a peculiar virtue. This John of Gaddesden, by the way, was a very sad knave, and the first Englishman, I believe, who had the luck to be made Court physician. He had one medicine so good as to be fit for the rich only; and he recommended a double dose for the wealthy. 'Duplum sit, si pro divite.' He flourished in the fourteenth century.

Sydenham was the first, in this country, to employ the opposite or cool regimen in small-pox; and although his prejudiced contemporaries refused to follow his example and adopt his practice, he confidently predicted its final triumph—'obtinebit demum me vitâ functo.'

But it was subsequently to the introduction of the method of inoculation that the cooling treatment was fairly established, by the Suttons—two brothers, one of whom, Robert, lived at Bury St. Edmunds; the other, Daniel, at Ingatestone, in Essex. These men, wiser in their generation than the regular physicians, had the good sense to pursue the same plan of general management which had been so prosperous in the East, whence the practice of engrafting was originally imported. Daniel, in particular, became famous for his successful inoculations: and the great secret of his success seems to have consisted in his making one puncture only; exposing his patients much and often to a cool atmosphere; supplying them freely with refrigerant drinks; and restricting them to a spare diet. Under this course, Cullen, who adopted it from the Suttons, declares that ninety-nine times in the hundred, inoculation imparts a distinct small-pox, and very generally of the mildest form.

Now the same principle applies to the casual disease when we have reason to suspect that it is impending, or have the opportunity of treating it at its commencement. The object is to prevent, if possible, a copious eruption; upon which, as we have seen, the severity and peril of the disorder entirely depend. It has been thought that venæsection, by its antiphlogistic power, and, perhaps, by letting out, with the blood, some portion of the regenerated virus, might lessen the number of the forthcoming pustules. But you cannot ensure this effect by blood-letting: and you must bear in mind that, should the eruption prove confluent, suppuration, to a large amount, is inevitable, and—like that of an extensive burn—will require, in order to go on favourably, a certain degree of constitutional vigour.

You may abate the force of the eruptive fever, and keep down, it is believed, the number of pustules, by saline purgatives, so given as to produce two or three loose stools every day, and by free ventilation of the surface of the body. The skin may even be sponged with tepid water, if the temperature be very high.

When the eruption is all come out, if the pimples on the face are very few and distinct, the danger is over, and there is no more to be done. At this period Cullen dissuades the further use of purgatives, as being sometimes hurtful.

But if the pimples on the face are many, and confluent, the patient will still require a great deal of attention. Our business is to look out for, and to meet, untoward symptoms.

About the eighth or ninth day, wakefulness, and restlessness, and sometimes tremors, are apt to come on; and the proper remedies for this set of symptoms, in small-pox as well as in continued fevers, are opiates. In variola, when given in full doses at bedtime, their good effects are often very conspicuous the next day. Perhaps, in future, chloral may supersede opium in these cases.

If the maturation of the pustules should proceed tardily, if they should not fill up properly nor their contents become purulent, then strong broths may be of use, or even wine. But the effects of these must be carefully watched, and their amount adjusted to the necessities of the case.

When the pustules are livid, and intermixed with petechiæ, and putrid symptoms occur, the disorder generally proves fatal. In such cases it is customary to prescribe bark and acids, in addition to the wine and opiates.

The proper plan of managing the patient during the continuance of the secondary fever, is to keep his bowels moderately open by gentle laxatives, or by enemata; and to give opiates once or twice a day. These are the more necessary on account of the irritation of the skin. The cooling regimen must now be given up; and the strength must be supported by a nourishing diet. Wine and cordials are indicated if the pulse be feeble; but the swelling of the hands and wrists often makes it difficult to feel the pulse.

Various external applications have been tried, with the view of relieving the intolerable itching; which often induces the patients to scratch and tear their faces, and to insure the formation of scars. Cold cream is used for this purpose; or a solution of common salt, applied luke-warm; or a lotion made by mixing a drachm of the *liquor sodæ chloratæ* with half a pint of water; or one-third of glycerine with two-thirds of rose-water; or a liniment composed of equal parts of olive oil and lime-water. This, which I have often used with good effect, may be smeared, from time to time, over the itching surface, by means of a soft camel's hair brush.

Other methods have been devised, having a more direct aim towards the prevention of that pitting or seaming of the face which is only less dreaded by many patients than the threatened extinction of life itself. The skin may be covered by a film of flexible collodion. Finely-powdered camphor, dusted over the surface, is believed by Mr. George to obviate the disfigurement. Mr. Startin produces a little spot of vesication by touching the apex of each pustule on the exposed surfaces of the body with the *acetum cantharidis*, by the help of a camel's hair pencil. Mr. Higginbottom touches each distinct papula with the solid stick of lunar caustic previously moistened; but when the spots are confluent he washes the whole face, about the third day of the eruption, with a very strong solution of the nitrate of silver, using eight scruples to the ounce of water. This Mr. Marson declares to be much too strong. A similar practice has been adopted, independently it would seem, by Dr. Alexander Howard of Quebec, except that his solution is weaker, an ounce of water containing a drachm only of the salt. This application, which is repeated from time to time, is said to be not only effectual for its primary purpose, but to be grateful also to the feelings, and even conducive to the safety of the patient; to allay the heat, itching, and tension of the face and scalp, and to abate the cutaneous inflammation. It is said also to give no pain, and to leave the features perfectly free from pitting; while it has the further recommendation of requiring no great skill or care in its use. Its management may be entrusted to a nurse.

The dyspnœa which sometimes comes on late in the disease, is a very ugly symptom. It is caused by swelling of the mucous membrane of the larynx. Some relief may be afforded by applying to the larynx, once or twice a day, by means of a bent brush, a solution of nitrate of silver in the proportion of a drachm to an ounce of water. In extreme cases tracheotomy might possibly save life.

LECTURE LXXXI.

Chicken-Pox. Measles. Scarlet Fever.

I MUST not omit a short notice of the disorder called *chicken-pox*; for although a very unimportant complaint, it has given rise to many disputes. Other names which it has borne are *varicella, crystalli, variolæ pusillæ, water-pox.*

Connected with the small-pox, and arising from the same contagion, there are several forms of eruptive disease. I mentioned the chief of them in the last lecture, as varieties of *modified* small-pox. Now these mild and irregular forms of variola, both parents and medical men, wishing, I suppose, to believe nothing in disparagement of the protecting power of vaccination, are very apt to consider, and to call, chicken-pox; and this error having been discovered, some persons have rushed to, or rather revived, the opposite opinion—equally erroneous in my judgment—that there is no such substantial disorder as chicken-pox; but that all the eruptions which have passed under that name have really been forms of modified small-pox. Dr. John Thomson of Edinburgh was one of the stoutest maintainers of this doctrine. No doubt an eruption of short duration, and vesicular through the greater part of its progress, is often caused, especially in persons who have been vaccinated, by the contagion of small-pox; but a similar eruption proceeds also from another distinct contagion, that, namely, of chicken-pox.

The disorder is almost peculiar to infants, and children of tender years. Willan has, however, described one unambiguous example of it, in a gentleman thirty years old: and another genuine instance was seen by Dr. Gregory, at the Small-pox Hospital, in the person of an adult female. The eruption is preceded by little or no premonitory fever, commencing usually on the shoulders, neck, and breast, affecting almost always the scalp, but sparing very much the face—which, in small-pox, never escapes.

According to the testimony of several accurate observers, the eruption begins in the form of small rose spots, which are rapidly followed by perfectly transparent vesicles, surrounded by a very slight degree of superficial redness. They are usually numerous, but distinct. Dr. Gregory says that when the eruption is very copious, the body has the appearance of having been exposed to a momentary shower of boiling water, each drop of which had occasioned a minute blister. The shape of the vesicles on the trunk of the body is said to be often transversely oval. Crops of vesicles appear in succession for four or five days, or rather for three or four nights, if, as Trousseau affirms, the accessions of fresh vesicles are mainly nocturnal. While new ones are forming, the first are beginning to shrivel.

After the second or third day the vesicles become slightly opaque, and like pearls; and they end in scabs. When irritated by friction, they sometimes take on so much inflammation as to be converted into pustules. The scabs are small and gummy, dry quickly, and crumble off, instead of being detached in one mass. In a few instances, shallow cicatrices are left by the vesicles. During the short progress of this eruptive disease there is no constitutional disturbance of any consequence.

It has been ascertained of this genuine chicken-pox, or varicella lymphatica, that it occurs once only to the same person; that it spreads by contagion; that, nevertheless, it is not communicable by inoculation—whereas the matter of modified small-pox, when engrafted, produces genuine variola; that it occurs equally among those who have, and those who have not, been vaccinated; that its course is not affected by antecedent vaccination; and that the vaccine vesicle and disease proceed with perfect regularity after the occurrence of chicken-pox. Now this does not happen after small-pox.

It appears, from Möhl's work, 'De Varioloidibus et Varicellis,' that from the year 1809 to 1823, chicken-pox was annually observed at Copenhagen without concomitant small-pox; and that both diseases have since prevailed at intervals epidemically, but always under circumstances which satisfied the physicians of the town that their sources were distinct.

In the 15th volume of the 'Medical Gazette' you may read a report—communicated by the late Sir James Macgrigor—from Dr. John Murray, giving an account of several distinct visitations of small-pox at the Cape of Good Hope.

Dr. Murray states, that from the year 1812, up to the date of his report in 1834, no case of small-pox had occurred in the colony; but that varicella prevailed there, more or less, every year: that it affected alike the vaccinated and the unvaccinated: that it had never been known to give rise to variola or varioloid disease: nor to have been the result of variolous contagion: nor to have affected the same person twice.

It must, therefore, I think, be admitted, that there is a separate disease, called chicken-pox, which springs from a specific poison; produces a vesicular eruption; runs a definite course; has no tendency, when undisturbed, to suppuration; occurs but once; and affords no protection against small-pox, while, on the other hand, small-pox affords no protection against it.

The main point of practical importance is, however, this: that if we meet with any eruption which is at all equivocal, we should use the same precautionary measures for preventing the extension of the disease as if we were sure that it was modified small-pox. But this salutary rule is often, I say, neglected or infringed, to the danger and detriment of those unprotected persons who happen to be in the vicinity of the sick child.

The treatment required in chicken-pox is abundantly simple; it is the same, in fact, which has been already recommended for the mildest cases of the discrete small-pox.

Another of these blood diseases is *the measles*: called, also, by nosologists, *rubeola*, and *morbilli*.

Like different human faces, all the complaints belonging to this group have the same set of features, and therefore a mutual resemblance, while the separate lineaments differ so much in their character and relation, as to give to each disease its distinctive aspect. There are also minor shades of difference between individual cases of the same specific malady.

Measles, accordingly, has its stage of incubation, its introductory fever, its period of eruption, its peculiar kind of eruption, its course by stages. It is communicable from person to person, and it generally occurs but once to the same person. On some of these points I spoke before.

The introductory fever is sometimes severe. Like all fevers, it begins with lassitude, and shivering, which are soon followed by heat of skin, acceleration of the pulse, anorexia, and thirst. But the peculiarity in the fever which precedes the eruption of measles is, that it is very constantly attended with an inflammatory condition of the mucous membranes; especially of those which come in contact with the air. The eyes become vascular and watery, the eyelids heavy, turgid, and red. The membrane which lines the nasal cavities, the fauces, the larynx, trachea, and bronchial tubes, is affected. Hence we have, generally, as symptoms, much sneezing, as well as lachrymation, a copious defluxion from the nostrils, soreness of the throat, and an obvious redness of the fauces, and most commonly a dry, hoarse, peculiar cough. In short, the symptoms which usher in an attack of measles are the symptoms of coryza and catarrh. In some instances there is diarrhœa also, indicating a simultaneous affection of the mucous membrane of the intestines; and not unfrequently vomiting: but the vomiting, as in small-pox, ceases upon the coming out of the eruption.

The regular period for the appearance of the eruption is the fourth day of the disease; seldom earlier, frequently later: sometimes as late as the eighth or tenth day from the commencement of the catarrh. The eruption itself is a rash, consisting, at first, of minute papulæ, which, as they multiply, coalesce into blotches that have, more or less, a horse-shoe or crescentic shape, and leave the intermediate portions of skin of their natural colour. It is two or three days in coming out, beginning on the face, neck, and arms, then reaching the trunk of the body, and so travelling down to the lower extremities. In this course it resembles the eruption of small-pox. It fades in the same order, standing out three days at least on the face before it begins to decline; so that its whole duration comprises a space of six or seven days. It becomes browner as it fades. You may feel that it is slightly elevated above the general surface of the skin, especially upon the face, which is somewhat bloated and swollen. The parts which the rash has recently occupied are left covered with a dry, small scurf. The cuticle does not peel off in large flakes, as I shall have to tell you that it oftentimes does in scarlet fever, but crumbles away in a fine branny powder. Occasionally, yet very seldom I believe, the rash is intermixed with a few small and short-lived vesicles.

This termination of the papulæ is very unlike what happens in variola: and connected with the eruption there are two other important particulars in which the measles differs essentially from the small-pox. In the first place, the fever does not cease, nor even abate, upon the emergence of the rash; but continues, decreasing however as it fades, unless indeed some complication arises. And in the second place, the disorder is not more severe, nor more dangerous, because the eruption is plentiful, or early. So far from it, indeed, that in some of the worst and most perilous cases the eruption is apt to be partial, and to appear late and irregularly.

The eruption is the *distinguishing* feature of measles, but the catarrhal affection is, in every way, the *most important.* Indeed the rash may, and sometimes does, happen without the fever and the catarrh; and nosologists recognise a variety of the disorder under the title of *rubeola sine catarrho.* But it is observed of this variety, that it confers no protection whatever against the recurrence of the malady: in truth, it is most commonly succeeded in a few days by an attack of measles in its regular and complete form.

I need not stop to repeat what I told you in a former lecture about the other general features of this eruptive complaint. The period of incubation is from ten days to a fortnight. The contagion is active enough, though certainly it is less strong and diffusive than that of small-pox. Mr. William Squire has recorded some cases which prove that the disease is infectious prior to the appearance of the cutaneous rash: and we are reminded by Trousseau, that before any exanthem becomes visible on the skin, the presence of the disorder is legibly inscribed upon the mucous surfaces of the eyes, pharynx, and palate. When once introduced into a family or school, it rapidly spreads to those individuals who have not already had it. It is capable, though with much less readiness and certainty than small-pox, of being propagated by inoculation; but as the disorder is not rendered milder by being so introduced into the system, this process has no utility or interest, and is never resorted to. Occasionally rubeola visits the same individual twice; but this is an exception to the general rule. Perhaps, in some reputed instances of its recurrence, the first accession may have been without fever and catarrh, and therefore an ineffectual safeguard for the future. I myself know, however, two large families in which most of the children have suffered a repetition of the genuine unmitigated disease.

The measles resembles the other diseases of the group in this also, that at times it pervades a community as an epidemic; at times occurs here and there only, sporadically. The general character of the symptoms varies considerably in different epidemics. Morton and Sydenham, and after them Sir William Watson, have described visitations of what they call *putrid* measles. Sir William Watson was physician to the Foundling Hospital, and he witnessed two epidemics of this putrid kind among the children in that institution. He states that the eruption appeared unusually early, so early as the second day of the disease; and that, besides cough and dyspnœa, the complaint was marked by extreme debility, and attended with dysenteric diarrhœa. More seemed to die of the intestinal affection than of the pectoral. He lost, in one of these epidemics, nineteen out of one hundred and eighty-three patients. The malign character of the disorder was manifested by the frequent occurrence of gangrene, both externally and internally. In this low form of measles, the rash is often irregularly and imperfectly developed, and of a livid colour.

Sydenham found that measles of an unusually bad kind prevailed in London in the years 1670 and 1674; the very same years in which small-pox was also remarkably malignant and fatal. This illustrates what I have stated before; viz., that the putrescent tendencies of these and other febrile disorders depend less upon any peculiar virulence in their *exciting* causes, than upon some change previously effected in the human body by the silent and gradual influence of certain *predisposing* causes.

The diagnosis of measles is seldom difficult. In the outset of the fever you may guess what is coming by the coryza, catarrh, and hoarse cough; especially if the disease be about. On the very first day of the eruption, the small, red, and hitherto separate spots are very like the incipient pimples of small-pox. Do not, therefore, at this period, express too confidently your opinion respecting the nature of the complaint. Parents and nurses might be charitable enough to attribute your mistake to inexperience or ignorance. The progress of the disease will soon remove all doubt. The eruption of small-pox has a harder feel than that of measles, and it presently exhibits some fluid, whereas that of measles has none—unless, indeed (what is uncommon), a few miliary vesicles mix themselves with it. But these make no advance in twenty-four hours. Ordinarily the isolated pimples visible upon the first day soon augment in number, and collect themselves into semicircular groups; and if any question at all arise, it is whether the disease be measles or scarlet fever. I shall presently describe the latter disorder; and then I will point out the marks of distinction between the two.

The prognosis in measles is governed chiefly by the mildness or the severity of the pectoral symptoms. The most common cause of death, in the fatal cases, is inflammation of some one or more of the textures that compose the lungs. And even when this immediate danger has passed by, the disease too often leaves chronic pulmonary mischief behind it. In scrofulous children, and young persons, it frequently awakens the slumbering germs of consumption. And when that specific effect is not produced, it is apt, in adults, to inflict upon the constitution a hurt which is never thoroughly recovered from; the patient becoming, from that time forwards, delicate and valetudinary. The prognosis is always unfavourable when the eruption does not stand out well, is of a livid colour, and accompanied with putrid symptoms, or with a disposition to gangrene.

We augur favourably of the case when the thoracic symptoms are not severe; when the fever does not much augment upon the coming out of the rash; and when the rash is steadily persistent, and there is no excessive prostration of the strength. The ordinary range of temperature is between 102° and 104°; and if it continue to be high after the eruption has begun to fade, some complication, present or impending, is to be dreaded.

Being contagious, and occurring for the most part but once, measles is principally seen in children, although no period of life is exempt from its attacks. In many children the disorder is so slight as to require little more than judicious domestic attentions. The free application of cool air to the surface, which is so beneficial in small-pox, would in measles be less safe, on account of the pectoral symptoms. For this reason the patient should be kept in bed; with no more clothes, however, or warmth of the apartment, than he is accustomed to in health. The antiphlogistic regimen must be adopted; and when the bowels are not open naturally, gentle laxatives should be given. It may be well, also, to prescribe some diaphoretic medicine; a draught, for example, containing two or three drachms of the *liquor ammoniæ acetatis*, with half a drachm of the *spiritus ætheris nitrici*, and an ounce of camphor julep, to be taken three or four times in the twenty-four hours.

The most important part of the treatment relates to the remedies to be employed for the pulmonary symptoms, which in the outset depend, almost always, upon slight bronchitis. But the inflammation is apt, in severe cases, to spread insidiously from the mucous to the other tissues—the bronchitis becomes pneumonia—and we find, after death, some portions of the lungs hepatized; usually small portions. For the most part, however, it is extensive inflammation of the bronchial mucous membrane that we have to dread. And really I cannot give you any better or fuller directions with respect to the management of these inflammatory affections, than I endeavoured to lay down when I was speaking of *bronchitis* and *pneumonia*, as they occur idiopathically. You will judge of the extent and severity of the inflammation, partly by the common symptoms, partly by the help of your ear; and you must apportion your remedies to that intensity, so judged of. You will take blood by leeches from the chest, apply linseed poultices or warm fomentations, and give diaphoretics. The known presence, however, of a specific poison in the blood should forbid any unnecessary resort to lowering remedies.

When the rash is about to decline, a spontaneous diarrhœa often sets in, and appears to have a beneficial effect in abating the febrile symptoms. If this natural curative process should fail to occur, it may be imitated by the prescription of gentle aperients.

In weakly children blisters are apt to cause troublesome sores; and in some epidemics of measles, the sores thus produced show a disposition to become gangrenous. When any such tendency is noticed, blisters had better be avoided altogether. At other times, the inconvenience to be apprehended from a blister may be prevented by one of two plans; either by interposing a piece of tissue paper between the blistering plaster and the skin; or by suffering the blister to remain upon the part three or four hours only, then taking it off, and applying a poultice. The cuticle will rise under the poultice, and the sore will not, in general, be a troublesome one.

If the eruption disappear prematurely, it may sometimes be restored by putting the patient into a warm bath. And if he be at the same time in a low state, especially if what I have called putrid symptoms threaten or show themselves, you must treat the case upon that indication, just as you would in continued fever; giving wine and animal broths, and watching the effects of these, and apportioning their quantity accordingly.

It is of considerable importance to protect the patient from danger *after* the disease has subsided; by warm clothing, by preventing him from going out of doors too early, or being in any way exposed to cold. Pneumonic inflammation, and dysenteric purging, are frequent consequences of the want of prudence in this respect.

In the year 1846, an epidemic of measles spread itself through the group of small islands lying between Shetland and Iceland, and called the Feröe Islands. The disease was so serious and general, that the Danish Government thought it necessary to send two physicians from Copenhagen, Dr. Manicus and Dr. Panum, to the relief of the Islanders. You may read a short but interesting report upon this epidemic, by Dr. Panum, in the *Archives Générales de Médecine* for April, 1851. It so well illustrates several of the points on which I have been speaking, that I am tempted to extract some of its statements.

In these islands—which are separated from each other by narrow but dangerous channels, and which are debarred from much intercourse with the world, both by their geographical position, and by their having no external commerce—measles had been totally unknown from the year 1781. The disorder was brought to them in 1846, by a man who left Copenhagen on March 20, arrived at the Island of Thorshavn, apparently well, on the 28th, and sickened on April 1. In October, the disease had again disappeared from the islands. During that interval of about six months, of 7,782 inhabitants of the seventeen islands, 6,000 underwent the disease.

You will notice here the entire absence of this contagious complaint for sixty-five years, and its immediate and rapid diffusion upon the introduction of the contagion.

In our own island we see the measles chiefly among children and young persons. There it affected persons of every age. In a village containing one hundred dwellers, eighty were laid up with it at the same time.

All the old people who had had the complaint in the epidemic of 1781, escaped it in 1846.

This shows two things—1st. That subsequent immunity from the disease is the rule. This rule was not broken in a single instance. 2ndly. That the protection afforded by one attack does not wear out as life advances.

Again, of the older persons who had been alive in 1781, and had not been exposed to the contagion (and Dr. Panum could reckon one hundred such persons), all took the disease in 1846; whereas a few youths, though they mixed with the sick, were untouched by it.

From this we learn that the susceptibility of the disorder does not decrease, as the age increases; and that the reason why it is so seldom witnessed in adults in this country is the same which I before assigned. The great majority have had it during early life, and are therefore incapable of taking it later.

Dr. Panum found—after careful and extensive observation, conducted under circumstances of unusual freedom from the fallacies and uncertainties which beset such enquiries in larger and more complex communities—that a period of thirteen or fourteen days intervened very regularly between the time of exposure to the contagion, and the time of the eruption of the characteristic rash. Take a single example of this from among many.

One of the smaller islands, called, however, Le Grand Dimon, contained eighteen persons only, who all belonged to the same family. A boat manned by a few of them made a voyage to Tveraa, where the disease was rife, and returned after staying there some hours. Up to the tenth day from that time all these voyagers seemed perfectly well. On the 14th day the eruption appeared upon them all; and fourteen days after that, it showed itself with the same regularity in all the other members of the family.

He noticed that the precursory symptoms were of uncertain duration; sometimes they occupied six or eight days, sometimes from four to six, but generally from two to four days.

The disorder proved very catching at the outset of the eruption and during its whole continuance. Isolation was the only sure defence against it. Dr. Panum thinks that 1,500 persons escaped it, by establishing regulations equivalent to those of quarantine.

From the time when the great fatality, and the intense dread, of small-pox were dispelled together by the practice, however incomplete, of vaccination, *scarlet fever* has possessed the bad eminence of being the least uniform, the most unmanageable, and by far the most terrifying and deadly of all the exanthematous group.

In the eighteen years from 1847 to 1864 inclusive, there died of this disease in England and Wales 318,122 persons; giving an annual death record of 17,673. It has been extraordinarily rife and virulent in this country during the last two or three years, destroying 27,641 lives in 1869, and 31,910 in 1870.

Like the rest, it is a contagious disorder; and it is attended almost always during a part of its course, by a rash, and by sore-throat. It seldom visits the same person twice: and among the rare instances of its recurrence there has never, according to Dr. Richardson, been a fatal case.

The subdivision of any disease into numerous and needless varieties serves rather to confuse than to simplify our conceptions of it as a whole. But the differences are so extreme between different manifestations of scarlet fever, as to make it convenient, for the purpose of description, and for the better direction of the treatment, to draw some lines of separation between them. There is more room, and more need, for the intervention of the physician in the progress of this disease, than in that of any other of the class.

Its two most striking and important features are the *affection of the throat* and the *affection of the skin*. They may both be well marked; or only one of them may be well marked: and this circumstance has led nosologists to divide one and the same complaint into two independent maladies; to which Cullen and others have assigned the respective names of *cynanche maligna*, and *scarlatina*. When, in an earlier part of the course, I was treating of the diseases of the throat, I purposely omitted the cynanche maligna; because that is only another name for a particular form of scarlet fever. If you look to Cullen's definitions of these complaints, you will see how very much alike they are. They both specify inflammation of the fauces, a cutaneous rash, and fever. But in the definition of scarlatina, the rash is dwelt upon and described, and the fever is called synocha; while in that of cynanche maligna, the ulceration of the throat is more insisted on, and the fever is said to resemble typhus. The truth is, that these two kinds of disorder are both caused by the same contagious poison. The malignant sore throat may be caught from a patient who has mild scarlet fever; and mild scarlet fever may, in like manner, be contracted from one who is suffering under the malignant sore throat. The two forms graduate insensibly, in different cases, towards each other; and it would be impossible, even if it were desirable, to draw any strict line of separation between them. Many would say, and probably with truth, that their difference is this: in the one form the poison of the disorder is seeking its vent principally by the throat, in the other by the skin. If the kidneys also were visible, we should in most cases conclude that they, too, were among the *vomitoria* of the poison.

Authors generally make three *varieties*, or I would rather call them *grades*, of scarlatina. Scarlatina *simplex*, in which there is a florid rash, and little or no affection of the throat; scarlatina *anginosa*, in which both the skin and the throat are decidedly implicated; and scarlatina *maligna*, in which the stress of the disease falls upon the throat. The epithet *maligna* marks truly the fearful character of this form of the malady.

To these three varieties, Dr. Copland has added a fourth, which he names scarlatina *latens*. This addition is warranted by the fact (certified now by the testimony of several careful observers, of Dr. Graves among others), that certain well-known and remarkable sequelæ of scarlet fever sometimes declare themselves in persons who had been living with others sick of that disease, but in whom its primary and diagnostic symptoms had not occurred, or had occurred in so slight a degree as to escape observation. It is the *scarlatine fruste* of M. Trousseau.

I need scarcely remind you of a sort of mystification which prevails among the public about this complaint, and which many practitioners, for no good reason that I can perceive, seem disposed to encourage. Mistaking the Latin and scientific name of the disorder for a mere *diminutive*, you will hear mammas say, 'Oh, my children have not got the *scarlet fever*, but only the *scarlatina*.' I always disabuse them of this absurd error, when the opportunity of doing so occurs. It can produce nothing but confusion, and a perilous disregard of requisite precautions.

Like measles, and for the same reasons, scarlet fever, though persons of all ages are susceptible of it, is eminently a disease of children; but it is much more to be dreaded than the measles.

It is somewhat strange that scarlet fever was not recognised, in this country at least, as a distinct disease, till about two centuries ago. In all probability it had long existed, and had been always confounded with measles. Morton speaks of it under the name of *morbilli confluentes*; and Hoffman calls it, by a similar mistake, *rubeola rossalia*. The febris scarlatina described by Sydenham, must have been of a very mild kind; for he does not mention any ulceration of the throat. Dr. Fothergill, in 1748, was the first to describe, as a new and separate disorder, that dangerous form of the complaint which Cullen designates cynanche maligna; and it was long called the Fothergill sore-throat. The identity of this affection with genuine scarlet fever has been slowly established by subsequent observers. The characteristic differences between scarlet fever and measles were first fully specified by Dr. Withering.

The period of incubation in this disorder is short; not exceeding, usually, five or six days; often briefer still. M. Trousseau relates an instance in which it was not longer than twenty-four hours. My bootmaker went down from London to see his wife and children in Devonshire. Arriving on a Sunday at noon, he unexpectedly found that one of his children had scarlet fever. On Monday he took a gallop with some hounds, and in the evening felt unwell. On Tuesday he had sore throat and sickness, which proved to be early symptoms of a thorough attack of scarlatina.

The disease begins, as the exanthemata in general begin, and as continued fevers which I have grouped with them are apt to begin, with lassitude, and rapidly augmenting debility; with headache, frequently severe, sometimes with delirium, occasionally with nausea and vomiting. There may or there may not be some shivering. Then, generally on the second day (and Cullen is wrong when he says it is generally on the fourth), the eruption begins to come out. In some of the worst forms of the disease it may, indeed, be deferred till the fourth day.

Although scarlet fever and measles were so long confounded together, the differences between them are well pronounced, and, when once pointed out, are easily enough recognised.

Rubeola is distinguishable, then, from scarlatina—

1. By the presence, at the outset, of catarrhal symptoms—by the sneezing, the cough, the defluxion from the eyes and nose, which precede the rash. There is, doubtless, in many cases of scarlatina, a running from the eyes and nose, but not till late in the disease; at any rate, not prior to the eruption.

2. By the absence of severe inflammation and ulceration of the throat; symptoms which always accompany severe cases, at least, of scarlet fever.

3. By the characters of the eruption itself. The rash in measles is more elevated above the surface than in scarlatina, and of a darker colour. In measles it is said to present somewhat the tint of a raspberry, and in scarlet fever to have that of a boiled lobster. In measles the papulæ are collected into semi-lunar groups, leaving interstices between them of healthy skin. The redness of scarlatina commences in minute points, which speedily become so numerous and crowded, that the surface appears to be universally red. They begin on the neck and breast, and extend to the extremities, pervading at last every part of the skin. The scarlet colour is deeper, in general, about the groins, and in the flexures of the joints, than elsewhere. Lastly, the rash of measles, in its most regular form, appears on the fourth day of the disease; that of scarlet fever on the second.

On the arms and legs the eruption of scarlatina occasionally differs somewhat from that which is visible on the trunk; is more streaky, spotty, papular, and the papulæ are somewhat prominent, while over the body there is a general punctuated blush.

In some cases of scarlet fever (probably in some epidemics, for I observed the phenomena I am about to mention in four or five cases in succession which were brought into the Middlesex Hospital within the space of a month or six weeks), some parts of the red surface are closely studded with little transparent vesicles, containing a thin colourless liquid, and resembling what I described to you before as *sudamina*. In all the instances in which I have seen them, these minute vesicles have been most thickly set on the thorax, and on the front and sides of the neck. The liquid is soon re-absorbed, and the cuticle under which it had been enclosed shrivels up, turns white, and comes off in a thick white scurf: so that the part from which it separates looks at first sight as if it had been powdered. I have recently seen two cases of this vesicular form of scarlatina in private practice. I show you Rayer's delineation of the vesicles.

The eruption, in the most regular and favourable cases, stands out for three or four days, and then begins to fade and decline, becoming by degrees indistinct, and disappearing altogether, in the majority of instances, towards the end of the seventh day. About this time desquamation of the cuticle begins to take place, in smaller scurf or scales from the face and body, in large flakes frequently from the extremities. The scarf-skin of the hands and of the feet sometimes separates almost entire. A glove or a slipper of cuticle comes away at once. You may see such things in most Museums.

In that variety of the disorder which we call scarlatina *maligna*, the rash is apt to come out *late*, and imperfectly, and sometimes not at all; and instead of being bright and florid, to present a bluish or livid tint. Sometimes it suddenly recedes; and then, perhaps, appears again: and occasionally it is diversified by purple spots.

Willan and Bateman have given the name of *roseola* to an eruption which is also attended with inflammation of the throat, and between which and scarlatina it is certainly difficult, if not impossible, at first to discriminate. The roseola, however, is not contagious, and has more of a chronic character than scarlatina. It comes and goes, and has no settled or definite course.

The appearances of the *tongue* in scarlet fever are peculiar and characteristic. In the scarlatina *simplex*, and *anginosa*, it is often covered, at the outset, with a thick, white, cream-like fur, through which are seen projecting the red and exaggerated papillæ; its edges being likewise of a bright red colour. The red points gradually multiply, and the white fur clears away, and at length the whole surface of the tongue becomes preternaturally red, and clean, and raw-looking: and after becoming thus clean, as well as red and rough, and like a strawberry, it will sometimes, when the disease goes on unpromisingly, get dry, and hard, and brown—as you know it is apt to be in certain species and stages of continued fever.

The temperature in scarlet fever varies with the varying intensity of the disorder. In the slightly-marked cases there seems to be no elevation of the bodily heat. In typical cases of ordinary severity it rises rapidly at the outset to 104° or even 105°. Dr. Bathurst Woodman, judging from his own experience, states that the temperature being 105° on the first day of the eruption, usually descends by one degree daily, till, on the sixth day, it reaches 100°, and in about two days more has regained its natural standard. According to Dr. Sydney Ringer, the curious law obtains that, in a large majority of cases, the temperature falls decidedly on the fifth day of the *disease*, or on the tenth, or on some other multiple of the fifth.

If, after the fall on the fifth day, it should rise again to any considerable extent, some complication may be inferred.

In fatal cases there is much irregularity. The temperature before death rises sometimes to enormous heights, to 110°, 111°, and even to 115°.

The first thing of which the feverish patient usually complains is sore throat, with some stiffness of the neck: and if you inspect the fauces, you will see, without in general so much swelling of the tonsils as occurs in common quinsy, a diffused redness, sometimes of a dark claret colour, including a large part of the palate. In a short time you may perceive that the tonsils and velum are covered irregularly with whitish exudations, or gray aphthous crusts: or, perhaps, you see a sloughy kind of ulceration left by the separation of these crusts.

The progress of the distemper, and its degree of severity and of danger, differ, as I have said, very greatly indeed in different cases. Sometimes the deviation from the feelings and condition of health is so very slight as scarcely to deserve the name of a disease; sometimes the disorder defies all treatment, and the deadliest forms of plague are not more fatal.

In these malignant and terrible cases, the eruption, if it appear at all, is livid and partial, and fades early, and is attended with a feeble pulse, a cold skin, and extreme prostration of strength. Sometimes the patient sinks at once, and irretrievably, under the virulence of the poison, and life is extinguished in a few hours. This may be from the large dose of the poison imbibed. The phenomena are analogous to those of the poisons properly so called. A large dose of arsenic, or of oxalic acid, may speedily prove fatal before there has been time for the establishment of any detectible organic mischief. A gentleman called one day at my house, and not finding me there, followed me between twelve and one o'clock to the hospital. He wished me to visit his wife, four or five miles out of town, who had been taken ill that morning. He feared that she was about to have scarlet fever, but he was not much alarmed for her safety; for when he found that I could not be at his house before six, he said that that hour would not suit the general practitioner in attendance upon her, and he asked me to fix some time for seeing her *the next day*. I did so; but the same afternoon rapid sinking came on, and the patient was dead very soon after the hour at which I had first proposed to visit her.

In other cases of scarlatina maligna, the typhus-like symptoms rapidly deepen: and death, in children, is apt to occur on the *fifth* day of the complaint; and not uncommonly as soon as the *third*. The pulse becomes frequent and feeble; the tongue dry, brown, and tremulous; the debility extreme; the breath offensive; the throat is livid, swollen, ulcerated, and gangrenous; and the respiration is impeded by viscid mucus which collects about the fauces. Over this variety of the disease, medicine has comparatively little control.

The chance of recovery is much greater in the scarlatina anginosa, when the eruption is florid, and stands well out. But even in this form of the disorder there are many sources of danger, and various ways in which it may prove fatal.

In the first place many of the patients die, apparently from inflammation or effusion within the head. They have violent headache, with furious delirium, which is followed by coma and death.

And, secondly, the state of the throat is full of peril. As the disease proceeds, although the rash may be steadily persistent, the throat becomes foul and sloughy; an acrid discharge from the nostrils, which are so stuffed and swollen internally that the patient can scarcely breathe through them, runs over and frets the upper lip; the parotid and submaxillary glands swell, sometimes enormously; and fever is lighted up afresh. In this way many cases prove fatal in the second week of the disorder. The cervical swellings cause constriction of the fauces and stiffness of the neck; and sometimes, doubtless by interfering with the free return of the blood from the head through the jugular veins, they produce a tendency to coma. With these symptoms there is often also purging, and an excoriated anus.

The acrid matters furnished by the ulcerating and gangrenous throat irritate the nasal membrane in the one direction, and that of the alimentary canal in the other. We thus account for the running from the nose, the soreness of the alæ nasi and upper lip, and the smarting diarrhœa: and the swelling of the parotids and neighbouring glands is evidently caused by absorption of the irritating and poisonous matter from the ulcerated throat. There is just the same relation and dependency between these different local alterations, as between the enlarged mesenteric glands, and ulceration of the follicles of Peyer in typhoid fever; between a bubo in the groin, and a chancre on the glans penis. It is the condition of the throat that gives rise, in these cases, to the most formidable symptoms. The system is re-inoculated from that source. Whenever I see the glands much enlarged at the angle of the jaw, and beneath the jaw, in a child labouring under scarlet fever, I augur ill of the case. Sometimes the mischief extends into the larynx, and so destroys the patient. But this is probably a very rare event. There is, however, still another, and a very common consequence of the throat affection—I mean inflammation of the eustachian tube, reaching sometimes the tympanum itself, and causing permanent deafness, either by closing up the tube, or by the destruction of the membrana tympani, and of the little bones belonging to it. In one case, which was under my own care, I observed that, for a short time before death, every time the child swallowed, a part of the fluid food ran out immediately at one of its ears. I had no opportunity of examining the state of the part after death, but the disorganisation arising from the sloughing ulceration of the throat must have been frightful.

Scarlet fever sometimes befals parturient women; and then it almost always proves fatal. I have seen three instances only of recovery from this perilous complication. Probably some other morbid conditions associated with the parturient state have been occasionally mistaken for scarlet fever. An eminent accoucheur ceased to attend a lady because he saw on her skin a redness which portended scarlatina. I saw her the same day, and found swelling and redness of her left shoulder; the joint was presently disorganised, and she died of pyœmia.

Scarlatina simplex is scarcely, I repeat, a disease. Sydenham has said of it, that it is 'fatal only through the officiousness of the Doctor.'

Even when the patient has escaped from the complaint itself, he is often exposed to great hazard and distress from its *consequences.* Children who have suffered a severe attack of scarlet fever are liable to fall into a state of permanent bad health, and to become a prey to some of the many chronic forms of scrofula; boils, strumous ulcers, diseases of the scalp, sores behind the ears, scrofulous swellings of the cervical glands and of the upper lip, chronic inflammation of the eyes and eyelids. The same afflicting results are very common after small-pox also, and measles.

I have several times, when the rash of scarlet fever was disappearing, known pain and swelling of the larger joints to supervene, simulating very closely the local phenomena of subacute rheumatism; and I have noticed that the painful joints were eased and benefited by friction: a circumstance which may help to distinguish this articular affection from genuine rheumatism. Another distinctive circumstance seemed to be that, although all these patients were children, the heart in no instance became implicated, in connexion with the tumid joints. Upon this point, however, my own experience may have been fallacious. Dr. Scott Alison has invited attention to the subject, in an interesting Essay 'On Pericarditis, a complication and sequela of Scarlatina.' Accepting his facts, I should ascribe the articular affection, and the cardiac affection, whether they occurred together or separately, to one and the same cause; namely, to the retention in the blood of a poisonous excrement, by the default of the principal emunctories, and especially of the kidneys.

But certainly the most common, and a very serious sequel of scarlatina, is *anasarca,* serous infiltration of the subcutaneous areolar tissue, accompanied often with dropsy of the larger serous cavities. So common is this that Cullen has even introduced the circumstance as a part of his definition of scarlet fever. He found the dropsy a very manageable complaint; but it really is, in many—nay, in most cases, if we look to its possible ultimate consequences—a most formidable one. This affection belongs to the class of *febrile dropsies.* It appears to have no relation, or, if any, an inverse relation, to the violence and danger of the preceding exanthem. It is much more common after a mild, than after a severe disease. This I believe to be chiefly owing to the circumstance that less care and caution are observed in the milder cases during the dangerous period of desquamation and convalescence; a period more dangerous, in that variety of scarlatina, than any other. In the graver cases the convalescence is slower, and more doubtful; and accidental or careless exposure to cold is more guarded against, or takes place later: whereas, in the slighter kinds of the disorder, the patients are apt to go out while the new cuticle is still forming. The escape of the fever-poison through the large outlet afforded by the skin is checked or prevented. More of it is hurried through the narrower wicket of the kidneys, and gives rise, in its tumultuous and embarrassed outbreak, to what Dr. George Johnson has called 'acute desquamative nephritis.' If you carefully trace the histories of dropsy succeeding to scarlet fever, you will very frequently find that the fever had been trifling; and that the patient, considering himself well or nearly so, had heedlessly encountered a cold or damp atmosphere so soon as he felt himself strong enough to leave the sick chamber. Plenciz, who has written well on this subject, and who was quite aware of its importance, remarks that those patients who have had much desquamation of the cuticle are the most liable to the dropsy; that it is more frequent in winter than in summer; and in such as are early exposed to the open air after having passed through the fever, than in those who remain longer at home. When the desquamation is over, and the new surface has become in some degree hardened, the peril is past. According to the observations of Dr. Wells, the dropsical symptoms commonly show themselves on the twenty-second or twenty-third day after the commencement of the preceding fever. They have been known to begin as early as the sixteenth, and as late as the twenty-fifth day. When no dropsy took place before the end of the fourth week, Dr. Wells always ventured to state that it was no longer to be dreaded.*

This anasarca is seldom observed except in children and young persons. The age of the oldest patient that Dr. Wells had known to be so affected was seventeen. Of ten instances of the disease seen by Dr. Blackall, six occurred in children not exceeding the age of ten, and two others in persons who were respectively ten and sixteen years old.

We cannot infer, from this, that the susceptibility of this dropsical condition lessens as years increase. The great prevalence of this variety of dropsy in early life has no direct relation to age as a predisposing cause. The fact is explained by the accidental peculiarities of the antecedent disease. The contagion of scarlet fever is active and widely diffused. Few children escape its agency. Few are capable of taking the disorder a second time. It follows that scarlet fever is rare in adult life: and as dropsy succeeds that disease in a very limited number of instances only, dropsy arising in connexion with scarlet fever must, at the adult age, be still more uncommon.

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* Dr. Tripe, in a paper contained in the 'Medico-Chirurgical Review,' for July, 1854, assigns larger limits to the first appearance of the dropsical symptoms, and states that the fourteenth day of the disease is most frequently of all the day of their invasion.

Yet it is not unknown. One of Dr. Blackall's ten patients was thirty, another forty-two years old. Both of these were women.

In this, as in other species of febrile dropsy, the urine is very dark, olive-coloured, albuminous, and sometimes bloody; and it contains fibrinous casts of the renal tubules, with epithelial cells intermixed.

Not only may one case of scarlet fever differ widely from another case, but very great differences are also observable in the general character of different epidemics. This is true indeed of all the exanthemata. It is signally true of this. In some epidemics the disease is almost uniformly mild, in some it is fearfully severe and dangerous. The inflammatory symptoms may in one epidemic run high, while throughout another the low or typhous type may predominate. We should learn from such differences not to dogmatise, in reliance upon our own experience of one or two epidemics, respecting the most fitting management of the disorder; nor to criticise ungenerously and with ignorant arrogance the treatment recommended by others, who may have observed the disease elsewhere, or at some former time. Dr. Graves, in his 'Clinical Lectures,' has left us some sound instruction on this point. In the years 1801–1804, an extremely fatal epidemic of scarlet fever prevailed in Dublin. 'It thinned many families in the middle and upper classes of society, and even left not a few parents childless.' During the next 27 years, epidemics were frequent; but they all were of a mild character. In one instance, not a single death occurred among 80 boys attacked in a public institution. It was thought in Dublin—even Dr. Graves was beguiled into the error of thinking—that the diminished mortality of the disease was entirely attributable to the treatment adopted, to the cooling regimen, and the timely use of the lancet and aperients, which had in times past been interdicted. But subsequent experience of a severe epidemic in 1834–1835 upset this belief; and showed that, in spite of the boasted improvement in practice, the Dublin doctors were not more successful than had been their predecessors in 1801–1802.

Take another illustration from the statistics, already referred to, of the Registrar-General. In the year 1861, there were in England 9,077 deaths from scarlet fever. Two years later, in 1863, there were 30,475. These diversities are well shown in the histories given of scarlet fever, as it has prevailed epidemically in the charitable institutions for children which abound in and near Edinburgh. In such places, the means of studying epidemic contagious disorders are singularly precious and instructive. The inmates are nearly of the same age, are all living under precisely similar circumstances as to food, clothing, shelter, and general habits, and are at the complete disposal, and under the frequent and close observation, of the attending physician. You may read in the 'Edinburgh Monthly Journal of Medicine' some very interesting and valuable records of these visitations of scarlet fever from the pens of Dr. Gillespie, Dr. Newbigging, Dr. Andrew Wood, and Mr. Benjamin Bell. Much attention was paid by these gentlemen to the relations subsisting between scarlet fever, the presence of albumen in the urine, and the occurrence of dropsy. This subject has also been carefully investigated by Dr. Warburton Begbie, who had likewise large opportunities of witnessing the disorder in the Edinburgh Infirmary and elsewhere. I have much trust in his conclusions, which are briefly these :—

In most, if not in all cases of scarlet fever, the urine, at one period or another of the disease, contains more or less albumen. With a few exceptions, the time when it begins to appear is shortly after the commencement of desquamation of the cuticle. The albuminous condition is most often transient, and is by no means necessarily attended with anasarca. It lasts from a day or two to ten days, its average duration being four or five days. It may easily be overlooked if the urine be not very frequently examined. The amount of albumen is generally small. When once it disappears from the urine, it never reappears. The specific gravity of the urine remains high, and its quantity is usually plentiful. Here, however, comes in the connexion with dropsy. If the urine, thus albuminous, become scanty, then the supervention of anasarca may be looked for. While there is no anasarca, the urine may be albuminous, but it contains no casts. As soon as anasarca occurs, casts and epithelium, and sometimes even blood, are found associated with the albumen.

The majority of these cases of scarlatinal dropsy end in complete recovery. No permanent damage is sustained by the kidneys. Yet the instances are not few in which the chronic form of renal dropsy, manifesting itself at some distance of time, has been distinctly traced back to its source in the acute anasarca immediately consequent upon scarlet fever. The patients carry within them a large white kidney.

It is natural therefore to expect that in the variety of febrile dropsy now under consideration, as well as in those which I formerly described, *inflammation*, and especially inflammation of the serous membranes, should not seldom be met with, and be manifested by its unequivocal effects. And it is so. But the *dropsy*, I am persuaded, has no essential connexion with common inflammation of any part, unless the state of the kidney be of that kind. I have examined the body very carefully in fatal cases, and found the serous cavities full of clear liquid, without a trace of redness or of any of the unmistakable products, or events, of inflammatory action.

The earliest threatenings of this formidable sequel demand attention. Its approach may often, I say, be detected, prior to any more obvious symptoms, by daily examination of the quality and the quantity of the urine. It is usually preceded for a day or two, or longer, by languor and peevishness; frequently by nausea and vomiting, and a costive state of the bowels. The pulse, in the outset, has been found slow, and beating with irregular intervals; but it afterwards grows frequent. The face becomes white and chuffy. Sometimes, as the disease proceeds, violent headache, drowsiness, dilatation of the pupils, convulsions, or palsy, denote serous effusion within the head. These symptoms may, however, be urœmic. Much more frequently the pleuræ are the seat of the internal dropsical accumulation, and dyspnœa is a prominent symptom. Ascites, to any considerable amount, is less frequent.

The contagion of scarlet fever is active, but uncertain. It is not so strong, nor so uniform in its operation, as that of small-pox; but it seems to be peculiarly subtle and tenacious. Fomites infected with the variolous poison soon lose their power to excite small-pox if they are freely exposed to fresh air. But the contagion of scarlet fever lurks about an apartment, or clings to furniture and clothes, for a very long time, even after some care has been taken to purify them. Of this I have known several remarkable examples. I will give you one. The disorder had attacked several persons in a large household.

When it was fairly over, the house was left empty, and then (as was supposed) most thoroughly ventilated and purified. A year afterwards the family returned to the house. A drawer in one of the bed-rooms resisted for some time attempts to pull it open. It was found that a strip of flannel had got between the drawer and its frame, and had made the drawer stick. This piece of flannel the housemaid put playfully round her neck. An old nurse who was present, recognising it as having been used for an application to the throat of one of the former subjects of scarlet fever, snatched it from her, and instantly burned it in the fire. The girl however soon sickened, and the disease ran a second time through the household, affecting those who had not had it on the first occasion. You will be asked at what period the danger of imparting the disease on the one hand, or of catching it on the other, is over; and I would recommend you to answer that you do not know. I am sure I do not: and therefore I always decline the responsibility of giving an oracular opinion on the matter. This, however, is clear. So long as desquamation in any degree is going on, so long is the patient a dangerous companion.

Till quite recently I had supposed that scarlet fever was unknown in India. I had been assured by men of large Indian experience, by Sir Ranald Martin, by Dr. Jackson, by Mr. Hewlett, who had the sanitary charge of the town of Bombay, that they had never seen nor heard of that disease throughout our Indian dominions. Rumours of its appearance, however, had of late arisen; and Surgeon Chapple, of the Royal Artillery, has set the question at rest, by a report of a series of cases of unequivocal scarlatina, which happened in the early part of the present year, at Kirkee. To Mr. Chapple also, after many years' service in India, the presence of scarlet fever there was a novelty.

Kirkee is a large artillery station, within six hours, by rail, of Bombay. On the 31st of January, 1871, 75 artillery men, 7 women, and 12 children, landed at Bombay, from the troop ship *Euphrates*. Two days afterwards they were sent, by rail, to Kirkee. Several cases of scarlet fever had occurred on board the *Euphrates*, during her voyage from England. On the 20th of February, a child was admitted into the hospital at Kirkee, with scarlet fever; on the 26th, two more children; on the 27th, another. Cases continued to occur up to the end of April, when the disease ceased to show itself. Three children died on the second day of the disease, with convulsions:

another on the fourth day. In two instances there were pain and swelling of the wrist, knee, and ankle joints. In one fatal case, of a woman, the temperature before death rose to 106°·4.

It is plain that the disease was imported from this country: and considering the previous freedom from it of that large portion of the habitable earth, the date of what may prove to have been its earliest infection is worth noting: and the truth which it helps to enforce is worth repeating; namely, that, like small-pox, measles, typhus, typhoid, and relapsing fever, scarlatina does not arise, now-a-days, except from contagion.

I may arrange what I have to say of the *treatment* of scarlet fever, according to the three varieties of it already mentioned, the scarlatina *simplex*—*anginosa*—and *maligna*. Of the scarlatina *latens*, the sequelæ alone become the subjects of treatment.

The first of these requires nothing more than confinement to the house; and the observance of the antiphlogistic regimen in regard to diet; and regulation of the bowels.

With respect to the management of the severer forms of scarlet fever, great differences of opinion have prevailed. I should recommend you to look into Dr. R. Williams's book on 'Morbid Poisons,' for some interesting and satisfactory information on this head. Satisfactory to me at least it is, because the result of it goes to justify that kind of practice which I have always considered to be the safest and the best in this disorder. You will find that it does not bear any active depletion. We have to keep in mind that the system is labouring under a specific poison, the dangerous effects of which we are to watch and, if possible, to obviate. We cannot indeed eliminate the poison from the blood; but we may promote, and even to some extent direct, its escape.

The main outlets for the multiplied poison are the throat, the skin, and the kidneys. Of these the skin is the safest. Of course, the patient must be kept in bed; and the eruption is to be encouraged, not indeed by a smothering heap of bed-clothes, but by warm baths, repeated once or twice a day, so long as the strength of the patient admits of the fatigue which they may cause him. Generally speaking, they are soothing and grateful. When the patient is too weak to bear these baths, his skin should be frequently sponged over with warm water; or he may be packed daily for an hour or more in a warm wet sheet, covered with blankets; an expedient that is feasible at all times and places, whereas warm baths cannot always be had.

This attention to the state and functions of the skin constitutes, in my judgment, the most important part of the treatment in the ordinary form of scarlet fever, the scarlatina anginosa. Should headache and delirium come on, there will be no inconsistency in applying at the same time *cold* lotions to the scalp; and the bowels should be kept open by moderate laxatives. The poison may possibly find a harmless vent by that channel. There is a consensus of testimony in favour of the carbonate of ammonia in this disease. I should therefore give effervescing saline draughts, which are usually liked, containing an excess of that salt, so that five or six grains of it in each dose may remain unsaturated by the lemon juice.

In that worst form of scarlet fever, the scarlatina maligna, all our care will too often be in vain. There appear to me two main sources of danger. The one arises from the primary impression of the contagious poison upon the body, and particularly upon the nervous system, which is overwhelmed by its force. The patients sink often at a very early period, with but little affection either of the throat or of the skin. If we can save such patients at all, it must be by the liberal administration of wine and bark, to sustain the flagging powers until the deadly agency of the poison has in some measure passed away. But another source of danger arises from the gangrenous ulceration which is apt to ensue in the fauces, when the patient is not killed by the first violence of the contagion. The system is *re*-inoculated, I believe, with the poisonous secretion from the throat. Now under these circumstances also, quina, or wine, and upon the whole I should give the preference to wine, are to be diligently, though watchfully, given. And something may be done, by way of gargles, to correct the state of the throat, and to prevent the distressing and perilous consequences which would otherwise be likely to flow from it. The *liquor sodæ chloratæ* diluted may be employed for this purpose; or, what I believe to be better still, a solution of the nitrate of silver. If the disease occur in a child that is not able to gargle, this solution may be injected into the nostrils, and against the fauces, by means of a syringe or elastic bottle: or a little mop, charged with the same solution, may generally be used without much difficulty. The effect of this application is sometimes most encouraging. A quantity of offensive sloughing matter is brought away; the acrid discharge is rendered harmless; the running from the nose, and the diarrhœa, cease; and the disease is converted into a form which approximates to the scarlatina anginosa. This is a great improvement upon the old plan of ordering capsicum gargles.

Of late I have been in the habit of directing a solution of the chlorate of potass in water (a drachm to a pint), as a *drink* for patients in scarlatina, as well as in typhus fever. This practice was suggested to me by Dr. Hunt, who tells me he has long employed it with advantage. Under the use of a pint, or pint and half, of this solution daily, I have remarked, in many instances, a speedy improvement of the tongue, which, from being furred, or brown and dry, has become cleaner, and moist.

From several distinct and highly respectable sources, *chlorine* itself has been strongly pressed upon my notice, as a most valuable remedy in the severest forms of scarlet fever. My informants have stated, that whereas they formerly dreaded to be summoned to cases of that disease, they now, having had experience of the virtues of chlorine, felt no misgivings in undertaking its treatment. Since these representations were made to me, I have not had opportunities enough of trying this drug to enable me to speak confidently of its sanative power: but I shall certainly employ it in future. I presume that its disinfecting properties may, in part, account for the good it does. It probably deprives the foul secretions of their noxious quality.

In the fourth volume of the 'Medical Gazette,' Messrs. Taynton and Williams, of Bromley, write in high praise of this remedy, and give a formula for its preparation.

You may make it, for extemporaneous use, in this way.

Put eight grains of the chlorate of potass into a pint bottle, and pour upon them one drachm of strong hydrochloric acid. Keep the mouth of the bottle closed until the violent action has ceased; then add an ounce of water, and shake the mixture well; then add another ounce of water, and again agitate well; and so on until the bottle is full. The chlorate should be pulverised; and in cold weather the bottle should first be warmed.

A tablespoonful, or two, of this mixture, according to the age of the patient, may be given for a dose, frequently. An adult may take the whole pint in a day.

We must remember, in this, as well as in the other forms of the complaint, to pay attention to the state of the bowels, and by no means to allow them to remain costive.

When the patient is at length convalescent, he will require careful watching till that period has gone by at which the dropsical symptoms are apt to appear. Very often it is by neglect or imprudence that these symptoms are brought on. The patient should be kept long in bed, and sedulously protected afterwards from all exposure to cold, wet, or fatigue; indeed he ought not to be permitted to go out of the house until the process of desquamation is fairly over; and I would not willingly let a patient go out till some little time *after* this. When dropsical symptoms *do* occur, if they be very slight, they may be removed in general by purgatives, and by digitalis. These patients are always pallid and exsanguine; for this reason, and still more because that remedy has been found actually serviceable in such cases, I would advise a combination of the muriated tincture of iron with the tincture of foxglove. Diaphoretic remedies, too, are beneficial; and in aid of them, or rather as one of the most efficient of them, the warm bath, which may be repeated every night. And inasmuch as we know that the kidneys are at least congested in all these cases, I would, if the urine were scanty, take a small quantity of blood from their neighbourhood, by leeching or cupping the loins. At the same time I should make use of the warm water, or hot air, or blanket, bath; and wrap linseed poultices or warm fomentations round the loins. Upon the same grounds I should refrain from prescribing stimulating diuretics: but the *Imperial drink* given liberally is a harmless and a useful diuretic.

But if there be any indication of *inflammatory* disease of any other internal part, we must adopt appropriate measures. We have not, *now*, so much to contend with the depressing influence of the original poison, as to dread the consequences of acute inflammation; or of the sudden effusion of fluid, the mere presence and pressure of which may fatally oppress vital organs. We should have for our object to arrest the inflammation—or to promote the removal of the effused fluid. The worst case of this kind that I ever witnessed occurred, many years ago, in a boy of fifteen, the son of a tradesman in my neighbourhood. He had had scarlet fever, *mildly*, and had got well, or nearly well, of it, as he believed; and he went, one evening, into his father's stable, and staid there some time in the cold, during the period of desquamation. A day or two afterwards he began to have headache, and in a few hours more was seized with convulsions of one side of the body, coma, and at length hemiplegia; and his face and extremities became at the same time anasarcous. Blood was taken from his arm, he was cupped on the temples, and took mercury; and in a short space of time was salivated. Under this treatment the coma and dropsy rapidly disappeared, and he presently recovered the use of his palsied limbs, and got quite well. I conclude that some effusion took place within the cranium, as well as into the subcutaneous areolar tissue.

The *prevention* of scarlet fever is a matter of deeper interest than even its successful conduct. This is true of disease in general, but it is emphatically true of this wide-spread and wasting pestilence. Valuable suggestions on this subject have been made by Dr. William Budd, and by the Association of Medical Officers of Health in the Metropolis.

So far as they may be practicable, the following rules should be strictly observed.

Upon the first appearance of the disease in a house, measures should be taken for impounding the poison; in order, first, thus to limit the sphere of its contagious power, and, secondly, the more easily and effectually to disarm and destroy it. The source therefore of the poison, in other words, the suffering patient, should at once be isolated. The room assigned to him should be inaccessible by other inmates of the house, except those who supply his wants; and these should be persons who have themselves passed through the disorder. They who have not done so had better be removed from the house. The room should be stripped of all needless furniture; especially of carpets, hangings, and all woollen or other draperies which might harbour the poison. The attendants should select glazed or smooth dresses. Rags, instead of pocket-handkerchiefs, should be used by, or for, the patient, and burned as soon as used. A basin containing some disinfecting fluid should be placed near the patient, for him to spit into: and a large vessel, holding water impregnated with chlorides or Condy's fluid should be ready for the immediate reception of changed bed and body linen. All the excreta of the patient should be received into vessels charged with disinfectants. The hands of the nurses, and all glasses or cups used by the patient should be diligently and frequently washed. As soon as a whitish efflorescence on the skin denotes the commencement of desquamation, Dr. William Budd recommends that the patient's whole body, his scalp included, should be anointed twice a day with camphorated olive oil. This is to prevent the pollution of the air of the chamber—and of the crevices in the walls or bedstead—

with contagious fragments of cuticle. The wisdom of this advice is evident; especially if, as Dr. Gee tells us, upon uncovering a scarlet fever patient in the direct rays of the sun a cloud of fine dust may be seen to rise from his body. Afterwards, while convalescence is in progress, the patient's body should be washed daily with carbolic acid soap till the peeling is quite over. All towels, and the like, should be steeped, before they leave the room, in boiling water, containing in each pint a teaspoonful of the liquor sodæ chloratæ, or of Condy's fluid.

There will still remain, after the prisoner is at length released, the disinfection of his room. The floors, ceiling, and walls should be washed or repapered. For fumigating infected rooms, and their contents, there is nothing better, say the 'Association,' than sulphur. 'A quarter of a pound of brimstone, broken into small pieces, should be put into an iron dish (or the lid of an iron saucepan turned upside down), supported by a pair of tongs over a bucket of water. The chimney and other openings are then closed with paper pasted on, and a shovelful of live coals is put upon the brimstone. The door is then quickly shut, the crevices covered with paper and paste, and the room kept closed for five or six hours. After this a thorough cleansing should be effected; everything washable should be washed, and all other things be cleansed by proper means.'

You are probably aware that *belladonna* is believed by many to exert a preventive and protecting influence upon the body against the contagion of scarlet fever. Hahnemann, the author of the Homœopathic hypothesis (and thereby of much mischief to mankind), was the first to assert this. The notion was evidently suggested by that hypothesis; for belladonna, administered in small doses, sometimes produces a scarlet efflorescence on the skin, and certainly tends to cause dryness and redness of the fauces. This is but a poor foundation on which to rest its prophylactic power. To test that alleged power is not very easy. Other precautions are commonly employed at the same time; there are great natural differences in different individuals with respect to the susceptibility of the contagion of scarlet fever; the prepared extracts of belladonna are not seldom worthless and inert. The conservative property, however, of that vegetable has, in my opinion, been completely disproved by the trial of it made in George Watson's Hospital, in 1851, by Mr. Benjamin Bell. Scarlet fever having appeared within the building, belladonna was given to fifty-four healthy boys;

at first in doses which caused dilatation of their pupils, and impaired vision. The drug therefore was not inert. After this plan had been in operation for a month—after full time allowed, therefore, for the development of the protecting influence of the belladonna, if it really possessed any—twenty-three of these fifty-four boys took the disease.

It is sometimes prescribed, by men who have little or no faith in its preventive virtues, in order to give confidence to those employed in nursing the sick, and comfort to anxious parents. But in proportion to the confidence thus produced will be the risk of the neglect of other and better safeguards. For my own part, totally discrediting the defensive power ascribed to the drug, abominating all shams, and believing that so poisonous a substance can scarcely be taken for some time together, even in small doses, without prejudice to the general health—I not only never propose it, but I think myself bound to state plainly my opinion of it, whenever its use is proposed to me.

A febrile exanthem was epidemic in London in 1863–64, the symptoms of which bore a hybrid resemblance to scarlet fever on the one hand, and measles on the other; showing the sore-throat of the first and the eruption of the second. By some it was named *Roseola*, by others *Rubeola notha*. The disorder was mild in character, and in no instance fatal that I know of. It required care and watchfulness rather than any active medication.

LECTURE LXXXII.

The Plague. Erysipelas. Erythema nodosum. Urticaria. Prurigo. Scabies.

Of that group of *contagious* exanthemata of which I undertook to give you some general account, two only remain to be noticed; viz. the plague and erysipelas.

Concerning one of these, the plague—as I have never seen, and hope never to see it; and as, with Cullen, I 'think it unfit for a person who has never seen the disease to attempt its particular history'—I shall not presume to offer you any observations in detail. It is a very malignant kind of contagious fever; prevailing, at certain times and places, epidemically; attended with a sort of eruption, namely with buboes and carbuncles; and not furnishing, apparently, any sure or permanent security against its future recurrence. In Sir J. Forbes's 'Select Medical Bibliography' you will find a long list of works on the plague. I would second Dr. Cullen's recommendation, that you should consult those authors only who have themselves had personal experience of the disease. Among the publications that fall within this rule may be mentioned Dr. Russell's History of the Plague as he saw it in Aleppo; Sir James M'Grigor's Medical Sketches; Sir Arthur Brooke Faulkner's Account of the Plague which occurred at Malta in 1813; Desgenettes' Histoire Médicale de l'Armée d'Orient; and Assalini's description of the malady as he witnessed it when in attendance upon the French army in Egypt.

I proceed, therefore, to *erysipelas*. And I wish, in the first place, to fix and define that specific complaint of which *alone* I propose at present to speak. The term erysipelas has been employed by medical men in a very loose and vague manner. Any diffused redness and inflammation of the skin is apt to be set down as erysipelas; and hence we have disputes as to the distinction between *erysipelas* and *erythema*. But it would tend, in my humble judgment, to the formation of more settled opinions with respect to erysipelas, if the term were restricted to that disease in which the integuments of the *face* and *head* become diffusely inflamed. The phrase *erysipelatous inflamma-*

tion may properly enough be applied to other cases, similar to this in so far as the condition of the *skin* is concerned; but in what I should consider true erysipelas, in the medical sense of the word, there are other characters belonging to the disorder quite as important as, and more distinctive than, the cutaneous affection. What is usually spoken of as erysipelas of the face and head, and what I would call simply erysipelas, falls naturally within that group of exanthematous diseases which includes small-pox, measles, scarlet fever, the plague, and continued fevers. It is an idiopathic inflammatory disorder, running a *tolerably regular and definite course*; attended by inflammation of the integuments of the body, or in other words by an *eruption*; often *prevailing epidemically*; and capable of being *communicated*, under circumstances favourable to its propagation, from one person to another. Its power to protect the constitution from its own recurrence is less certain; but in truth, so many different affections have been lumped together under a common name, that the proper phenomena of true erysipelas have not been made sufficiently an object of separate study to enable us to speak with any confidence on this point. I recollect, however, a certain woman who has been three or four times my patient in the hospital with erysipelas: and one of the night nurses there, whom I treated for that complaint some time ago, is now lying ill of the same disorder under the care of one of my colleagues.

Erysipelas, in the sense now explained, called in Scotland *the rose*, and in this country *St. Anthony's fire*, resembles other disorders of the same group in these points also, that the fever *precedes* the local inflammation, that certain premonitory symptoms frequently go before the outbreak of the disease, and that *sore throat* is an early, and almost a constant, accompaniment of the complaint. The patient feels ill—shivery, feeble, languid, and often drowsy. The actual attack generally sets in with distinct rigors; and the pulse is often very frequent from the first, for many hours, perhaps, before the redness commences. Very commonly there is also manifest disturbance of the alimentary canal, marked by nausea and vomiting, and not unfrequently by diarrhœa. Then some part of the face, usually one side of the *nose*, or one *cheek*, or the rim of one of the *ears*, begins to feel hot, stiff, and tingling: and upon examining it you find it to be of a deep continuous red colour, and to be swelled and hard. The redness and swelling gradually, and sometimes rapidly, extend themselves: they are defined by a distinct elevated margin; which advances, and invades progressively the neighbouring healthy surface, until the whole of the face, or of the scalp, or of both, is occupied with the inflammation. The lips swell enormously, the cheeks enlarge, the eyes are sealed up by their œdematous and prominent lids, and all traces of the natural countenance are effaced. I know of no disease, except perhaps the confluent small-pox, by which the human face divine is so completely and speedily deformed and disguised. A stranger seeing a young female in the height of the disorder, and revisiting her after her recovery, is astonished at the change. It seems as if, by some magic process, such as we read of in our nursery tales, a hideous monster has been metamorphosed into a comely damsel.

The inflammation frequently spreads from the face and forehead, or ears, to the hairy scalp; and from the head it travels backwards, in some cases, to the neck and the shoulders. Sometimes—and in this it exactly resembles a scald—the inflamed surface becomes covered with irregular bullæ, or blisters; but often there is *no* vesication. This circumstance, therefore, which has been mentioned by some as furnishing a point of distinction between erythema and erysipelas, fails of that purpose.

In many cases the inflammation is quite superficial: in others it dips, as it were, through the skin, and affects the subcutaneous areolar tissue; and then, and there, suppuration, and even sloughing of that tissue, are apt to take place. We find this to be the case often in the loose tissue of the eyelids; and it is more common on the scalp perhaps than on the face.

After the redness has lasted three or four days, it fades, the swollen surface subsides, and desquamation ensues; and as the inflammation creeps, perhaps, gradually from one part of the surface to another, you may find the face becoming pale, and covered with patches of dead cuticle, while the scalp, or the upper part of the neck, is becoming red. Sometimes those parts of the inflamed surface on which blisters had formed are covered with *crusts*, rather than with merely dead and dry cuticle. In almost all these cases of erysipelas of the head and face, there will be found to be redness and soreness of the *throat* also; although this is not always inquired into, or complained of.

There is considerable variety in the intensity and complication of the symptoms. Sometimes the sufferer lies patiently still, yet apparently conscious and rational, till the tumefaction diminishes, and he is again able to open his eyes. Generally there is some wandering of the mind, especially at night; and in bad cases there is much delirium, and at length complete coma, and the patient dies at the end of a few days. In some of these cases the inflammation has extended to the encephalon; in others it is probable that the functions of the brain are disturbed through the febrile derangement of the circulation. When death takes place, and the head is examined, serous fluid is usually discovered beneath the arachnoid, or in the cerebral ventricles; and the veins of the pia mater are turgid. I have stated before that I doubt whether such appearances are always to be attributed to inflammation. Sometimes there are no morbid appearances at all within the skull.

It is said that the erysipelas does, now and then, suddenly desert the surface; and that inflammation of some internal part, and particularly of the brain, is apt to follow such rapid subsidence of the external malady. I presume that this metastasis is rare. I do not recollect to have seen it. But the *extension* of the disease, the supervention of delirium and coma, while the external inflammation *continues*, is of common occurrence.

This, then, is one way in which erysipelas is accustomed to prove fatal; by effusion within the head, and *coma*.

And there is another mode in which death is not unfrequently brought about, and which has not been so much attended to; I mean by the affection of the throat. The patient dies sometimes almost suddenly: unexpectedly; you cannot account for the unlooked-for dissolution. But if the throat be examined, you may (sometimes at least) there discover the solution of the mystery. The sub-mucous tissue of the glottis and epiglottis is filled with serum, or pus, the chink of the larynx has been nearly or completely closed; and the patient has died of *apnœa*. This is just analogous to what takes place externally: the enormous swelling of the eyelids, and lips, and face, is owing, in a great degree, to serous fluid poured out into the sub-*cutaneous* areolar membrane.

Another way in which erysipelas may kill, is by gradual *asthenia*. Without any stupor or much wandering, without any marked affection of the breathing, the pulse becomes weaker and weaker, the surface cold, and the heart at length ceases to pulsate. This mode of dying is less common in this disorder than the other two.

The causes of erysipelas are various, and often obscure. I have stated that it is communicable, by contagion, from person to person: yet this contagious property is so feebly marked, that it is denied by many. It is more active at certain times, at certain seasons, at certain places, than at others; which is the same thing as to say that there are *predisposing* causes of the disease; that there are influences which augment the susceptibility of the body to the agency of the poison.

I believe that on the Continent they do not allow erysipelas to be contagious at all: but very satisfactory evidence of the fact has been collected by several of our own practitioners. In the second volume of the 'Transactions of a Society for the Improvement of Medical and Chirurgical Knowledge,' Dr. Wells has brought together several examples in which the complaint appeared to be unequivocally propagated by contagion. 'I visited (says he), on August 8, 1796, in Vine Street, Clerkenwell, an elderly man, named Skelton, who had been attacked several days before with *erysipelas of the face.* In about a week afterwards he died. On the 19th of the following month, I saw a Mrs. Dyke, of about seventy years of age, the landlady of the house in which Skelton had been a lodger, and found *her* labouring under an erysipelas of her face. I inquired whether any other person in her house had been ill of the same disease since the death of Skelton, and was told that his wife had been seized with it a few days after his decease, and had died in about a week. During my attendance upon Mrs. Dyke, an old woman, her nurse, was attacked with the same disorder, and was sent to her parish-workhouse, where *she* died. Mrs. Dyke has since informed me that a young man, the nephew of Skelton, was taken with the disease of which his uncle had died, shortly after visiting him, and survived the attack only a few days. That she herself had been several times with Skelton and his wife during their sickness, and after their death had removed some furniture from the room they had occupied to her own apartment.' Dr. Wells relates other histories of the same kind, all occurring when there was no particular *epidemic* of erysipelas prevailing to account for them. Professor Arnott has given some other examples, still more striking, of the propagation of erysipelas from one person to another, not only under the same roof, and in the same locality, but also when the parties lived at a distance from each other, and the intercourse between them had been casual and temporary. These cases are stated, I think, in the fifty-seventh volume of the 'London Medical and Physical Journal.' The following incident has been told me upon good authority. A man living somewhere in Westminster fell ill of idiopathic erysipelas. In that state, for some reason or other, he was removed thence; and his brother, who was a servant in or near Portland Place, received him clandestinely into his master's house, and allowed him (for two nights, I believe) to share his bed. That brother was soon attacked with erysipelas; and in the course of his illness was visited by his master. The master also was attacked; and it is worthy of remark, that in both master and servant, the disease showed itself just seven days after they had respectively come near another who was affected with it. Dr. Elliotson gives an account of having suffered the disease in his own person. It began in him five days after the breath from one of his patients, over whom he was stooping to examine the skin, and who had erysipelas badly, and died of it, had come upon his face. 'I turned away (he says) disgusted, and said, *I hope I have not caught it*; but five days afterwards, having forgotten the circumstance, I was seized with it.' More than once I have had occasion to remark that successive tenants of the same bed in a hospital have been seized with erysipelas after their admission.

But allowing, as I think we cannot but allow, that contagion is one of the exciting causes of erysipelas, there are others which *more frequently* excite it. At least there are many instances of the disorder in which we can trace no exposure to contagion, and in which we can perceive some other probable reason for its occurrence. Sometimes, no doubt, it comes on without any obvious cause. The application of cold often gives rise to it. Irregularity of diet is said to do the same. Violent mental emotions are also accused of being occasionally its cause: it is said to have been brought on both by anger, which is an exciting, and by fear, which is a depressing, passion. It is incidental to what I have called purulent infection of the blood, and in all probability there has been a previous morbid condition of the blood in all those instances of the disorder which appear to be produced by exposure to cold, or by mental agitation. Many cases, not to be distinguished in their appearances and effects from idiopathic erysipelas, result from local injury.

I say that erysipelas further resembles the disorders of the group in which I have placed it, in that it sometimes prevails, *epidemically*: and on those occasions, like the rest of the group, its occurrence is promoted by all circumstances that tend to debilitate the body: by intemperance; by previous disease; by low spirits and anxiety; by insufficient nourishment; and by foul air. It used to be much more common formerly in hospitals than it is at present; when less attention was paid to cleanliness and ventilation.

In general the temperature, as ascertained by the thermometer, rises much and fast at the outset of the disease—although the sensations of the patient may be those of chilliness. The heat may reach 104°, or even more, in a few hours. So long as the cutaneous inflammation continues and spreads, the temperature increases, and may attain 106°. Defervescence, when it arrives, is usually rapid, so that the normal standard may be reached, or nearly so, in twelve hours. Occasionally the temperature undergoes a remittent sinking. Should any fresh or striking rise of temperature happen, it may be regarded as an accompaniment, or the herald, of a new extension of the cutaneous inflammation.

When erysipelas takes an erratic form, these repetitions of higher temperature are the most apt to occur. In fatal cases the death takes place with very high temperatures.

Erysipelas is another of the diseases concerning the *treatment* of which there has been, and perhaps there may still be, a most embarrassing difference of opinion. When a student many years ago at St. Bartholomew's Hospital, I observed that in the physicians' wards nearly every case of erysipelas was treated at once with bark and wine; in the surgeons' wards nearly every case with depletion and tartarized antimony. Recoveries took place under both methods; and mutual sneers were not wanting. However puzzling this opposition of opinion was to me at the time, I have since learned to side with the physicians. Their practice was indeed consistent with the lessons that had long been inculcated by careful observers, of great sagacity and experience. Dr. Fordyce, Dr. Wells, Dr. Heberden—all recommended the treatment by bark: or as we should now say, by the sulphate of quina. Dr. Jackson, an eminent American physician, advocates, I see, the same plan. He says, that after a purge, and, if necessary, an emetic, the sulphate of quina should be given in as large doses as the patient will bear; that from twelve to twenty-five grains in the twenty-four hours will generally suffice; and that we know when the dose is sufficient by a buzzing which comes on in the ears. Dr. Elliotson also—whom I quote the more readily because he was rather of an antiphlogistic turn than otherwise, in general—states in his

Lectures that he never saw quina do harm, even in active tonic erysipelas; and that in doubtful cases, when you might hesitate whether to bleed and put the antiphlogistic plan in force, or to stimulate and support, the quina is *always* a safe and *eligible* medicine. Dr. Robert Williams, of St. Thomas's Hospital, thought better still of *wine*, which he gave in *all* cases *from the very beginning*.

But we have evidence as strong, or stronger, in favour of another drug. For many years past the tincture of the sesquichloride of iron has been largely used, and with the best results, in the Middlesex Hospital, both in this disorder and in others of a like asthenic character, severe whitlows, carbuncles, and bad sore-throats. The medicine is given in drachm doses mixed with water, every three hours at first, without any regard to the condition of the tongue, which often becomes clean under its use. Within twenty-four hours the state of the patient undergoes, generally, a striking improvement; he feels in better heart and spirits; and he often misses the supporting influence of the medicine, and asks to have it again, when it has been intermitted.

Dr. Russell Reynolds writes in high praise of the same remedy; and believes that its occasional failure is due to the smallness and infrequency of the dose; that ten or fifteen minims given three times a day are useless. 'The effects of this medicine (he says) may be seen sometimes after the first, often after the second dose: the local inflammation ceases to extend; the inflamed part becomes paler, less tender, less swollen; the feeling of exhaustion is diminished, and with it such symptoms of exhaustion, as exaggerated frequency of pulse, dry brown tongue; the temperature falls; and sleep frequently ensues. As soon as such changes take place, the quantity of the tincture may be reduced.'

We owe our knowledge of the value of this drug, thus used, to Dr. G. Hamilton Bell, who in 1851 published the results of his experience with it, in the 'Transactions of the Medico-Chirurgical Society of Edinburgh.' Two years later Dr. Balfour, after treating twenty cases on this plan, expressed in the 'Monthly Journal' his belief that we have now 'a certain and unfailing remedy, whether the erysipelas be infantile or adult, idiopathic or traumatic.'

But it is not every case of facial erysipelas that requires this heroic treatment. Taking the disease as I see it in London, I should say that many cases do well, with but little care or interference; that many also, but a much fewer number than the former, prove fatal under whatever plan of treatment may be adopted; and that many patients are to be saved by judicious management who would otherwise die. It is for the rescue of these perilous yet recoverable cases, that the iron treatment is to be commended.

Trousseau's rule was to abstain from every kind of treatment in erysipelas of the face. He even boasts of the success of the *expectant* plan, which he says he had followed for twenty-eight years, and could not recollect having lost three patients during that period. We may pronounce him lucky. He kept his patients in bed, and fed and supported them liberally.

In all cases it will be right to empty the bowels at the commencement. A dose of neutral salts, or of rhubarb and magnesia, answers better I think, in these cases, than calomel and senna. If you see the patient very early; and if there be any nausea or oppression of stomach, it will be proper to give an emetic.

Various external medicaments have been used and recommended for the inflamed part. Putting leeches upon it; puncturing it with needles, or lancets; forming round it a cordon with the lunar caustic in substance or in strong solution; dusting the surface over with magnesia, or with flour; smearing it with various unguents, and particularly with mercurial ointment; covering it with flexible collodion; keeping it wet with some cold lotion; or fomenting it with hot flannels.

Now, of all these local appliances, that which, according to my own observation, is the most useful, and which affords the greatest comfort to the patient, is the last that I mentioned; fomentation by flannels wrung out of a hot decoction of poppy-heads. But in order to give this fair play, it should be *continual*: not used for half an hour, and then intermitted; but it should be one person's business to apply the fomentation assiduously, so long as it is soothing and grateful to the patient. The local treatment most in favour with our apothecary at the hospital is that of covering the inflamed face and head with flour, by means of a dredging box. The patients declare that the flour cools, soothes, and comforts them. This is a more convenient, and, in some respects, a more eligible application than that of hot flannels. It is less likely to fail of its purpose through the negligence of the nurse.

So much for erysipelas, as it usually comes under the notice and the management of the physician. You are aware that an affection of the skin very similar to that which I have been describing, and called also by the same name of erysipelas, is very common in other parts of the body; on the extremities especially, and occasionally on the trunk: and it will travel sometimes from an extremity till it reaches the head. These varieties of cutaneous inflammation are, in most instances, the indirect consequences of some local injury: of punctured wounds; of the stings of insects, or the bites of venomous reptiles; of mere scratches sometimes. Or the cutaneous inflammation will spread from old sores; or supervene upon dropsical limbs. It is curious that these complaints also are much more apt to occur, and even to multiply by a sort of contagion, or in virtue of some epidemic atmospheric influence, at certain times and places, than at others. You will find that there are periods when the surgeons of hospitals dread to perform any operation, lest it should be followed by this spreading inflammation of the skin. There are many points of great interest connected with these diversified forms of what is called erysipelas: they are more liable to be attended with gangrene than erysipelas of the face and scalp: on the other hand, they are more liable also to be complicated with inflammation of the subcutaneous areolar tissue, and with suppuration; and to require incisions to relieve the great tension of the inflamed parts, and to facilitate the escape of the pus, or of sloughy dead portions of areolar tissue: but all these matters belong rather to surgery, and have been discussed, I make no doubt, by the Professor of surgery.

Very closely connected with erysipelas, and continually confounded with it, is *erythema*. It also consists in superficial redness of some portion of the skin; but it is not attended with inflammation of the areolar texture under the skin: nor with vesication; nor, in general, with fever; nor is it peculiar to the face and head.

There are numerous varieties of erythema described by writers on cutaneous disorders, to whom I must refer you for an account of them. Willan and Bateman; Wilson; Willis; Rayer; Alibert; and Biett, as his practice and lessons are reported by two of his pupils.

The only variety on which I am disposed to say a word, is one which is attended with more or less febrile disturbance. I mean what is called erythema *nodosum*.

This curious affection occurs much more often in young women than in any other persons: sometimes in feeble boys.

The eruption is commonly preceded for a few days by indisposition, and some slight degree of fever. Then red elevated spots come out, on the fore part of the legs, and occasionally, but very rarely, on the arms. The redness appears in oval patches, of which the long diameter is parallel to the axis of the limb. They are pretty large patches, an inch and half long, and an inch broad perhaps, and they evidently project and form bumps upon the anterior surface of the leg. From their look, you would suppose abscesses were about to form; but after lasting a few days, the red colour fades, or rather changes to a blue, and the protuberances gradually subside. This eruption seems sometimes connected with disturbance of the menstrual functions. Rayer has seen it occur in connection with acute rheumatism. So have I. A patient of mine in the hospital was attacked with acute rheumatism of the joints immediately upon the cessation of erythema nodosum. In another this order was reversed.

Now I am persuaded that, after an aperient, *rest, the horizontal posture*, and *quina*, constitute the proper treatment of this affection. I had once a housemaid in whom the disorder appeared, and was attended with unusually high fever, and much indisposition. I treated her, therefore, antiphlogistically; i.e., I kept her on low diet, and gave purgatives; but the disease went on. Fresh knots came out as the old ones faded. At length, I do not remember why, I prescribed some quina for her; and the improvement was immediate, and very striking. She relapsed, however, once or twice, upon leaving off the bark; but by persisting subsequently in its use for some days after she appeared to be well—a permanent cure was effected. Since that time—now nearly twenty years ago—I have seen a good many examples of erythema nodosum, and I have treated them all alike; viz., first with an aperient, and then with the sulphate of quina; and they have all rapidly got well. Probably they would have recovered nearly as soon under some other tonic treatment; but I have been so well satisfied with this, since I began it, that I have felt no temptation to try any other.

There is a rash which is well known, and very tormenting, and therefore, not without interest, although it is almost always without danger: I mean *urticaria*. It is arranged by Cullen among the exanthemata; but it does not properly belong to the group to which I would restrict that name, for it is not contagious, and it may happen to the same person a hundred times over. The eruption consists of what, from analogy, are called wheals: i.e., of little solid eminences, of irregular outline, but generally roundish or oblong, and either white or red, or (which is most common) both red and white; the whiteness occupying steadily the central and most projecting part of the spot, or becoming manifest there when the integuments are put upon the stretch. The rash is accompanied with intense heat, a burning and tingling in the affected spots, and great itching and irritation. In truth, both the appearances upon the skin, and the sensations that attend them, are very much like the appearances and feelings produced by the stinging of nettles. Hence its trivial name, *nettle-rash*: which is, indeed, the same, in meaning, with the scientific appellation, *urtica* being the Latin for a nettle. Similar appearances follow almost immediately upon a smart blow with a cane, or with the lash of a whip, on the skin. Red stripes or *wheals* arise, and within the reddened surface one or more elevated spots of a white colour are visible.

There are two varieties of urticaria: one in which the complaint runs a short course, and soon subsides, and may be considered acute: another in which it is chronic, and either persistent or intermittent. The acute form is attended with feverishness, which sometimes begins two or three days, but commonly not more than a few hours, before the eruption appears; or the fever and the rash may commence together. In most cases, perhaps in all, the disorder is intimately linked with some derangement, manifest or latent, of the stomach, and it may often be traced to the operation of particular articles of food. It is very curious that the introduction of certain substances into the alimentary canal should affect the external tegument precisely in the same manner as the virus of the nettle, and some other irritants, when these are applied to the skin itself. The stomach may be healthy, yet incapable of digesting some particular substance, which then becomes, in fact, a poison. The skin is probably affected by the actual presence of the irritant which has entered the circulation: so that urticaria is simply a case of mild blood-poisoning. The offending articles of food do not produce the cutaneous affection in all persons, nor even necessarily in the same person at all times. But there are some edible substances which are much more likely than others to be followed by nettle-rash. Certain vegetable matters are very apt to excite the disorder in some persons: oatmeal; almonds, especially the bitter almonds; any bitter kernels; particular species of strawberries; raw cucumbers; mushrooms. Some of the vegetable substances used in medicine are known to have, frequently, the same effect; capivi, for example; the cubebs pepper; valerian. Urticaria has been brought on by drinking porter, or, most probably, by some of the drugs with which our porter is sophisticated. These effects are not confined to vegetable substances. Shell-fish is a common source of nettle-rash. I have known it to be occasioned by prawns; crabs sometimes have the same unpleasant consequence; and muscles still more often. An hour or two after some one of these substances has been swallowed, and perhaps much sooner, nausea is felt, and oppression about the epigastrium; the patient becomes giddy, his face and head sometimes swell, his skin begins here and there to burn and tingle, and presently the eruption, as I have already described it, breaks forth. It is attended with intolerable itching and pricking sensations, especially at night, when the patient is warm in bed; or when the affected surface is exposed to the air. Vomiting and diarrhœa often supervene, and prove the natural cure of the attack.

We read that this disorder has sometimes proved fatal; but this must be under very unusual circumstances of weakness in the patient, or of some peculiar virulence in the exciting cause.

The chronic form of the complaint is apt to be very obstinate and teasing. It comes and goes, and comes again. The evening is one of its favourite periods. In those who are subject to it, the itching and the wheals are readily brought on by scratching or rubbing the surface. This is the urticaria *evanida* of Willan. Dr. Heberden had known persons afflicted in this way for ten years together. I have observed nettle-rash to occur in connection with sudden and violent paroxysms of dyspnœa, resembling asthma fits; so that I could not help suspecting that the mucous membrane of the respiratory passages was irritated after the same fashion with the external skin.

Even the chronic variety of urticaria is, in some instances at least, produced by certain *ingesta*; and the peccant substance may often by detected, and the tiresome malady be cured, by following the simple and judicious plan recommended by Willan; namely, that of instructing the patient to abstain, for a while, from all his customary articles of diet, one by one, in their turns. This experiment does not, indeed, always answer. The urticaria will sometimes abide, notwithstanding: so that although it probably depends in all cases upon some disordered condition of the stomach or bowels, we cannot say that such disorder is *always* the consequence of something that has been swallowed.

The treatment to be adopted in the acute or febrile nettle-rash, when it depends upon something recently received into the stomach, is that which common sense would suggest, and which nature often plainly indicates. We seek to expel the offending material by an emetic, and by purgatives: and this being done, the cure is completed. In the more chronic and recurring varieties, we endeavour, in the first place, by making the experiment recommended by Dr. Willan, to detect, that we may thenceforth interdict, any article of diet which may have caused the disorder. If we fail in this attempt, our object must be to correct that faulty state of the digestive organs, or to neutralize that inbred poison, upon which the cutaneous affection depends. Laxatives and antacids are found to be the most successful means of attaining these ends. They may be given together, or separately. A few grains of rhubarb taken daily just before breakfast, and just before dinner, have cured a chronic case of long standing. Or rhubarb and magnesia may be taken together; the carbonate and sulphate of magnesia; castor oil. The snake-root has obtained some repute as a remedy for urticaria. You may prescribe, therefore, if you please, a scruple each of the carbonates of magnesia and of soda, in the infusion of serpentaria.

External applications seem to be of but little avail in this disease; and those which do appear to be of service, act uncertainly, and produce different effects in different persons. The warm bath sometimes gives ease in the severer cases. In the more chronic form of the disorder, spirituous washes, vinegar, sea-bathing, are things to be tried. And cases are related in which, when every other expedient has failed to give permanent relief, removal to a warmer climate has been successful. Dusting the itching surface with flour has, in my experience, afforded much temporary comfort. Still more useful perhaps is a lotion (first recommended by Wilkinson, in a little work on skin diseases) composed of a drachm of the carbonate of ammonia, a drachm of the acetate of lead, and eight ounces of rose water. Its efficacy may be increased by the addition of half an ounce of laudanum.

In a short paper in the 'Guy's Hospital Reports,' Dr. Gull seeks to determine the actual nature of the marks presented in urticaria. Willan had described a form of the complaint which may be excited in a few seconds by strong friction or scratching. Dr. Gull calls this *factitious* urticaria. A great facility of accepting this mechanical effect upon the skin exists in certain persons, and apparently runs sometimes in families. In such persons, a wheal of almost any shape can be artificially made upon their healthy skin. 'If a line (says Dr. Gull) be traced with slight force on the skin, the first noticeable change is a wrinkling of the surface, as in *cutis anserina.* In forty seconds there is a slightly raised red line; in sixty seconds it is palpably raised and hard; in ninety seconds there is an obvious wheal. If a large space be rubbed, there is a sensation of tightness and stiffness, as if the part were hide-bound. If two points be marked on the skin previously to the friction, they are found nearer together after the wheal has risen. With the rising of the wheal, which is white and firm, there is an accompanying areola of capillary hyperæmia, which after some minutes (fifteen or twenty-five) disappears, leaving the wheal for a longer time persistent.'

Dr. Gull concludes that these phenomena are principally due to *contraction of the muscular tissue of the skin.* It seems corroborative of that opinion, that after dropping chloroform on the skin no wheal could be brought out by friction; and when chloroform was applied to a wheal already risen, it quickly reduced it. When ice was applied to the part immediately after friction, a wheal did not rise.

'How a wheal is excited in ordinary urticaria, whether directly by the circulation of some irritating substance through the cutaneous tissues, or indirectly by reflexion through the vaso-motor nerves, or indifferently by both modes, is not proved. Admitting that the blood is the more common channel, it is plain that it is not the exclusive one.'

Prurigo—itching—is a cutaneous affection bearing some analogy to urticaria, at least in the sensations which accompany it. And a most terrible and melancholy affection it often proves to be. Sometimes the parts of the skin which are the seat of the itching do not present any perceptible deviation from the condition of health; but in the majority of instances, you will find, upon close inspection, that they are covered with papulæ, which are nearly of the same colour with the skin itself. Willan therefore places prurigo in the order of Papulæ. He describes several varieties of this troublesome complaint:

prurigo *mitis*; prurigo *formicans*; prurigo *senilis.* The torment experienced by patients suffering under the severer forms of the malady is scarcely describable; they scratch and tear themselves incessantly till the blood flows, their sleep is broken, and their lives are rendered utterly miserable. Sometimes this itching is diffused irregularly here and there over the surface; sometimes it affects the extremities only; and frequently it has a still more limited habitat, occurring round the anus, when it is called prurigo *podicis*; or on the scrotum, prurigo *scroti*; or, worst form of all, the prurigo *pudendi muliebris.*

All these forms of prurigo are apt to be aggravated by heat, and by exposure to the air; they are, therefore, especially distressing when the patient undresses and goes to bed. The scratching tears away the summits of the papulæ, and some watery fluid mixed with blood escapes, and concretes into small thin, black scabs. In the prurigo formicans, the itching is combined with other painful and disagreeable sensations, which different patients describe in different terms: the feeling is like the creeping of ants, or the stinging of insects, or as if hot needles were thrust into the skin. The prurigo senilis, occurring, as that name implies, in old persons, is usually very obstinate, and often effectually destroys all comfort for the rest of the patient's life.

In such cases as I have now been mentioning, great care should be taken thoroughly to cleanse the surface of the body: and the diet should be rigidly plain. All kinds of rich sauces, hot condiments, pickles, and indigestible substances, should be peremptorily forbidden. Various local applications have been praised; but they are, in many cases, used in vain: vinegar, lime water, decoction of dulcamara, lotions composed of prussic acid in an emulsion of bitter almonds, a dilute solution of creasote, decoctions of stavesacre, and of digitalis, ointments containing mercury, tar ointment, and a hundred others. In one instance lately, where the ingenuity of another practitioner had been fruitlessly exhausted, I was fortunate enough to effect perfect relief by smearing the itching surface with an ointment containing a small quantity of aconitine. Mr. Gabb has found a weak dilution of the *liquor sodæ chloratæ* very serviceable. Of internal remedies, sarsaparilla, alkalies, arsenic, the iodide of potassium, dulcamara, are the most hopeful. When these means fail, opium is our best, and indeed our only resource.*

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* Since the republication of these lectures in America, I have been favoured by Dr. Bowling, of Adairville, in Kentucky, with an account of a plan of treat-

The local forms of prurigo are frequently connected with local disease, and are most likely to be relieved by measures directed against the primary disorder. Prurigo podicis is sometimes dependent on the presence of ascarides in the rectum. The same troublesome affection is not an uncommon symptom of internal piles: and it sometimes accompanies stone in the bladder. Prurigo senilis is said by Mr. Balmanno Squire to be almost always the effect of the presence of body-lice.

The prurigo pudendi muliebris—itching of the genitals in females—is sometimes so constant and tormenting, and the impulse to scratch the itching part so urgent, as to drive the unhappy patient from society. It even gives rise, in some severe cases, to nymphomania. It may proceed from leucorrhœa: it is frequently a sign of uterine disease. It most commonly affects women in whom the menstrual discharge has ceased to appear. I have never had an opportunity of trying the aconitine in such cases. One local application which has been found very serviceable is the *yellow wash*, which, as you probably know, is a solution of corrosive sublimate in lime water, in the proportion of a drachm to a pint. A saturated solution of borax, first recommended, I believe, by an American physician, Dr. Dewees, has also much testimony in favour of its efficacy.

You will sometimes be consulted—at least I have been, on more than one occasion—about itching of the pubes and scrotum, produced by the presence of the pediculi that are vulgarly called crab-lice. The patients are sometimes quite unaware of the cause of the itching. You may relieve them by the wash I have just mentioned; or, by a more elegant lotion, made by dissolving corrosive sublimate in a little spirit, and adding rose water. A single washing with such a lotion will destroy the whole colony; and the vermin become much more visible

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ment which he has found eminently successful against this most distressing malady, and which ought, therefore, to be made generally known. I extract that portion of Dr. Bowling's obliging letter which relates to this subject:—

'I have, in the last fifteen years, prescribed for a great number of cases of *prurigo senilis*, and I can say, with a most rigid adherence to truth, that I have not failed in a single instance to effect a permanent cure.

'I direct that the affected parts be sponged for a minute or so with good apple vinegar, and then be allowed time to dry. After this they are to be *smeared* over with the citrine ointment (*unguentum hydrargyri nitratis*). The applications are to be made twice a day. The cure is usually effected in a week. I have never known the constitutional effects of the mercury to be developed in this treatment, save in a single instance, and then but very slightly.'

after this violent death, turning black, and relaxing their hold upon the skin.

Prurigo is a convenient generic name for these cutaneous affections, of which the prominent feature is the teasing sensation that accompanies them. But, besides all these, there is a specific disorder, which, from the intensity of that sensation, is emphatically termed *the itch*, and which deserves a short notice; for it is exceedingly common, and exceedingly distressing, and (what is more interesting still) it is easy of cure. It is one of the very few complaints for which we possess a specific or infallible remedy.

Scabies, or the itch, is, as everybody knows, contagious; but it is contagious only in that particular sense which implies contact. It is not producible by any effluvia which the atmosphere can convey: it requires, for its propagation, that the healthy person should touch the diseased person, or some substance which has been in contact with his unhealthy skin. Certain parts of the skin are more liable to it than others. It is most common at the roots of the fingers and thumbs, on the webs, as it were, between them; on the wrists; between the toes; in the flexures of the joints. It may spread to almost every part of the trunk or of the extremities; but all observers agree in stating that it is seldom or never seen upon the face and head; a curious and unaccountable exemption.

The eruption is at first papular and then vesicular, presenting a number of pointed watery heads. When the inflammation is aggravated by intemperate habits, or by the scratching from which the patient is unable to refrain, the vesicles are liable to be converted into pustules: and this has needlessly been made a separate *species* of itch, scabies *purulenta*, pocky itch: you see large pustules, filled with a yellow viscid matter, standing on an inflamed base. If you are not aware of these varieties and changes, you may make unlucky errors of diagnosis: affront your patient by telling him he has the itch when he has it not; or suffer him unconsciously to betray and shame himself by communicating it to others, when he has it. You will easily understand how it has come to be considered a disgrace to have the itch: for it is fostered and propagated in most unfashionable places, amidst poverty, vulgarity, and filth. Yet the most delicate and high-bred lady may contract the distemper; and when once it is contracted, it will go on indefinitely, through life, unless proper means are adopted for its cure. It never gets well if left to itself.

The most curious point in this discreditable malady, is its connection with a peculiar insect, called accordingly the *acarus scabiei.* The existence of this ectozoon had long been affirmed and denied; but the vexed question was at length set at rest by the public demonstration of the acarus, by a M. Renucci, to a number of medical practitioners in Paris. It has since been often detected and exhibited here. Dr. Nevinson assured me that he furnished Shaw the naturalist, Dr. Wollaston, and others, with living itch-mites for examination, more than fifty years ago. It is described, and its form is depicted, in a letter written by Dr. Bonomo, and communicated to the Royal Society, by Dr. Mead, in the year 1703. One reason, probably, why it has often been searched for in vain is that the acari are not equally numerous with the vesicles; there is not an insect for every vesicle. Another reason is, that the hunters have not known exactly where to look for the insect. It is not *in* the pustules or vesicles, but *near* them; at the extremity of a short, small, superficial tunnel or furrow which runs from them. A third reason why the insect has so long and so often escaped detection, is to be found in its minuteness. It is barely visible by the naked eye; but under the microscope it is seen to be a most formidable monster, in outline like a tortoise, and having eight legs. I show you here its portrait, not in little, but enormously magnified. I hope to procure for you the privilege of seeing the creature itself. The first that I can catch shall be shown to you through the microscope.

There is no doubt that this parasitic animal is, not merely a casual companion, but the veritable cause, of scabies. Various attempts have been made, and made in vain, to produce the disease by inoculation of the fluid from the vesicles. On the other hand, transportation of the acarus has always excited the eruption.

These facts explain how it is that the itch, though readily communicable by direct contact, or by fomites, is not communicable through the medium of the air; that fomites long retain the contagious property; and that the disease is curable by whatever destroys the acari. I believe that the complaint called *the mange*, in dogs, camels, and sheep, has the same, or a similar origin: and I think it extremely probable that certain other varieties of prurigo or pruritus, in the human subject, may depend upon a like cause.

Now *sulphur* is as sure to cure the itch, as quina is to stop an ague. I presume that it kills the acarus; but whatever may be its *modus operandi*, I have never known it fail to remove true scabies. It is applied externally; and the only objections to its use are its disagreeable smell, and the dirtiness that belongs to ointments: but these inconveniences are far outweighed by its certain efficacy. Although many substitutes for this substance have been recommended, I pretermit them all, and advise you to employ the sulphur ointment, of which you may disguise the smell by the addition of a little bergamot, and the colour by intermixing a small quantity of vermilion.

The whole surface of the patient's body should be washed perfectly clean, with plenty of soap, while he is in a warm bath. As soon as the skin has been made again quite dry, the ointment should be well rubbed, all over it, before a fire, for half an hour together. Then, a second thorough ablution with soap and warm water, and the destruction of the contaminated clothes by fire, will complete the purifying process.

LECTURE LXXXIII.

Herpes; Eczema; Pompholix; Lepra; Psoriasis; Impetigo; Boils; Carbuncle; Purpura; Scurvy. Conclusion of the Course.

If you look at the list of *genera* and *species* appended to the various works which treat exclusively of cutaneous diseases, you will find that they are exceedingly numerous. But these disorders differ widely in their relative importance: and the principles upon which their remedial management proceeds are not so greatly diversified as these 'tables of contents' might lead you to suppose. I have spoken pretty fully of the most serious and interesting of these maladies—I mean of the febrile exanthemata; but I have no time left for pursuing in detail the host of chronic affections to which the human skin is subject. Nor do I much regret this. To become expert in the diagnosis of these blemishes, and in curing such as are curable by our art, you must see them with your own eyes. Verbal descriptions of their changeful characters are of comparatively little service or interest. They are among the things that require to be 'oculis subjecta fidelibus.' Even pictured representations convey but an inadequate notion of the morbid appearances they are designed to portray. The lecturer on skin diseases should have actual patients before him, to whose bodies he could point. In the lack of living specimens, I would recommend you to study the magnificent and beautiful series of wax models, presented to the Hunterian Museum by Professor Erasmus Wilson.

In this, the final lecture of the course, I can do no more than offer you a few very cursory remarks upon some of the genera into which nosologists have distributed this class of disorders: and I may premise, that the treatment of the genus includes for the most part that of the species.

Vesicles are, as you know, small transparent elevations formed by a drop of aqueous fluid effused beneath the cuticle. Sometimes they are thinly scattered over the surface; sometimes collected into clusters; sometimes situated on a red patch of skin; sometimes quite free from redness. The whole crop comes out at once in some cases; in others the vesicles appear in irregular succession. They terminate also in various ways:

by the reabsorption of the liquid, and slight desquamation; by the giving way of the cuticle, and the formation of little scabs, under which new cuticle is generated; and sometimes, though rarely, by ulceration.

Of the vesicular class of cutaneous disorders I have already described the three most important; namely, cow-pox, chicken-pox, and the itch.

Another not uninteresting genus of this class is *herpes*. This is a transient non-contagious eruption, consisting of red patches of irregular form and variable size, upon each of which stands a crop of vesicles. The eruption runs a definite course; and its several periods—its beginning, its increase, its acme, and its decline—are completed, when its progress is not interfered with, in about ten days. These characters, once known, are easily recognised; and it is of importance that you should recognise them. Not that the disorder itself is of any great moment; nor that it is at all under the influence of remedies; but because it may be confounded with some graver malady. For example, Herpes *præputialis* is a very common and a very trifling species, affecting the foreskin; but it might readily be mistaken for an effect of the poison of syphilis, and so cause much alarm and distress to the subject of it, and entail upon him perhaps a needless course of mercury, and bring unmerited suspicion upon the person with whom, whether lawfully or unlawfully, he might have been connected. It has nothing whatever to do with sexual intercourse; and it requires no treatment beyond cold ablution, and the interposition of a piece of lint between the prepuce and the glans penis. In like manner another species, Herpes *circinatus*, the vesicular ringworm, is liable, when it appears upon the hairy scalp, to be mistaken for that pest of schools and of school-boys, the *favus confertus*, or *porrigo scutulata* of Willan, the common scald-head; a complaint which is naturally shunned as filthy, stubborn, and contagious: and which is in fact a vegetable parasite. Herpes *iris* is a mere curiosity. In this species each group of vesicles is surrounded by four concentric erythematous rings, of different shades of colour. The rings form and fade in succession, one after another, by a slight extension outwards of the inflammation each time. The spots constituting the eruption have been likened to small parti-coloured cockades. But the most singular species of all, and the only species of any serious importance, is the Herpes *zoster*, in which the separate patches lie in the direction of a band that encircles half the circumference of the body. Hence its names, zoster, zona, zona ignea, and in our vernacular tongue, *the shingles*; and even this Dr. Samuel Johnson derives from the Latin, *cingulum*, a girdle.

Most commonly the zone is confined to the trunk of the body; has a somewhat oblique direction like a sword belt, and occupies exactly one-half of the circle, lying between the spine and the linea alba, on one side only: following, in fact, the distribution of the dorsal nerves proceeding from the spinal cord. Sometimes, however, it extends from the trunk to the limbs. Thus it may begin in the loins, pass obliquely across the flank, and terminate at the inner part of the thigh. Or it may commence from the upper part of the spine, cross the shoulder, and end on the arm or forearm. In the year 1833 I saw a lady affected with this strange eruption, in whom the clusters of vesicles began near the spine in the neck, passed over the scapula, then to the shoulder and axilla, whence the main line ran along the outer side of the upper arm till it reached the elbow, where it turned inwards, followed the inner side of the forearm, went across the palm of the hand, and terminated by two or three patches upon the palmar and inner side of the ring finger. Very rarely indeed it appears on the limbs only. Twice I have seen it limited to the thigh and leg, and in both cases its track corresponded with the course of the sciatic nerve. Thrice I have known it spread from the neck, behind, up into the hairy scalp: and in one of these instances a patch fell upon the conjunctiva of the right eye, of which the vision was for some time in jeopardy. Sometimes the deeper textures of the eye become inflamed. Iritis is an occasional complication. The most common situation of the demi-cincture is across the base of the thorax. It is a curious feature of this curious disorder, that, in nineteen cases out of twenty, according to Biett, it occupies the *right* half of the body. Of this singular preference of the right side, if indeed it be a general fact, I can give you no explanation. I have seen fifteen cases of the complaint since I began to attend to that circumstance, and in ten of these the eruption was on the right side.* Rayer, in the first edition of his book, said that eight cases in ten would be found to be on that side; but a longer experience has reduced that proportion. Of fifty-three examples seen by himself, thirty-seven only were on the right, and sixteen on the left. Reil states that he has *always* observed it on the left half of the body; and Mehlis, among twenty-five patients, counted sixteen in whom the left side was affected. This statistical point remains therefore to be settled, if it be worth settling, by a larger induction of particular cases. The zone seldom transgresses the median line at either extremity, unless perhaps the redness of the extreme patches may extend a little further. It is said, however, in some exceedingly rare instances, to complete the circuit of the body: or, in more correct language, to affect both sides of the body at once, and symmetrically. There is a vulgar but erroneous notion, that the eruption proves fatal when it thus seems to encircle the whole of the trunk; and this notion is as old as the time of Pliny, who says, 'Zoster appellatur, et enecat si cinxerit.'

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* Since this was written many more cases of shingles have come under my notice, but I am sorry to say that I have mislaid the memoranda which I had made of the positions of the patches.

The most important, because the most distressful, of the symptoms, is an intense darting pain, described by the patients as being deep-seated, very acute, and shooting through the chest. Fortunately, however, this is by no means a common incident. At least so I formerly thought, and such is the tenor of recorded experience. Yet within the last three years these sufferings have occurred in five or six of my own patients. Sometimes the pain precedes the eruption; more often it accompanies it; and it is apt to last, in spite of remedies, for some time after the eruption has disappeared. Mr. North tells me that, in a female patient of his, this pain continued to be severe and intractable for eighteen weeks. In two instances I have myself known it last for two years; and in one of the two its severity had scarcely abated in that long time. That this is, however, a rare complication of the disorder we may conclude from the experience of M. Biett, who never once witnessed this symptom in more than 500 cases of shingles. The severe and intermitting character of the pain, and the peculiar direction of the row of herpetic patches, indicate very clearly that the whole malady falls within the category of diseases of the nervous system. You will find it interesting—and it may be a useful test of your anatomical knowledge—to trace the various directions in which the line of oval patches runs in different cases. You will find that the spots, and even sometimes little sprigs or branches of the line, follow strictly the track of nerves. To use Mr. Jonathan Hutchinson's words, 'the vesicles are the flowers to which the nerve twigs are the footstalks.' It is seldom that any grave or permanent organic mischief is associated with the disorder. Yet I have been informed that, not many years since, the son of a distinguished meteorologist in this country suffered an attack of shingles on the right side of his face, following the second, and in some degree the third, branches of the fifth nerve. It was attended at the outset with severe pain, which at first was attributed to neuralgia, or tic; and it ended with necrosis of the palatal bones of that side. Dr. Gull has seen an instance in which necrosis of one of the vertebræ occurred.

Many incidental circumstances might be mentioned illustrative of the nervous character of the complaint.

One of the three patients in whom I had seen the scalp affected with the herpetic patches, had been plagued for seven years with continual noises in his head. Upon the breaking out of the eruption these noises ceased; and remained absent for a year and half: then they returned. Another person who had an attack of shingles in February, suddenly lost a cough which had teased him all the previous winter. In July, 1855, I was asked to see an elderly maiden lady, who in a former part of her life had had ague, and neuralgia. Shingles had appeared on the left side of her body two months previously to my visit. A demi-cincture of patches, which almost touched each other, extended from one mesial line to the other. But besides this row, there were scattered spots of herpes, not linear in their direction, on the right side of her body, on the shoulder, in the arm-pit, and one on the right thigh. She had suffered and was suffering intense neuralgic pains in the track of the belt, just along the edge of the left ribs. Any cold fluid taken into her stomach would excite the pain at any time; but it often occurred spontaneously. The right half of this patient's body was sometimes warm and perspiring, while the left was chilly, shivering, and affected with horripilatio. The neuralgic pain had somewhat of a periodic character. On one occasion it was banished for twenty-four hours by half a scruple of quina. But the quina so disturbed her head that she would not repeat it, although various other remedies had been tried in vain.

Of the causes of herpes zoster we have no certain knowledge. It is said to attack young persons more especially, and those who have fine and delicate skins, and the male more frequently than the female sex. But I suspect that these assertions rest on a very loose foundation. Of the fifteen cases already mentioned, ten occurred in females. One of the patients was a child two years and seven months old; another was an aged man of about seventy-five. I have since seen an example of the complaint in a gentleman who wanted only five weeks to complete his eighty-fourth year. In several instances I have found upon inquiry, that the patients, being children, were in the nightly habit of wetting their beds. Whether this has been anything more than a casual coincidence, I do not know; but my attention was first directed that way, some years ago, by Mr. Wheeler, the apothecary at St. Bartholomew's Hospital, who told me that he had often noticed the same circumstance. According to Bateman, the disorder 'seems occasionally to arise from exposure to cold after violent exercise. Sometimes it has appeared critical, when supervening on bowel complaints. Like erysipelas, it has been ascribed by some authors to paroxysms of anger.' Schwartz saw three cases which followed violent fits of passion; and Plenck affirms that he had known it occur twice after furious anger—*and* a copious potation of beer.

The duration of the eruption is from ten days to a fortnight; but it is liable to be considerably prolonged by troublesome ulceration, whenever the vesicles and crusts are prematurely chafed off by friction or pressure.

Very little, as you must perceive, can be done, or is requisite, in the way of treatment. The patient is to be cautioned against rubbing off the heads of the vesicles. They may be protected by a thin film of flexible collodion. A medical friend of mine, who had herpes on his forehead, got much relief from pain and itching by using a lotion consisting of three fluid drachms of the dilute hydrocyanic acid with six ounces of lime-water. Attention should of course be paid to the state of the stomach and bowels; and the diet should be regulated. Our main business is to look on, and to endeavour to set right whatever function may be manifestly wrong.

Should the eruption be attended or followed by the intense shooting pain which sometimes, but not very often, harasses the patient, it will be right to apply opiates, by friction, over the affected region. I would use the aconite or the belladona ointment in such a case, or morphia subcutaneously. Warm baths will also be proper; and as the pain is probably neuralgic, the carbonate of iron is a remedy which ought to be tried.

Eczema is another genus of the vesicular class of diseases. It is characterised, in its commencement, by an eruption of very minute vesicles, scarcely prominent, closely crowded together, and requiring a microscope sometimes to render them distinctly visible. They terminate either by the reabsorption of the fluid they contained, or by the formation of superficial moist excoriations. Eczema is not contagious.

There are several species or varieties of this form of cutaneous disorder also. It is sometimes produced by great heat, and particularly by the direct rays of the sun; and this is named eczema *solare*, *heat-spot*; sometimes by the contact of irritating substances with the skin, as in what is vulgarly called the grocer's itch, affecting the hands of those who are much conversant with sugar. Eczema often occurs upon the scalp, and constitutes, I believe, the most frequent form of what is loosely called scald-head, porrigo, or tinea capitis. But the most severe of all its species is that which has received the names of *Hydrargyrium*, *Erythema mercuriale*, and *Eczema rubrum mercuriale*. This, as these names imply, is an occasional consequence of mercury; an unusual consequence, no doubt, and one that happens only in a few peculiar constitutions: but you ought to know it, in case it should follow the use of mercury prescribed by yourselves.

The eruption begins usually in the groins and upon the thighs. It is at first red, and is accompanied by much heat and itching. It soon extends, in the severer cases, over the whole body; and an innumerable multitude of very minute glittering vesicles may be seen, with the aid of a magnifying glass, from the beginning. Like that of erysipelas, the eruption is attended with a good deal of swelling. The intumescence of the face is such as to close up the eyes: and the disorder *becomes* febrile, in its course; for there is seldom much fever at the onset. The vesicles increase in size, turn milky, burst, and pour forth an acrid exudation, that irritates and inflames the skin with which it comes in contact, and thus increases the local complaint. The distress and worry occasioned to the patient by the fœtid smell of the discharge, by the stiffening which it causes of his body-linen, and by the heat and itching, are, I conceive, the main causes of the febrile disturbance. The discharged matter is apt to become thick and hard, and to present the appearance of large scabs: and in this state the nature of the disease may very easily be misunderstood, it being impossible to say, when it is seen for the first time under these circumstances, whether it was originally vesicular or not.

The duration of this harassing distemper is variable. It may be over in a fortnight, or it may last several weeks. It terminates by the cessation of the discharge, and then the cuticle detaches itself in large flakes. Sometimes in this disease also the epidermis falls entire from the hand, like a glove.

Without being dangerous to life, this disorder is apt to be obstinate. It is not much within the control of remedies. What little can be done is chiefly palliative. The mildest local applications must be used: tepid water, barley-water, strained gruel. The warm-bath, when circumstances permit. Poultices are sometimes of much service, in preventing the hardening of the matter that exudes, and so obviating one source of irritation. Flour, or powdered charcoal, may be sprinkled over the eruption in the slighter and early cases, for the purpose of absorbing the discharge. Equal parts of olive-oil and lime-water make a soothing liniment, which may be applied by means of a feather. The local remedies may fairly be varied, for sometimes one is found to give relief, and sometimes another. The patient's linen must be frequently changed, especially whenever it becomes stiff and hard with the exudation.

With respect to the general treatment, opiates to procure rest, and to allay irritation, are probably indispensable. The bowels must be kept moderately open, but no severe purging should be employed, for the patient must at all events undergo a long and weakening process, and therefore it must be our care that the *treatment* be as little weakening as possible. In protracted cases, where there is much exhaustion, wine may with propriety be given; and, almost always, good strong broths. As the disorder declines, some of the reputed tonics may be prescribed; the mineral acids, quina, sarsaparilla.

I need not say that, in such cases, you must be scrupulous in seeing that no more mercury be administered or applied.

Eczema, spontaneous in its origin, is a very common, and a very teasing complaint among children. Mr. Erasmus Wilson, after large experience in treating it, expresses his confident belief that almost every case of *eczema infantile* admits of a ready cure. His method is to give calomel at moderate intervals, with the view of clearing and regulating the digestive organs. This preparatory treatment is followed by the administration of arsenic in small doses, internally, and by the external use of well-prepared oxide of zinc ointment.

In many children thus tormented with eczema, I have remarked that the rapid departure, or even the sudden diminution, of the eczematous eruption has been immediately succeeded by wheezing in the chest, and oppressed breathing; which in their turn have subsided upon the re-appearance of the eruption. This alternating affection of the tegumentary membrane and of the mucous membrane of the air passages, has sometimes made me dread the curing, so earnestly desired by mothers, of the unseemly disorder of the skin. Mr. Wilson believes that no such dread need be entertained, when the cure has been preceded by a due course of calomel.

A separate class of cutaneous diseases, very analogous, however, to that which we have been describing, is the class of *bullæ*, or blebs. Anatomically speaking, there is but little difference between the two: *bullæ* are *large vesicles.* When the eruption is at its height, it is composed of hemispherical prominences of various sizes, from that of a pea to that of a hen's egg, and having the shape and appearance of the bubbles raised in a pool of water by a hard shower of rain. They are formed by the effusion of a serous, or a sero-puriform fluid, between the true skin and the cuticle. You can only be sure of the diagnosis when you see the eruption in this stage of its progress.

The best example of this class is that which is called by some writers *Pemphigus*, by others *Pompholix.*

It is characterised by the presence of bullæ, varying in their magnitude, commonly distinct, but numerous, springing up in successive crops, on one or more parts of the surface. At first these bullæ are nearly transparent, and contain a thin limpid serum; but they become gradually opaque, pearl-coloured, and ultimately many of them acquire a reddish tinge.

Pemphigus has been described as being sometimes acute, sometimes chronic. The acute form is attended with smart fever, the bullæ rise spontaneously or in quick succession, run their course, and disappear; and then the disease is over. This is a very rare form. In general the bullæ continue to come out; the complaint is spread over weeks, or months, or years; and it is accompanied by little or no febrile reaction. This, on the other hand, is a common form of disease. It is the *Pompholix diutinus* of Willan and Bateman. The eruption often occupies all parts of the body at the same time, or in succession: in other cases it is confined to a limited space. I have most frequently seen it on the fore-arms and legs. When the bullæ are very numerous, they may give rise to some febrile symptoms, but not else. The complaint may be indefinitely prolonged by successive crops.

The eruption begins in small red points, the formation of which is attended with a slight pricking sensation. Some patients have likened this sensation to that which accompanies the passage of the electric spark. In the centre of each of these spots, the cuticle becomes lifted, while the circumference of the spot enlarges, so that bullæ are rapidly formed, often in the space of a few hours only, as big as a hazel-nut, or a walnut: or the blebs may even be much greater than that. Either in consequence of their distension, or of the pressure made upon them by the movements of the patient, some of these bullæ burst, and a straw-coloured alkaline fluid escapes. Then the epidermis collapses into folds and wrinkles; or if it be detached at a part of the margin of the bulla, it is rolled back, so as to expose a portion of the red, painful, and smarting surface beneath it. Towards the third or fourth day, when the bullæ lose their transparency, and the liquid they contain becomes reddish, those bullæ which have not been broken sink down and wither; the cuticle is no longer stretched; but, sodden by the serous fluid, it assumes a whitish hue, becomes opaque, and forms at length small brownish flat crusts, of no great thickness.

In the meantime fresh bullæ appear by the side of the former ones, and pursue the same course; so that generally you may see in the same person, tense bullæ containing a transparent and yellowish serum; thin crusts; and irregular patches of various sizes, slightly excoriated.

This is the ordinary course of chronic pemphigus: and I say it may thus go on for months or years.

Local applications have seldom been much employed, except some mild ointment to the excoriated parts. Biett advises emollient lotions, or even opiate washes when much irritation exists; but a case recorded in the *Medical Gazette*, by Dr. Graves, of Dublin, affords a remarkable instance of a cure by local applications alone: and it is a case worth recollecting, although, as he justly remarks, we ought not to generalise from a single instance.

His patient was a boy, fourteen years old, of slender frame and delicate constitution, yet enjoying uninterrupted health, except the cutaneous disease, which had lasted five years. During that time the succession of bullæ had seldom ceased. The bullæ were very numerous, occupying not merely the face and extremities, but the trunk also: and they were in various stages of their progress, some healing after having burst, some of a larger size and unbroken, others small, and recent.

Dr. Graves observes, that from the descriptions of Bateman, and of Biett, although both authors describe it correctly, we should scarcely form a notion of the occasional severity of this disorder. He had seen two examples of it in young men, where the irritation and suffering produced by the constant exposure of large portions of skin denuded of epidermis, had operated most unfavourably on the general health, almost banishing sleep, and reducing the patients to a state of great debility. These cases did not yield to the methods of treatment recommended by authors; and, therefore, Dr. Graves determined, whenever another opportunity should occur, to have recourse to a new plan.

In the boy in question, therefore, he had all the bullæ opened with a lancet, and the denuded surface of the corium was then touched with a stick of lunar caustic. The nitrate of silver was also applied to the skin around each bulla, for the breadth of a line; and the recent pimples, which indicated the formation of future bullæ, were all treated in the same way. The boy was then washed, and supplied with clean linen.

This single application of the nitrate of silver had not merely the effect of entirely destroying the morbid action in the portions of the skin which were then affected, but (what was very remarkable) no fresh bullæ made their appearance afterwards: none at least had appeared for four months, when Dr. Graves wrote his account. The only part where a repetition of the process was required, was the palm of the hand, where the thickness of the cuticle rendered it more difficult to expose the diseased surface of the cutis to the full action of the caustic.

It might strike you from this cure of a long standing disorder, so readily, by mere local means, that the disease propagated itself from one part of the surface to another, by a sort of re-inoculation. But it has been fully proved that the disease is not contagious. A Mr. Gaitskill engrafted himself, with impunity, with the fluid; and analysed it, and found it apparently like the thin serum of hydrocephalus. Dr. Graves, therefore, supposes that the cure was owing to the simultaneous destruction of all the parts of the skin that were in a state of morbid action: a morbid action which would have been otherwise propagated to other portions of the surface, by what is called the sympathy of *continuity*.

The class of scaly eruptions—the *squamæ*—is distinguished by the occurrence of red spots or blotches, upon which laminæ of altered cuticle form, and are thrown off, and constantly renewed. You will perceive that, anatomically, this class of cutaneous disorders has a close analogy with the rashes; and yet it is separated from them by very obvious particulars. In the exanthemata of Willan and Bateman, the redness is *followed* by desquamation; in the squamæ these two appearances co-exist: in the exanthemata the sequence of redness and desquamation takes place, in general, once only; in the squamæ the morbid cuticle continues for an indefinite time to scale off again and again, in successive fragments, from the abiding red patch of skin.

Lepra, psoriasis, and pityriasis, and some syphilitic eruptions, constitute the principal of the squamous affections.

Lepra is a very common disorder of this class; hence its name, lepra *vulgaris*. It consists in red scaly patches, of various dimensions, but always affecting a circular or elliptical shape, and scattered over different parts of the body. It commonly begins on the limbs, most usually near the joints; just below the knees, or the elbows; and Dr. William Budd has pointed out the curious fact that these patches, especially when they are few, and the disease is recent, are distributed symmetrically, each spot on the one limb answering in situation to a similar spot on the fellow limb. This shows that the disease is probably a blood disease; that it depends upon some poison, introduced from without, or, more likely, bred within the body. By degrees the patches both enlarge in size, and multiply in number, and extend along the extremities to the trunk. The eruption is seldom seen upon the hairy scalp, or upon the hands. As the patches enlarge they sometimes become confluent; but even then, the outline of the confluent scaly space is defined by arcs of circles. It is not easy to set these things before you in mere verbal description. To have *seen* lepra once, is to know it for ever.

When the patches begin to get well, the restoration of the altered surface to its natural condition and appearance commences in the centre—*i. e.* in the spot first affected—and proceeds outwardly towards the circumference: so that the scaly redness assumes a ring-like arrangement. This ring becomes gradually narrower and narrower; at length its continuity is here and there broken; and at last it vanishes entirely.

The eruption does not, however, run any definite course. Sometimes it goes rapidly through its phases; in other cases it persists for a very long period. It is not at all contagious.

Neither is lepra attended, in general, with much local inconvenience, nor with much constitutional disturbance. When the eruption is very copious and extensive, and especially when it is plentiful or almost continuous around the larger joints, it renders the movements of the limbs stiff and difficult; and even sometimes painful, from the cracking of the inflamed surface as it is stretched in the bending of the joint.

But I have seldom found lepra to exist *unconnected* with some disorder of the digestive organs. Usually the connection is that of alternation, and not of coexistence. The patient is dyspeptic till the eruption comes out, and then the dyspepsia is relieved: and it often returns as the leprous patches disappear. The eruption is the more unsightly; the dyspepsia is the more troublesome. This alternation would seem to mark the shifting location of the *materies morbi.*

When the patches are small, and chronic, and white, that variety is no longer called lepra vulgaris, but lepra *alphoïdes*; and there certainly is another distinct variety, of a more blue, or livid, or copper colour than the ordinary; and a result of the poison of syphilis. It is named accordingly syphilitic lepra. This species will get well under the biniodide of mercury; which, so far as my observations go, does not cure the others.

Psoriasis is closely allied to lepra. When it occurs in distinct patches it is often difficult to say to which genus the eruption belongs. In general the patches of psoriasis are not so broad as those of lepra; their edges are less raised, and their centres less depressed; the scales adhere more firmly; and the patches are less uniform and less circular.

But, psoriasis frequently spreads itself over large portions of the skin, and it may come to occupy nearly the whole surface of the body. It is then called psoriasis *diffusa*. It often renders the patient hideous to look at. The scaly incrustation is interspersed with chaps, furrowing the skin in all directions, and following particularly its natural folds and angles. These cracks, when the skin is put upon the stretch by the movements of the patient, are apt to bleed. In these severer cases (which are said to be examples of psoriasis *inveterata*) the laminæ of altered cuticle are thick, and very abundant. They fall off perpetually, or are rubbed off, and may be shaken from the patient's clothes, or collected in handfuls from his bed.

The diseases last mentioned—pemphigus on the one hand, lepra and psoriasis on the other—are to our view, and in some particulars, very unlike; nevertheless, they are bound together by affinities of more importance, which have been well set forth by Mr. Jonathan Hutchinson. He states that both the bullous and the scaly eruption come out suddenly in persons apparently in perfect health, and that both prefer the young.

Pemphigus, lepra, psoriasis, some forms of eczema, some forms of lichen, present in their history a very remarkable contrast with the febrile exanthemata which we have so lately been discussing. The chronic, non-febrile skin diseases are, nevertheless, constitutional, as we infer from their symmetrical characters; i.e. they are either blood diseases, or they are determined by some obscure influence of the nervous system. They are not, however, contagious; they do not run a definite course; have no tendency to spontaneous recovery; confer no protection against their own recurrence, but, on the contrary, show a strong disposition to recur or relapse. The most cheering feature of the contrast is, that they are, for the most part, obedient to medicine. They yield with remarkable steadiness, though often slowly, to arsenic given in small repeated doses; two or three minims, for instance, of Fowler's solution, three times a day. If they return after cure by this drug, they return in a milder form, and recede again on its resumption.

This I believe to be the most trustworthy remedy. But the Harrowgate waters, a strong decoction of dulcamara, pitch-pills (and if pitch-pills, I should suppose *à fortiori* creasote), tincture of cantharides, and the iodide of potassium, are also remedies of some renown for the scaly diseases. In all cases the diet should be regulated, and all kinds of stimulating food refrained from. Dr. Bateman knew a man who was always attacked with lepra if he took spices with his food, or drank ardent spirits: and a patient of my own got rid of long-standing and very troublesome psoriasis of the scrotum, upon adopting, for other reasons, a very abstemious and simple mode of living.

Parts of the surface of the body—the chest, the neck, the shoulders, the abdomen, even the forehead—are marked sometimes by irregular brown patches of what is called *Pityriasis*; from πίτυρον, bran. The discoloured portions are in fact covered with small bran-like films, which fall off, and are succeeded by others. I call your attention to one variety of pityriasis, the pityriasis *versicolor*, chiefly because it is (like the favus confertus or scald-head) a sample of the *vegetable parasites* with which the human integuments are liable to be defaced. Viewed through a microscope, these bran-like scales present the spores and filaments of a minute cryptogamous plant or fungus, the *microsporum furfurans.* Pityriasis is an eyesore or blemish rather than a disease: but it sometimes excites the apprehension of some syphilitic or other constitutional taint. Whether the skin upon which this fungus clings and grows must be in an unhealthy state to admit of its first invasion, has not been learned. Drugs are often diligently administered to cure the disfigurement: but I believe that they are always useless and superfluous. Some years ago, before I was aware of the true character of these blotches, I tried various means, in vain, to remove a large one from the neck of a young lady whose beauty it was marring. At length it yielded at once to a couple of sulphur baths. The medication must be external. A saturated solution of sulphurous acid gas in water is an effectual remedy; or the parasitic plant may be killed and dislodged by a wash containing corrosive sublimate.

Among the *pustular* diseases of the skin there is one which assumes many forms, and is termed *impetigo*. Whatever may be the minuter peculiarities of this eruption, its general characters are the following. It consists of crops of small, pointed pustules, sometimes scattered irregularly, sometimes collected into groups. The pustules burst, or are broken, dry up, and scab over. The crusts are yellowish, and very friable, and resemble in appearance little masses of candied honey; or sometimes they look like small pieces of dirty plaster. From beneath these crusts a considerable discharge continues to take place; the crusts become thicker and larger, and around their margins the skin is red and raw, as it is also beneath them.

We have, I say, various forms of this complaint; impetigo *figurata*, impetigo *sparsa*, &c. It often borders closely on eczema, so that authors describe an eczema *impetiginodes*, or an impetigo *eczematodes*. These varieties are delineated by Rayer, by Willan and Bateman, and by others; and knowing their characters, you can examine and study their appearances for yourselves. Impetigo is a non-contagious disorder.

Sometimes this complaint occurs in an acute form, and is attended with fever. Whatever local applications are made should not be unctuous. It is seldom that impetigo will be the better for, or will bear, ointments. Purgatives and alkalies internally, and very weak spirit or alkaline lotions externally, with a scrupulous diet, constitute, I believe, its best treatment. When the complaint is chronic, and the discharge copious, the oxide of zinc has often a very beneficial effect. It may be dusted over the affected surface, from a thin muslin bag; or it may be applied in the shape of a lotion—fifteen grains to an ounce of rose-water. You will find this a most useful lotion for that disfiguring impetiginous or eczematous eruption which sometimes covers the faces of children like a mask, and is called *crusta lactea.* The phrase crusta lactea is, however, very loosely employed by medical men.

There is a very common, and a very teasing pustular disease of the skin, usually called a *boil*, in some parts of England a *push*, and by the learned *furunculus.*

First, there is a slight degree and extent of hardness to be felt, a tender knot, just beneath the surface, which soon begins to look red, and a small swelling arises, which gradually increases up to a certain size, that of a large pea, or of a hazelnut, or of a walnut. The tumour is painful, and undergoes a process of slow suppuration. Some time from the fourth to the eighth day it acquires a conical or pointed form, and its apex becomes of a white or yellow colour. At last the cuticle gives way, and the patient begins to congratulate himself that the little abscess is ripe, and that his troubles are nearly ended. But he is disappointed; an insignificant quantity of pus mixed with blood escapes, and leaves visible a mass of dead areolar tissue—a *core,* as it is called—of greater diameter than the opening, which is commonly small. At last, two or three days perhaps after this, the slough is expelled, in company with more pus, and a deep cup-like cavity remains, which soon, however, fills up, and the boil is really over.

These little phlegmons frequent the buttocks, the thighs, the arm-pits, the nape of the neck, the abdomen. They may occur almost anywhere. They are apt to come in crops, or in a series: and any kind of irritation suffices to cause them when a constitutional tendency to their formation exists. I have known a piece of soap plaster applied to the skin give occasion to a long succession of boils. Poultices, used to promote the suppuration of any existing furunculus, are believed to encourage, by their warmth, the growth of others around it. In truth, these phlegmons belong primarily and essentially to the subcutaneous areolar tissue rather than to the skin. Dr. Prout corroborates the statement of Cheselden, that they are often accompanied by a saccharine condition of the urine. You know probably that, in Dr. Prout's theory of assimilation, the areolar tissue represents the saccharine element.

Boils have been very prevalent for some few years past in this country: and not here only, but on the continent of Europe also, and throughout America. It is stated—by an anonymous contributor to the 'Medical Times' (December 2, 1854), who founds his conclusions upon the observation of several hundred cases among the out-patients of St. Bartholomew's Hospital and of the Hospital for Skin Diseases—that the number of males affected with boils in a given time is rather more than double the number of females: also, generally, that boils are twice as common during the first four months, as during other parts of the year.

The individual boils are intractable: the state of system which engenders them, or which favours their formation, may often, I believe, be corrected. Some dab them, when nascent, with a solution of corrosive sublimate in spirit; some support them with sticking-plaster; some paint them with the compound tincture of iodine; others apply poultices, or what is better, and perhaps the best local application, lint wetted with water and covered with oiled silk; and others again cut the hard tumour through, while it is yet crude. Do what you will, you can seldom prevent or accelerate their deliberate course; but I believe that by applying leeches, or cold, you may prolong, though you cannot arrest that course.*

From the time of John Hunter, who cured himself of a disposition to boils by taking 'the fossil caustic alkali, night and morning, in milk for two months,' it has been the fashion to prescribe alkalies for persons so troubled; particularly the liquor potassæ, in combination with sarsaparilla: but I have satisfied myself that a better remedy—of opposite chemical quality—is to be found in the dilute sulphuric acid. For some years, acting upon a hint received from Dr. Bullar, of Southampton, I have given this acid, twice daily, before meals, in doses of ten or fifteen minims, to a great number of persons who were infested with boils, and the instances have been but few in which it has failed to check the tendency to their formation. When the system is below par, the sulphate of quina, and a generous regimen, may be added: meanwhile sugar, and saccharine food of all kinds, should be scrupulously avoided.

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* 'Is anything useful to be done to the part affected? . . . I reply unequivocally in the affirmative. The boil cannot be jugulated, as it has no neck; but its head may be split open in its embryo state, and there will be an end of it. . . . The treatment succeeds if adopted on the second or third day; and even later it does some good. If done late, the disease is not entirely overcome: it is blighted, and is short-lived. This treatment is not worth trying after the fifth or sixth day; not after the affection has extended deep into the true skin.'—Dr. Jackson. 'Letters to a Young Physician.' Boston, U.S., 1855.

Carbuncle, *alias* anthrax, is a gigantic boil, and something more. It constitutes a far more serious disorder than the common furuncle, not only in respect of its magnitude, and of the amount of suffering which it occasions, but also on account of the constitutional vice that it betokens. A carbuncle is a large, flat, circumscribed, very hard, and very painful tumour, of a purplish red colour, and attended with a sensation of burning heat. Its ultimate diameter may be three or four inches, or more. It ends in the formation of a deep slough, of more than corresponding dimensions, and the destruction of the skin above it. A number of pinhole openings at length present themselves on the dark red surface, and disclose the immense core beneath.

That carbuncles and boils are *kindred* disorders appears distinctly from this—that occasionally a carbuncle results from the confluence of two or three boils which had arisen near each other: and not less distinctly from the simultaneous prevalency of the two. The recent increase of the carbuncular disease has been even more marked than that of the furuncular. Of this I may offer you proof from the records of the Registrar-General. These records deal, indeed, with fatal instances only. Of course the whole number of cases that occurred in the same periods must have been far greater. In the five years ending with 1845, the average number of deaths from carbuncle in London alone was 5: in the next five years, ending with 1850, the number increased to 14: in 1851, it amounted to 19: in 1852, to 50: in 1853, to 70: and in 1854, to 89. In that year, which you will recollect was a cholera year, the deaths from carbuncle in England, exclusive of London, were no fewer than 300.

The cause of this vast increase of these disorders has not, I think, been ascertained. Professor Laycock indeed imputes it to their *contagious* properties—classing together boils, carbuncles, whitlow, and the charbon and pustule maligne of the French, which are less frequently seen in this country, under the common title of the *contagious furunculoid*. He suggests the question whether that disease may not have had an epizootic origin, and whether its present wider and wider diffusion may not be derived from the imported hair and hides of animals affected with a carbuncular distemper, which has been epidemic among cattle in the South of France, Italy, Germany, Poland, Hungary, and Russia. I am bound to tell you that, in my judgment, this alleged quality of contagiousness, with respect at least to the two forms of disease with which we are most familiar here, the boil and the carbuncle, is ‘ not proven.’

Carbuncle is met with chiefly in advanced life, in corpulent males, and in persons who have lived fully : chiefly, but not exclusively. The writer in the ‘ Medical Times,’ to whom I have already referred, gives a tabular account of 35 cases of carbuncle, of which 25 were noted, within six months, among the patients at St. Bartholomew’s Hospital. Instances of it occurred at various periods of life from 15 years of age to 80 ; among the ill-fed and the well-fed, the temperate and the intemperate, and more than twice as often in males as in females. Though the carbuncles may appear in almost any place, they most commonly affect the more brawny portions of the skin, and the hinder parts of the body ; the nape of the neck, the shoulders, the buttocks. I have however myself seen a large carbuncle on the belly. A virulent form, resembling the pustule maligne of the French, has been described as occurring upon the face, at St. Bartholomew’s, by Dr. Harvey Ludlow. Carbuncle is then perhaps most dangerous, though not necessarily fatal, when it attacks the scalp.

The local disease is productive of high constitutional disturbance and irritation. Surgeons were long in the habit of dividing the firm mass into quarters, by deep crucial incisions. This is a sharp remedy, but it purchases speedy ease, by removing that tension of the inflamed parts whereupon the pain chiefly depends. I am persuaded, however, that this severe operation has been done too indiscriminately. When there is no evident tension, when there is not much complaint of pain, and the inflammation is not extending, you had better, in my opinion, leave these tumours to the care of nature, and address your remedies to the system at large. Support is almost always needed ; and opiates are sometimes indispensable ; and the bowels must be kept clear by purgatives. When the interference of surgery *is* requisite, Mr. Travers, Jun^{r}. advocated the destruction of the central integuments by caustic, rather than their division crosswise by the knife ; for this, among other reasons, that less hazard was so incurred of subsequent phlebitis.

I shall not attempt to discuss, even in this cursory and disjointed manner, any more of the inflammatory affections of the skin, whether acute or chronic : but I wish, before I conclude, to direct your attention to a peculiar morbid condition, of much greater consequence and interest than many of those which I have just been describing. I mean the malady which is best known by the appellation of *purpura*, or *the purples*, and which usually, though it must be confessed very incorrectly, is ranked among cutaneous disorders. It is strictly a hæmorrhage. Its external phenomena are so obvious, and so well known, that I need not dwell upon them. Small round spots appear on various parts of the surface, generally upon the legs first and most plentifully, of a dull crimson, or of a deep purple colour. They are accompanied by no local pain, by no sensation of any kind. Pressure upon them does not efface the colour, nor render it fainter, as it does that of inflammatory spots of the skin. There is scarcely ever any prominence of the purple stigmata; but they are sometimes intermixed with livid blotches, with appearances exactly resembling bruises: and both the circular spots and the ill-defined vibices undergo, before they disappear, the same changes of colour, from red or blue to a greenish yellow, which a bruise undergoes. In fact, the anatomical condition of a bruise is exactly the same with the condition of the diffused livid blotches of purpura. In each case the colour is the result of ecchymosis. With all this, passive hæmorrhages from various parts, and particularly from the mucous membranes, are common.

It is clear, therefore, that this complaint cannot be regarded as a cutaneous complaint, even in the loose sense in which that epithet is sometimes applied to affections which are really *beneath* the skin, but visible *through* it. The hæmorrhage takes the form of red or purple spots when the quantity of blood extravasated in the same place is only a drop. And the spots are not peculiar to the skin, nor to the subcutaneous tissues, but are found, occasionally, upon all the internal surfaces also, and within the substance of the several viscera. I have seen these purple spots on the mucous surface of the mouth, the throat, the stomach, and the intestines, on the pleuræ and pericardium in the chest, on the peritoneal investment of the abdominal organs, in the substance of the muscles, and even upon the membranes of the brain, and in the sheaths of the larger nerves: and I have known them to be accompanied with large extravasations of blood in most of the vital organs of the body.

The superficial markings of purpura, the red and purple spots and livid blotches, exactly resemble the spots and bruise-like stains which characterise sea-scurvy: and I confess that I formerly regarded the two affections as being identical, or as mere varieties of the same disorder. But it is not so. For a very full and interesting account of scurvy, I must refer you to an essay, by Dr. Budd, in the 'Library of Practical Medicine.' He has there collected from various sources, and exhibited in a clear light, convincing evidence that scurvy is caused—neither by contagion, nor by cold weather, nor by impurity of the air, nor by the continued use of salt provisions, all of which have been alleged as sources of the disease, but—by the privation, for a considerable length of time, of fresh succulent vegetables. Now purpura often makes its appearance when there has been no deficiency of such food, and no remarkable abstinence from it. Scurvy is most common in winter, or in the beginning of spring; purpura in the fruit seasons, in summer and autumn. In scurvy the gums are uniformly soft, and swelled, and spongy, and bleed readily; this is no necessary feature in purpura. Scurvy is marked by extreme debility and dejection of spirits, and is gradual in its approach: it is always rendered worse by blood-letting and by mercury; and it is infallibly and rapidly cured by the administration of lemon-juice, or of other fresh fruits and vegetables. Purpura, on the other hand, is apt to spring up all of a sudden; has been apparently cured by venæsection; it is not constantly nor surely, if ever, benefited by the antiscorbutic juices; it is not always attended by sponginess of the gums, nor by feebleness of the mind and body; and I have seen it clear speedily away upon the supervention of hypercatharsis.

Lemon-juice is really a specific against scurvy, whether it be employed as a preventive or as a remedy. It supplies something to the blood which is essential to its healthy properties. Its virtues were known in this country more than two hundred years ago, as appears from the work entitled 'The Surgeon's Mate, or Military and Domestic Medicine,' by John Woodall, Master in Surgery: London, 1636. But the merit of making the fact generally known, and of procuring the systematic introduction of lemon-juice into nautical diet, by an order from the Admiralty, is due to Dr. Blair, and Sir Gilbert Blane, in their capacity of Commissioners of the Board for Sick and Wounded Seamen, in 1795. 'The effect (says Sir John Herschel) of this wise measure may be estimated from the following facts. In 1780 the number of cases of scurvy received into Haslar Hospital was 1,457: in 1806 *one* only, and in 1807 *one*.' He adds, 'There are now many surgeons in the navy who have never seen the disease.'

Dr. Budd, however, has assured me that the Dreadnought

Hospital-ship, at Greenwich, was often full of cases of scurvy; most of the patients so affected having just arrived in *merchant ships*, from a long voyage. This surely ought not to be. It *could* not be if the owners of these vessels knew how easily, certainly, and cheaply this truly dreadful scourge may be averted.

Scarcely less—if indeed less at all—of antiscorbutic virtue, belongs, fortunately, to that common esculent root, the potatoe. *Raw* potatoes have long been in good repute, both for the cure and for the prevention of scurvy: but raw potatoes are neither palatable, nor easy of digestion; and it is a great discovery, which we owe to the sagacity of the late Dr. William Baly, that this vegetable is equally effective for these purposes, when cooked. During some months of continued observation of the prisoners confined in the Penitentiary at Millbank, I had remarked, without being able to account for it, that among the small number of *soldiers*, committed for comparatively short periods, for offences against military discipline, scurvy was not uncommon; whereas I noticed it in one instance only among the much more numerous class of *convicts*, whose term of imprisonment was considerably longer. Dr. Baly was afterwards appointed Physician to the Penitentiary, and the same curious fact soon caught his attention: and he traced the cause. By the examination and comparison of various dietaries—those, namely, which have been adopted at different periods in the Penitentiary itself, those which, at the same period, were prescribed respectively for the military offenders, and for the ordinary convicts, and those in use in sundry other gaols in which scurvy has occurred with different degrees of frequency—he showed, most satisfactorily, that the liability to that malady had a strict relation to the amount of succulent vegetables consumed by the prisoners, and especially of potatoes. 'Wherever this disease has prevailed, there the diet of the prisoners, though often abundant in other respects, has contained no potatoes, or only a very small quantity. In several prisons, the occurrence of scurvy has wholly ceased on the addition of a few pounds of potatoes being made to the weekly dietary. There are many prisons in which the diet, from its unvaried character, and the absence of animal food, as well as green vegetables, is apparently most inadequate to the maintenance of health; and where nevertheless, from its containing abundance of potatoes, scurvy is not produced.'

In corroboration of these views may be adduced the remarkable prevalence of scorbutic complaints in these islands, subsequently to the potatoe rot of 1846.

Now potatoes are *food* as well as medicine, and they are a cheap kind of food, and it may be hoped that a more general knowledge of their antiscorbutic properties, even when cooked, will abolish this wretched complaint, whenever a good supply of them is attainable. Dr. Baly believed that from three to six pounds, weekly, for each person, would suffice. He thus accounted for their salutary influence.

'A glance at the chemical analysis of the potatoe at once explains its antiscorbutic virtue. The various fruits, succulent roots, and herbs, which have the property of preventing and curing scurvy, all contain, dissolved in their juices, one or more organic acids—such as the citric, tartaric, and malic acids. Sometimes these acids exist in the free state, but more generally they are combined with potass, or lime, or with both these bases. Now potatoes have been submitted to most elaborate chemical examination by Einhoff and Vauquelin; and by both these chemists they have been found to contain a vegetable acid in considerable quantity. According to Einhoff, this acid is the tartaric combined with potass and lime. According to Vauquelin it is the citric, partly in combination with those bases, and partly in the free state. The farinaceous seeds, as wheat, barley, oats, and rye, which are destitute of antiscorbutic property, contain no organic or vegetable acids.'

My friend, the late Dr. Martin, of Ventnor, believed that the *cruciferæ*, and water-cress especially, had more speedy and sure effect in removing sea-scurvy, than even lemon-juice. He assured me that, at St. Helena, he had seen the worst forms of the disease cured in the space of three days by an abundant ingestion of water-cresses.

The same causes which give rise to *sea*-scurvy will produce precisely the same effect on land. Of this I must give you one illustration from my own case-book. In August, 1830, I admitted into the Middlesex Hospital a blacksmith, thirty-five years old, covered with round purple spots of various sizes, and with irregular blotches of ecchymosis. He had *vomited* blood on the preceding day. He was continually *coughing up* blood at the time of his admission, and his wife estimated the whole quantity that he had then lost to be more than half a pailful. The interior of his mouth and palate was pouring forth blood from a number of livid fungous tumours, formed by the extravasation of blood into the areolar tissue beneath the membrane, and the subsequent rupture of that membrane. He was passing blood by the *bowels* also; and his *urine* was loaded with blood.

Here were the *symptoms* of scurvy strongly marked. In the man's history we could trace its peculiar *cause*. He had long been subsisting on very poor and insufficient nutriment, seldom eating any meat, but living almost entirely on tea, coffee, and bread-and-butter. He had been too ill and weak to work regularly, yet he had been obliged occasionally to over-exert himself to obtain a scanty supply of food for himself, his wife, and a large family of children. He had been a settled dram-drinker, but for some time had taken much less of that stimulus; merely because he had not the means of procuring it. His pulse was frequent and feeble.

I had not much hope that this patient could be saved by any treatment. He was immediately put upon a diet of roast meat, and began to take daily half a pint of fresh lemon-juice diluted with a pint and half of water. This plan, with some tonic medicine, was commenced on the 3rd of August. He improved at once. On the 8th all hæmorrhage had ceased; the fungous tumours in the mouth had disappeared, leaving small scars in the places they had occupied; and the discoloration of the skin was almost gone. The amendment was so striking and rapid, and so immediately consequent upon the institution of the treatment, that no room was left for mistaking recovery for cure.

It is chiefly by investigating the previous history of the patient, and by noting the degree of strength that he possesses, and the condition of his pulse, that we are guided in our diagnosis of ambiguous cases. The late Dr. Parry, of Bath, was one of the first to point out the efficacy of abstinence, venæsection, and purgatives, in some instances at least, of purpura. I may refer you to an example of this kind detailed in the 'Medical Gazette' for the 5th of April, 1828. It occurred in one of Dr. Latham's hospital patients; and several of the symptoms were very like those I have just been relating. In particular the whole tongue was livid, one half of it presenting the appearance of a large, black, bleeding fungus; and on the inner surface of each cheek were several black fungoid patches. The patient was voiding also unmixed blood from the bowels. In this case there was no evidence of the operation of any debilitating cause, and the pulse, though frequent, was *hard*. Bleeding from the arm always gave relief to his uneasy sensations: he was purged also, and put upon low diet. Under this plan he steadily improved, and in four or five days no vestige of the complaint remained, except the fading spots. For some time afterwards, however, 'the frequent use of active purgatives, and a rigid restriction to low diet, were necessary to obviate costiveness, and to keep down the circulation, which had a tendency to become over-active.'

You are not to suppose that all cases of purpura bear this sthenic character, or require these heroic remedies. Your treatment must be guided by the previous circumstances and habits of the patient, by the state of his pulse, and by the other symptoms which accompany the purple spots. In many cases your chief reliance will be placed in the watchful employment of purgatives. These have been highly recommended by Dr. Harty, of Dublin, as having proved eminently successful in his practice. The late Dr. Whitlock Nicholl, and others, have spoken in terms of strong praise of the oil of turpentine, administered in moderate and repeated doses, as a remedy in purpura.

I have adverted to one peculiar source of danger in purpura, the hazard that blood may be effused in some vital organ where even a slight amount of hæmorrhage suffices to extinguish life. Dr. Bateman states that he had seen three instances in which persons were carried off, while affected with purpura, by hæmorrhage into the lungs. During the course of one week, in the year 1825, I was present at two inspections in the deadhouse of St. Bartholomew's Hospital, illustrative of the same point in respect of another vital organ, and involving a question in forensic medicine. The subjects of examination were both of them women of middle age, who had been brought into the hospital covered with purple spots and bruise-like discolorations, and suffering hæmorrhage from the mucous membranes. Each of these women declared that the apparent bruises were marks of beatings received from her husband. One of them became suddenly hemiplegic a little while before she died. Of the manner of dissolution in the other case I am not sure. In both instances a considerable quantity of blood was spread over the surface of the brain, between its membranes: and in one of them, blood had been shed also into the cerebral substance, which it had extensively lacerated.

It may be worth mentioning that in one of these corpses there were indications, either of unusually rapid putrefaction after death, or (what I think more probable) of some degree of decomposition even before life was extinct. This woman died in the evening, and the body was examined the next day, twelve or fourteen hours afterwards. A quantity of fœtid gas escaped from the cavity of the abdomen as soon as it was opened, and small bubbles of air were seen to ooze from the areolar tissue of various parts of the body. Even when incisions were made into the *liver*, air frothed up, as it might do, under ordinary circumstances, from a section of the lungs.

I have no time left for discussing the pathology of these complaints. They are eminently *blood-diseases.* In scurvy the blood is starved of some essential ingredient, which the juice of lemons, or other fresh succulent vegetable food, readily supplies and renews. When drawn from a vein the blood is often visibly unnatural. A very small quantity was taken before I saw him, from the arm of the blacksmith whose case I just now mentioned. After standing for some time, it continued to fill the whole area of the vessel in which it had been received, without any apparent contraction, or separation of serum. On its flat upper surface was a thick, gray, semitransparent jelly, and beneath this there was, strictly speaking, no coagulum, but a black, semifluid substance of the consistence of syrup. Huxham describes similar appearances. ‘The blood of such persons (says he), when it hath been drawn off, always appears a mere gore, as it were, not separating into crassamentum and serum as usual, but remaining in a uniform half coagulated mass, generally of a livid or darker colour than usual, though sometimes it continues long very florid ; but it always putrefies very soon.’ In another place, when describing a particular case, he says, ‘I found that neither of the portions of the blood that had been drawn had separated into serum and crassamentum as usual, though it had stood many hours; but continued, as it were, half coagulated, and of a blueish livid colour on the top. It was most easily divided on the slightest touch, and seemed a purulent sanies rather than blood, with a kind of sooty powder at the bottom.’

Dr. Budd, however, states that in some cases of scurvy the separation of blood into serum and clot is as perfect, and takes place as readily, as in healthy blood.

When you recognise the disease as genuine scurvy, and trace a previous abstinence, whether forced or voluntary, from fresh vegetables, the treatment is plain; you must supply the kind of nutriment which has been defective, and support your patient's strength in such other ways as the circumstances of the case may dictate.

And now, Gentlemen, I must needs stop. Here ends my course. And if this were all I had to say, I should say it with something like glee, and you, no less than myself, would rejoice that at length a breathing-space and holiday had arrived. But I cannot feel so when I add that this is the last lecture, not of this course merely, but the last of any kind, that I am ever likely to deliver in King's College. I cannot say this without concern and regret.

I am quite aware that my lectures have been in many respects imperfect. They have been very unequal to my own wishes. But they have been as full, and as carefully weighed, as my broken leisure, and irregular opportunities, and slender ability, would permit. I can only hope that at any rate I have not misled you. If I have been intelligible, if I have drawn such a sketch of a great and difficult subject as may help you in studying it for yourselves, I have achieved my task. I told you, in the outset, that I could not here teach you the *practice* of physic, but only its *principles.* It would be idle for me to speculate further upon the success of my endeavours. You are to be the judges of that matter. Whatever rules and precepts I have laid down, you will soon test by your own experience, and adopt or reject them accordingly. The well-known maxims of Bacon apply with especial truth to medical instructors and their pupils. 'Etsi non displiceat regula, *oportet discentem credere*; huic tamen conjungendum est, *oportet jam edoctum judicio suo uti*: discipuli enim debent magistris temporariam solum fidem, judiciique suspensionem, donec penitus imbiberint artes: non autem plenam libertatis ejurationem, perpetuamque ingenii servitutem.'

Retiring reluctantly from this place, in obedience to the force of circumstances, there are yet many things to comfort and console me. It is a great satisfaction to reflect that I have never had any serious disagreement with yourselves; have never experienced any but the most respectful and kind reception either from my present or from any preceding class. I have reason to thank you—and I do thank you—for the courtesy and attention you have at all times shown me. It is a source of gratification also that I carry with me the good will, as I believe, of my excellent colleagues; and that I go without having forfeited any of that confidence which the Council first reposed in me as their servant, when they offered me, without solicitation, the chair I now resign.

Had I been a few years younger, unembarrassed by previous official engagements, and somewhat more at leisure than I am, I should have been glad and proud to have attached myself to the new hospital, and to have laboured still in the cause of King's College, and of its Medical School. But it is otherwise ordered: and I will mention as the last source of consolation in taking leave of you, my conviction that to you my loss (if without presumption I may so venture to speak of my resignation) will be more than supplied by my successor. I know that gentleman well. I know, indeed the world knows, his talents. He was highly distinguished in the Senate House at Cambridge. He has since devoted, and will continue to devote, the powers of a very strong intellect, to the investigation of disease. Dr. Budd is one of the most strenuous cultivators of our science that I am acquainted with: and I am confident—without any affectation of modesty—that he will soon give a much better course of lectures than you have heard from me. That you may prosper under his instruction, and afterwards; that by the humane exercise of our noble calling, you may do good in your generation, to others, and so to yourselves; is my earnest desire and prayer. I hope it is unnecessary for me to assure you that I shall always continue to take a lively interest in your welfare individually; and that it will give me sincere pleasure if I shall find any future opportunity of rendering you any service. Gentlemen, I do not like this sort of parting, and I will not further protract the pain that belongs to it; but bid you finally, and most cordially—Farewell.

EPILOGUE.

TO THE READER.

I HAVE left the closing sentences of the last lecture as they stood when first printed; for I know not how to alter them. Feeling unable, and indeed unwilling, to recast, in my study, the style and character of these lectures, I have, in revising them, fed the pleasant fancy that I was still sitting in the Professor's chair, with my flock of juvenile students before me. This has of necessity led to anachronisms which you, my reader, will readily detect. My prophecy respecting my successor was amply fulfilled; and he also—too soon for science and for the public—has retired from the toils and responsibilities of medical practice. Of *his* successor, Dr. George Johnson—for whose friendly assistance in re-editing the Lectures I am under the deepest obligations—of him, I will only say that I think him unsurpassed for accomplishments and ability in his vocation. King's College may well be proud of him.

Considering the rapid advance of medical science during the last fourteen years, the present edition would be worthless if it did not differ much from the last. Working with many interruptions, and the slowness of old age, there are a few things in the first of these volumes which would have been somewhat differently put, if they had not been already printed while I was preparing the second. I will mention one only. Had I known what I now know about the effects of chloral in procuring sleep, I should have recommended it as preferable to opium in the treatment of delirium tremens.

INDEX.

ABDOMEN, diseases of the, ii. 415
— mode of examining by the eye, ii. 415
— — the hand, ii. 417
— — the ear, ii. 418
— encysted dropsy of, ii. 440, 445
Abdominal respiration, ii. 7
— dropsy, ii. 445
— tumours, ii. 797
Abscess, i. 182
— in the brain, i. 377
— in the lung, ii. 80
— in the liver, ii. 432, 661
Abscesses, consecutive scattered, ii. 380
— tropical, ii. 664
Absorption, interstitial, i. 176, 185
Acarus scabiei, ii. 1010
Accidental intrusion of solid substances into the air-passages, ii. 258
Acephalocysts, ii. 629
Acetate of potass in rheumatism, ii. 811
Acid, sulphuric, in painters' colic, ii. 567
Acids in scurvy, ii. 1033
Aconitine in tic-douloureux, i. 752
Active congestion, i. 49; ii. 497
— inflammation of the brain, i. 208
— hæmorrhage, i. 279; ii. 156
— dropsy, i. 300, 305
— idiopathic epistaxis, ii. 820
— aneurism of the heart, ii. 270
— ascites, ii. 439
Acupuncture of dropsical abdomen, ii. 461
— of anasarcous limbs, ii. 773
Acute inflammation of the brain, i. 207
— dropsy, i. 305; ii. 768
— arachnitis, i. 344
— hydrocephalus, i. 389
— tetanus, i. 565
— symptomatic tetanus, i. 557
— rheumatism, ii. 802
— laryngitis, i. 864
— bronchitis, ii. 31
— pneumonia, ii. 106
— phthisis, ii. 226
— articular rheumatism, its connexion with pericarditis, ii. 323
Acute inflammation of the pericardium, ii. 323, 353
— — of the endocardium, ii. 323, 353
— pyæmia, ii. 383
— — of the stomach, ii. 464
— — of the liver, ii. 658
— yellow atrophy of liver, ii. 665
— pericarditis, ii. 323, 353
— endocarditis, ii. 323, 353
— peritonitis, ii. 418
— gastritis, ii. 464
— dysentery, ii. 606
— hepatitis, ii. 658
— nephritis, ii. 690
— cystitis, ii. 715
— desquamative nephritis, ii. 690, 746
— anasarca, ii. 766
— urticaria, ii. 1003
— pemphigus, ii. 1021
Acuteness of hearing, a symptom, i. 136
Addison's disease, ii. 792. See *Supra-renal Capsules, disease of*
Adeps in diarrhœa, ii. 609
Adhesions of the pleuræ, ii. 203
Adhesive inflammation, i. 177
— pleurisy, ii. 118, 203
— phlebitis, ii. 385
Ægophony, ii. 19, 126
Æther, inhalation of, i. 130
— — in asthma, ii. 405
— as a remedy for spasm, i. 663
Æthmoid bone, disease of, i. 342, 344
Afferent nerves in palsy, i. 537
Age, atrophy in old, i. 26
— its influence in phthisis, ii. 236
Agraphia, i. 490, 493
Ague, i. 756. See *Intermittent Fever*
— leaping, i. 694
— brow, i. 728
— thought salutary, i. 794
— the cold, the hot, and the sweating stages, i. 756
— quotidian, i. 759
— tertian, i. 759
— — double, i. 761
— secundan, i. 760

Ague, quartan, i. 760
— treatment, i. 800
— protection against, i. 815–817
— formerly rife in London, ii. 605
— rules respecting, i. 764
— predisposing causes, i. 766
— cake, i. 794; ii. 499, 688
— drop, tasteless, in intermittents, i. 810
Air, impurity of, a cause of disease, i. 113
— dangers of allowing air to enter into the external jugular vein, i. 254
— press, Dr. Arnott's, i. 159
— bath, hot, in ague, i. 800
— — in anasarca, ii. 771
— effects of, in cretinism, i. 834
— in the chest, ii. 150
— passages, intrusion of solid substances into, ii. 258
— — inflammation of the, i. 203
Albuminous urine, ii. 741, 755
— means of detecting the albumen, ii. 742–744
— nephritis, ii. 756
Albuminuria, ii. 744
Alcohol, effects of, on the liver, ii. 449
Alisma plantago in rabies, i. 626
Alterations in bulk, i. 17
— in consistence, i. 29
Altered sensations, i. 134
Alternate rubbing sounds of the heart, ii. 330
Alternating generations, ii. 624
Alum, its use in colic, ii. 564
Alveolar cancer, i. 240
Amadou plaster, to prevent bedsores, ii. 909
Amenorrhœa, a cause of hysteria, i. 720
Ammonia, muriate of, in face-ache, i. 753
— — in hepatic disease, ii. 671
Ammoniacal urine, ii. 711
Amphoric resonance, ii. 209
Amygdalitis, i. 852
Anæmia, i. 51
— death by, i. 65, 288
— anatomical characters of, i. 65
— phenomena, i. 66
— accidents and diseases in which it occurs, i. 66
— of the brain, i. 414
— common in chorea, i. 676
— in exophthalmic goître, i. 839
Anæsthesia a symptom, i. 134
— of one side, i. 541
— facial, i. 541
— of other parts, i. 553
— in palsy, i. 722
Anæsthetic agents, i. 130
Anasarca, i. 292; ii. 750, 758
— phenomena, ii. 759
— general symptoms, ii. 760
— cardiac, ii. 759, 760
— with what conditions of the heart it is connected, ii. 761
— relations between anasarca and pulmonary diseases, ii. 767
— renal, ii. 767
— acute, ii. 768, 906
Anasarca, chronic, ii. 768
— treatment of cardiac dropsy, ii. 769
— — of renal, ii. 770
— mechanical expedients, ii. 773. See *Dropsy*
— a consequence of scarlet fever, ii. 979
Anazoturia, ii. 740
Aneurism, mortification from attempt at cure of, i. 189
— of the thoracic aorta, i. 871
— of the heart, ii. 270
Aneurisms, thoracic, i. 871; ii. 361
— their various situations, ii. 362
— — treatment, ii. 373
Angina parotidæa, i. 844
— pectoris, i. 743; ii. 313
— numerical account of the organic changes in, ii. 316
— prominent and principal elements in, ii. 317
— preventive measures, ii. 320
— treatment, ii. 320, 321
Animal putrefaction, i. 774
— sugar, ii. 733
Aniseed, in phthisis, and cough, ii. 249
Ankles, œdema of, in phthisis, ii. 221
Anorexia, a symptom of dyspepsia, ii. 512
Anthrax, ii. 1029
Antimony as a remedy in inflammation, i. 270
— in croup, i. 887
— — pneumonia, ii. 104
Antiphlogistic regimen, i. 243
Antiscorbutics, ii. 1033
Anus, artificial, ii. 550
Aorta, aneurism of, thoracic, i. 871; ii. 361
— disease of, ii. 304, 360
Aphasia, i. 490
— phenomena of, i. 491
— amnemonic, i. 492
— atactic, i. 492
— combination of, with hemiplegia, i. 499
Aphemia, i. 490
Aphonia, i. 490, 722
— in hysteria, i. 722
Aphthæ, i. 849; ii. 221
— treatment of, i. 851
Apnœa, death by, i. 69
— causes of, i. 71
— phenomena, i. 74
— anatomical characters, i. 76
— general pathology of, i. 75
— diseases in which this mode of dying occurs, i. 75
Apoplexy, general symptoms, i. 453
— diagnosis, i. 456
— modes of attack, i. 457
— simple, i. 456
— sanguineous, i. 456
— serous, i. 457
— general diagnosis, i. 504
— predisposing circumstances, i. 505
— premonitory signs, i. 512
— symptoms, i. 514

Apoplexy, physical causes of, i. 516
— morbid anatomy, i. 518
— predisposition to, i. 517
— exciting causes, i. 516
— treatment, i. 521
— spinal, i. 530
— pulmonary, i. 530; ii. 161
— hepatic, i. 530; ii. 658
Appetite, inordinate, in diabetes, i. 134
— perverted in chlorosis, i. 134
— loss of, ii. 512
Arachnitis, i. 344
— acute, i. 344
Arachnoid, inflammation of, i. 335
Arachnoidean hydrocephalus, i. 435
Arcus senilis, its connexion with fatty degeneration, i. 38
Areola of small-pox, ii. 939
Areolar tissue, i. 33
— — inflammation of, i. 198, 199; ii. 803
— — softening of, i. 33
Arsenic in chorea, i. 684
— as a remedy for ague, i. 804, 810
— in hay-asthma, ii. 57
Arseniate of soda in ague, i. 811
Arsenite of potass in intermittents, i. 810
Art and science, i. 2
Arteries, inflammation of, i. 204
Arterial blood liable to buffy coat, i. 171
Arteriotomy, i. 246
Articular rheumatism, acute, its connexion with pericarditis, ii. 323
Artificial respiration in poisoning, i. 77
— anus, ii. 550
— diabetes, ii. 729
Ascarides, ii. 617
Ascaris lumbricoïdes, ii. 617, 647
— vermicularis, ii. 617, 648
Ascites, i. 290
— active, ii. 438
— diagnosis, ii. 442. See *Ovarian Dropsy*
— chronic, ii. 446
— — pathology, ii. 446
— — causes, ii. 446
— — treatment, ii. 453
— hepatic, ii. 453
Asiatic cholera, ii. 573
Asphyxia, death by, i. 69
— from cold douche, i. 354
Aspirator, the, ii. 633
Asthenia, death by, i. 65
— anatomical characters of, i. 66
— opium useful in preventing, i. 273
— phenomena, 66
— diseases in which this mode of dying occurs, 66
— tendency to death by, in gastritis, ii. 465
Asthenic hyperæmia, i. 58
Asthma, a symptom, i. 141
— thymic, i. 903
— hay, ii. 54, 362
— caused by emphysema, ii. 183
— grinders', ii. 238
— spasmodic, ii. 77, 187, 393
— symptoms, ii. 393
Asthma, dry, ii. 395
— humid or humoral, ii. 395
— proofs of its spasmodic character, ii. 395
— associated with organic changes, ii. 398
— causes, ii. 399
— treatment, ii. 403
— prevention, ii. 407
Astringents in hæmorrhage, i. 288
Atheroma, i. 39
Atmosphere, its effects on disease, i. 83
— impurity of, a cause of disease, i. 113; ii. 46
Ataxic fever, ii. 864
Ataxy, locomotor, i. 535, 695. See *Locomotor Ataxy*
Atonic gout, ii. 821
Atrabilis of ancients, ii. 506
Atrophy, i. 17, 26, 185
— its causes, i. 26
— — consequences, i. 28
— of the brain, i. 386
— — the heart, ii. 297
— — the liver, ii. 665
Attraction, heterogeneous, i. 294
Aura epileptica, i. 636, 650
Aurigo, ii. 675
Auscultation, general account of, ii. 8
Autumnal ague, i. 764
— intermittents, ii. 764
— fevers, ii. 776
Axillary region, ii. 14
Azote, effect of food without, i. 35
Azoturia, ii. 770

BANDAGING the head in hydrocephalus, i. 427
Bantingism, ii. 188
Bark, Peruvian, as a remedy for ague, i. 804
— cultivation of, in India, i. 809
— willow, in ague, i. 811
Bath, shower, in chorea, i. 683
— — in colds, ii. 31
— hot-air, in ague, i. 800
Bastard peripneumony, ii. 41
Bedsores, amadou plaster to prevent, ii. 909
Beer treacle, its use in lead poison, ii. 567
Beet-root as food, effect of, ii. 781
Belladonna as a remedy for iritis, i. 265
— in tetanus, i. 582
— in rabies, i. 626
— in scarlet fever, ii. 990
Bellain, ii. 556
Bellows-sound of the heart, ii. 279
— — double, ii. 304
Belly-ache, dry, ii. 556, 558
Bezoars, ii. 612
Bile, black, ii. 500
— in diarrhœa, ii. 517
— suppression of, ii. 673
— retention of, ii. 673
Bilharzia, ii. 788
Biliary concretions, ii. 678
Bilious headache, ii. 522
— fever, ii. 864
Bitters, remedy for worms, ii. 649

BLA

Black-spit, ii. 253. See *Melanosis*
— drop in peritonitis, ii. 435
— bile, ii. 500
— vomit, ii. 501
— urine, ii. 781
Bladder, urinary, rupture of, ii. 432
— gall, rupture of, ii. 432
— — diseases of, ii. 715
— worms, ii. 624
Bladderwrack in bronchocele, i. 835
Blanket-bath in anasarca, ii. 771
Blebs, ii. 840, 1020
Bleeding piles, ii. 568
Blindness, a symptom, i. 135
Blood, diseases of, i. 45; ii. 1037
— changes in, i. 47
— plethora, i. 49
— poverty of, i. 51
— active congestion, i. 54
— mechanical congestion, i. 59
— passive congestion, i. 59
— constituents of the, i. 52
— phenomena of circulation of the, i. 54
— state of, in inflammation, i. 164, 168
— vessels in inflammation, i. 168
— stagnation, i. 164
— buffy coat of, i. 164, 169
— mercury in, ii. 377
— disease, acute rheumatism, i. 802
— — in hæmatemesis, ii. 497
— vomiting of, its symptoms, ii. 501
Blood-letting, general account of, as a remedy of inflammation, i. 246
— true philosophy of, in disease, i. 254
— in dropsy, i. 308
— as a remedy for iritis, i. 265
— in encephalitis, i. 355
— — hydrocephalus, ii. 408
— — apoplexy, i. 522
— — tetanus, i. 577
— — ague, i. 801
— — laryngitis, i. 867
— — influenza, ii. 52
— — pneumonia, ii. 103
— — pulmonary hæmorrhage, ii. 170
— — heart disease, ii. 288
— — pericarditis, ii. 355
— — endocarditis, ii. 355
— — peritonitis, ii. 433
— — gastritis, ii. 469
— — continued fever, ii. 916
Bloody flux, ii. 603, 606
— small-pox, ii. 934
— urine, ii. 695, 780
Blowing-sound, systolic, ii. 280
Blue line on gums, ii. 560
Boils, ii. 1027
Bones, softening of the, i. 34
Borax in aphthæ, i. 851
Bothriocephalus punctatus, ii. 623
— latus, ii. 626, 655
Bowel, intus-susception of, i. 42
— twisted, ii. 550
Bowels, inflammation of, ii. 530. See *Enteritis*

BRO

Bowels, mechanical obstruction of, ii. 538
Brain, disordered functions of, i. 134
— inflammation of the substance of, i. 206; ii. 876
— and nervous system, general remarks on diseases of, i. 311
— effects of pressure on, i. 320
— — varying pressure, i. 326
— symptoms of cerebral disease, i. 330
— inflammation of, i. 331, 334, 349. See *Encephalitis*
— softening of, i. 374
— white softening of, i. 380, 477, 479
— ramollissement of, i. 374
— abcess in, i. 377
— suppuration of, i. 377
— induration of, i. 377
— tumours in, i. 378
— hypertrophy of, i. 383
— atrophy of, i. 386
— aphasia, i. 490
— fever, ii. 794
Brain-case, enlargement of the, i. 384
Brain-like cancer, i. 231
Bran-loaf in diabetes, ii. 734
— cakes in diabetes, ii. 735
Branks, or mumps, i. 844
Breast, hysterical, i. 723
— pang, ii. 314
Breathing, thoracic, ii. 4
— abdominal, ii. 7
— puerile, ii. 17, 89
— bronchial, ii. 18
— vesicular, ii. 18, 24
Bright's kidney, ii. 744
— disease, ii. 744
— anatomical characters, ii. 745
— signs of, ii. 746, 747
— changes in the urine, ii. 748
— — in the blood, ii. 750
— course of the disease, ii. 755
— secondary affections, ii. 758
— causes, ii. 757
— nature, ii. 758. See *Anasarca*
Broken wind in horses, ii. 183
Bronchi, dilatation of, ii. 65
Bronchial polypi, i. 202; ii. 61
— respiration, ii. 18, 86, 88
— voice, ii. 88
— hæmorrhage, ii. 160
Bronchitis, ii. 23
— acute, ii. 31
— morbid anatomy, ii. 34
— treatment, ii. 35
— chronic, ii. 58
— varieties of, ii. 58
— constant and variable symptoms, ii. 59
— morbid anatomy, ii. 65
Bronchocele, i. 821
— effects of, i. 823
— endemic, i. 825
— supposed causes of, i. 827
— its treatment, i. 833
Broncho-hæmorrhage, ii. 162
Bronchophony, ii. 18, 88, 127

Bronchotomy as a remedy for hydrophobia, i. 621
Bronchus, sudden infarction of, ii. 40
Bronzed skin, ii. 793
Brow ague, i. 728
Bruit de pot félé, ii, 213
— de soufflet, ii. 279, 371
— de scie, ii. 280
— de râpe, ii. 280
— de diable, ii. 287
Buffy coat of the blood in inflammation, i. 169
— not shown by blood drawn by leeches, i. 171
Bullæ, ii. 840, 1020
Buzzing, effect of quina, i. 807
Burnt sponge in bronchocele, i. 835

CACTUS opuntia, ii. 780
Calculus, mulberry, ii. 711, 712, 714
— fusible, ii. 709
— of the kidney, ii. 787
Callous, or indolent, ulcer, i. 185
Calomel as a remedy for tracheitis, i. 891
— in croup, i. 886
Cancer, i. 230
— scirrhous, i. 231
— medullary, or encephaloid, i. 231
— epithelial, i. 231
— its growth and dissemination, i. 233
— open, i. 233
— its varieties, i. 236
— habitudes of the several varieties, i. 237
— its origin, i. 238
— affinities between cancer and tubercle, i. 239
— colloid, or gum, cancer, i. 240
— of lung, ii. 257
— — stomach, ii. 431, 486
— — incipient, ii. 505
— — advanced, ii. 506
— hereditary, ii. 491
— its symptoms, ii. 486, 492
— — situation, ii. 488
— — treatment, ii. 493
Cancer-juice, i. 231
Cancerous tumours in brain, i. 379
— liver, ii. 800
Candle-snuff in ague, i. 813
Canine madness, i. 606. See *Hydrophobia*
Cantharides, inflammation by, i. 201
Capillary hæmorrhage, i. 276, 819; ii. 495
— — gastric, ii. 496
Carbonate of iron as a remedy for tetanus, i. 583
Carbuncle, ii. 1029
Carcinoma, i. 231; ii. 227, 410
— of the stomach, ii. 486
— its symptoms, ii. 486, 492
— — situation, ii. 488
— — diagnosis, ii. 493
— — treatment, ii. 493
Cardiac disease, its signs, ii. 285
— dropsy, i. 304; ii. 759, 760. See *Anasarca*
Cardialgia, ii. 515
Carditis, rheumatic, ii. 323, 325
— anatomical account of, ii. 326
— its relations with rheumatic fever, ii. 338
— head symptoms, ii. 340
— relations of carditis with articular rheumatism, ii. 348
Carnified lung, ii. 37, 114, 373
Catalepsy, i. 735
Catarrh, ii. 21
— its varieties, ii. 22, 29
— — treatment, ii. 29
— prevention of, ii. 31
— senile, ii. 41
— epidemic, ii. 43. See *Influenza*
— — migration of, ii. 48
— sporadic, ii. 48
— chronic, ii. 58. See *Bronchitis*
— — varieties of, ii. 58
— — cough of, ii. 215
Catarrhal stage of hooping-cough, ii. 68
Catarrhus senilis, ii. 41, 132
— e contagio, ii. 43
— æstivus, ii. 54
Caterpillars in the intestines, ii. 643
Cats, rabies in, i. 597, 605
Causes of diseases, i. 80
— proximate, i. 80
— exciting, i. 82
— predisposing, i. 81
— heat, i. 86
— cold, i. 94
— seasons, i. 109
— impurity of the air, i. 113
— hereditary predisposition, i. 115
Caustic in rabies, i. 623
Cavernous respiration, ii. 208
Cellular membrane, diffused inflammation of the, i. 199
Centric tetanus, i. 570
Cerebellum, i. 315
— disease of the, i. 337
Cerebral diseases, i. 311. See *Brain*
— disease, symptoms of, i. 138, 330
— peculiarities of the cerebral circulation, i. 321
— hæmorrhage, i. 456, 473. See *Apoplexy*
— epilepsy, i. 643
Cerebrum, i. 315
Cestoid worms, ii. 618, 637
Chalk mixture in diarrhœa, ii. 571
— stones, i. 210; ii. 818
— — chemical composition of, ii. 819
Chamomile in ague, i. 813
Change of colour, a symptom, i. 145
Changes of situation of solid parts, i. 39
Charcoal in the cure of intermittent fever, i. 813
Charms, their effect in curing ague, i. 814
Chest, tapping the, ii. 133, 140, 175
— cold in, ii. 23
— pain of, in phthisis, ii. 218
Chicken-pox, ii. 962
—phenomena of, ii. 963

CHI

Chicken-pox, treatment of, ii. 964
Chigoe, ii. 617
Child-crowing, i. 882, 903
— symptoms, i. 904
— diagnosis, i. 906
— treatment, i. 906
Chimney-sweep's cancer, i. 239
Chin-cough, ii. 68. See *Hooping-cough*
Chloral in acute bronchitis, ii. 36
— — continued fever, ii. 919
— — delirium tremens, ii. 1040
Chlorides in hay-asthma, ii. 57
Chlorine in puerperal fever, ii. 427
— as a remedy for scarlet fever, ii. 987
Chloroform an anæsthetic agent, i. 130
— in hydrophobia, i. 621.
— — asthma, ii. 404
Chlorotic girls, their appetites, i. 134
Choking, death by, i. 69
Cholera, English, i. 91; ii. 571
— sporadic, ii. 571
— summer, ii. 571
— morbus, ii. 573
— Asiatic, ii. 573
— epidemic, ii. 573
— malignant, ii. 573
— spasmodic, ii. 573
— symptoms, ii. 574
— morbid anatomy, ii. 576, 591
— a new disease in the country, ii. 576
— mode of propagation, ii. 577
— predisposing causes, ii. 579
— modes of attack, ii. 580, 590
— treatment of, ii. 581
— contagious nature of, ii. 584
— the atmosphere a vehicle of infection, ii. 584
— water also a vehicle, ii. 588
— importance of the law of altitude, ii. 586
— evacuant or cleansing theory of treatment, ii. 594
Cholesterine, ii. 609
Chorea, or St. Vitus's dance, i. 667
— its symptoms, i. 667
— — pathology, i. 670
— in pregnancy, i. 673
— — complications, i. 674
— — causes, i. 679
— — treatment, i. 682
— chronic, i. 685
— Sancti Viti, i. 687
Chronic inflammation of the brain, i. 207, 374
— hydrocephalus, i. 417
— — epilepsy attendant on, i. 646
— tetanus, i. 563
— chorea, i. 685
— affections of larynx, i. 887
— enlargements of tonsils, i. 861
— catarrh, ii. 58. See *Bronchitis*
— — cough of, ii. 215
— mucous catarrh, ii. 58
— pituitous catarrh, ii. 58
— bronchitis, ii. 58. See *Bronchitis*
— phthisis, ii. 226

COL

Chronic inflammation of pericardium, ii. 303
— — of the stomach, ii. 471
— — of the liver, ii. 667
— peritonitis, ii. 443, 436
— ascites, ii. 446
— gastritis, ii. 471
— ulcers of the stomach, ii. 431
— hepatitis, ii. 667
— cystitis, ii. 716
— diuresis, ii. 738
— general dropsy, ii. 676, 773
— rheumatism, ii. 814
— urticaria, ii. 1004
— pemphigus, ii. 1021
Chyle, ii. 511
Chylous urine, ii. 777
Chyme, ii. 509
Cicatrix, lung, ii. 187
Cinchona in ague, i. 805
Circulation, disordered functions of, symptoms of, i. 142
— peculiarities of the cerebral, i. 321
Circumscribed pulmonary hæmorrhage, ii. 154
Cirrhosis of liver, ii. 447, 596
— causes of, ii. 449
Clavus hystericus, i. 728
Climate in phthisis, ii. 241
Clonic spasm, i. 556
Closure of lung, death by, ii. 42
Coagulable lymph in inflammation, i. 176
Cod's-liver oil in phthisis, ii. 245
Colchicum as a remedy for acute rheumatism, ii. 814
— — iritis, 342
— — gout, ii. 833
Cold, degree of, compatible with life, i. 92
— a cause of disease, 94
— effects of extreme, on the system, i. 96
— in inducing sleep, i. 96
— laws by which the operation of cold upon the bodily health is regulated, i. 100
— benumbing power of, in surgical operations, i. 132
— in inflammation, i. 258
— in hæmorrhage, i. 288
— in encephalitis, i. 354
— in hydrocephalus, i. 410
— a cause of apoplexy, i. 520
— in peritonitis, ii. 433
— in the head, ii. 22
— in the chest, ii. 22
— crying, ii. 55
Cold bath, in tetanus, i. 577
— douche, in encephalitis, i. 354
— — in hysteria, i. 732
— affusion, in fever, ii. 914
Colic, ii. 554, 556
Colica pictonum, ii. 171, 556
— — its cause, ii. 558
— — its symptoms, ii. 557
— — its treatment, ii. 563
— — its prevention, ii. 566
Colique métallique, i. 705

Collapse of lung, in bronchitis, ii. 37
— — diffused, i. 38
— — lobular, i. 38
— — — in hooping-cough, i. 71
— — in cholera, ii. 574
Colliers' phthisis, ii. 251
Colliquative purging, ii. 220
Colloid, or gum, cancer, i. 240
Coma, death by, i. 76, 454; ii. 995
— phenomena, i. 76, 463
— diseases in which this mode of dying occurs, i. 78; ii. 877
— from cold, i. 96
— sensation of pain in inflammation prevented or abolished by, i. 159
— causes of, i. 466
— hazard of, in using opium, i. 272
— vigil, ii. 869
Commemorative symptoms, i. 127, 365
Compression in hydrocephalus, i. 434
Concentric hypertrophy of the heart, ii. 270
Concocted expectoration, ii. 33
Concretions, intestinal, ii. 535, 612
— biliary, ii. 678
Confluent small-pox, ii. 930
Congestion, active, i. 54; ii. 497
— local, i. 53
— mechanical, i. 57
— passive, i. 59; ii. 497
— — gastric hæmorrhage from, ii. 497
— its relation to other diseases, i. 61
— of blood, i. 54, 59
— hepatic, i. 656
Conopeum in ague, i. 817
Consecutive scattered abscesses, ii. 380
— enteritis, ii. 535
Constipation, ii. 547
Consumption, ii. 191. See *Phthisis*
— its connexion with scrofula, ii. 191
Contagion, ii. 45
— in phthisis, ii. 238
— ii. 845, 848, 863
Contagious exanthemata, ii. 842
— fevers, ii. 854, 863
— furunculoid, ii. 1059
Continued fever, ii. 865, 896. See *Fever*
Contracted kidney, ii. 746
Convulsions, a symptom, i. 140
— general, i. 346
Coqueluche, ii. 68. See *Hooping-cough*
Cord, spinal, general pathology of, i. 435
— inflammation of, i. 441
Cordials in ague, i. 799
Corpora striata, i. 315
Corpuscular lymph, i. 180
Coryza, ii. 23
Costiveness, ii. 522
Cough, a symptom, i. 141; ii. 19
— hysterical, ii. 723
— its purpose, ii. 6
— stomach, ii. 7, 215
— how modified, ii. 19
— produced by ipecacuan dust, ii. 57
Cough, hooping, ii. 68
— of pleurisy, ii. 120
— of phthisis, ii. 215
— of chronic catarrh, ii. 218
Counter-irritation in the treatment of inflammation, i. 174, 260
— — acute bronchitis, ii. 36
Coup de soleil, i. 93, 526. See *Sunstroke*
Cow-pox, ii. 939
Crab-lice, ii. 1008
Cracked-pot sound in phthisis, ii. 213
Crackling of pneumonia, ii. 84
Cramp, i. 557
— of the stomach, ii. 519
Cranio-spinal axis, i. 315
Creasote in diabetes, ii. 744
Crepitant rhoncus, ii. 841
Crepitation, large and small, ii. 27
— minute, ii. 84
— dry, ii. 182
Cretinism, i. 826, 831
— its connexion with goître, i. 826, 831
Croup, membranous exudation of, i. 202, 902
— or infantile laryngitis, i. 881
— — remedies, i. 886
— spurious, i. 903
Cruciferæ in scurvy, ii. 1034
Crude tubercles, i. 212, 215
— — in brain, i. 380
— expectoration, ii. 33
Crusta lactea, ii. 1027
Crying cold, ii. 55
Crystalli, ii. 963
Cucurbitine worms, ii. 621
Cultivation reduces malaria, i. 792
Cupping, as an art, i. 247, 255
— for apoplexy, i. 522
— in inflammation, i. 260
Cutaneous diseases, ii. 839
— — causes of some, i. 93
— — kinds of, ii. 839
— hæmorrhage, i. 275
— tissue, inflammation of, i. 201
Cutis anserina, i. 94; ii. 1006
— in ague, i. 757
Cyder, productive of colic, ii. 556
Cynanche maligna, i. 188; ii. 972. See *Scarlet Fever*
— parotidæa, i. 844, 856
— tonsillaris, i. 852
— pharyngea, i. 853
— laryngea, i. 856, 864. See *Laryngitis*
Cyst worms, ii. 624
Cystic entozoa, ii. 621
— oxide diathesis, ii. 715
Cysticercus cellulosæ, ii. 626, 628
— fasciolaris, ii. 625
— pisiformis, ii. 626
Cystitis, ii. 715
Cysts in the liver, ii. 445
— in the kidney, serous, ii. 445
— dermoid, ii. 453

DANCE, St. Vitus's, i. 667. See *Chorea*
Death, i. 62
— different modes of, i. 62
— beginning at the heart, i. 64 ; ii. 466
— — lungs, i. 68, 71
— sudden, pathology of, i. 65
— — by anæmia, i. 65
— — by asthenia, i. 66
— — by syncope, i. 67
— — by starvation, i. 68
— — by inanition, i. 68
— — by asphyxia, i. 69
— — by apnœa, i. 69
— — by suffocation, i. 69
— — by coma, i. 76
— — by sudden closure of lung, i. 80
Debility, muscular, a symptom, i. 139
Decubitus of pleurisy, ii. 120
Degeneration, fatty, i. 37 ; ii. 301
— — of liver, i. 38 ; ii. 206
— — of the heart, ii. 301
— granular, of the kidney, ii. 748
— of the omentum, ii. 798
Delirium, i. 346
— attendant on inflammation of membranes of the brain, i. 346
— ferox, i. 364
— traumatic, i. 365
— a symptom of pneumonia, ii. 93
— in heart disease, ii. 329
— of typhus fever, ii. 870
— tremens, i. 359
— — its peculiar character, i. 360
— — causes, i. 363, 365
— — treatment, i. 360, 368, 373 ; ii. 1040
— line between encephalitis and delirium tremens, i. 371
Delitescence, i. 174
Dentition, cause of chorea, i. 681
Derbyshire neck, i. 821
Derivation, i. 174
Dermoid cysts, ii. 453
Descending colon in dysentery, ii. 602
Desquamation, i. 201
Desquamative nephritis, acute, ii. 690, 746
Determination of blood, i. 164
Devonshire colic, ii. 556
Diabetes, ii. 718
— qualities of the urine in, ii. 719
— — its specific gravity, ii. 723
— anatomical appearances, ii. 726
— its origin, ii. 726
— general pathology, ii. 726
— causes, ii. 730
— treatment, ii. 732
— insipidus, ii. 719, 738
— mellitus, ii. 719
— artificial, ii. 729
Diagnosis of disease, i. 122
Diagonal posture in pleurisy, ii. 120
Diarrhœa caused by hot weather, i. 91 ; ii. 571
— in phthisis, ii. 220, 568
— its kinds, ii. 568
— crapulosa, ii. 569
Diarrhœa, its causes, ii. 569
— — treatment, ii. 572
— dysenteric, ii. 602
— adiposa, ii. 609
— — symptoms, ii. 610
— in typhoid fever, ii. 881
Diastolic sound of the heart, ii. 277
— bellows-sound of the heart, ii. 281
Diathesis, scrofulous, i. 211, 221
— cancerous, i. 238
— lithic, ii. 702
— phosphatic, ii. 709
— oxalic, ii. 712
— cystic oxide, ii. 715
Diffused inflammation of the cellular tissue, i. 199
— suppuration of pulmonary texture, ii. 80
— rheumatism, i. 805
Digestion, physiology of, ii. 509
Digitalis in tetanus, i. 582
Dilatation of bronchi, ii. 65
— of heart, ii. 295
— of œsophagus, ii. 413
Diluent drinks in ague, i. 779
Diphtheria, i. 881
— characters of, i. 889
— spreads by contagion, i. 892
— sequelæ of, i. 894
— diagnosis, i. 899
— treatment, i. 899
Direct and indirect symptoms, i. 122
— percussion, ii. 10
Discrete small-pox, ii. 928
Disease, use of the word, i. 6
— causes of, i. 80
— hereditary tendencies to, i. 115, 224
— signs of, i. 126
— cerebral, i. 311. See *Brain*
— spinal, i. 435, 441. See *Spinal Cord*
— of the supra-renal capsules, ii. 792
— thermometry of, ii. 862
— varioloid, ii. 931
Diseases of the fluids, i. 43
— of the blood, i. 43 ; ii. 855
— of the veins, i. 204 ; ii. 376
— of the brain and nervous system, i. 311
— spasmodic, i. 719
— of the thorax, ii. 1
— of the heart, ii. 268
— of the aorta, ii. 361
— of the œsophagus, ii. 407
— of the abdomen, ii. 415
— of the liver, ii. 656
— of the gall-bladder, ii. 689
— of the spleen, ii. 689
— of the pancreas, ii. 689
— of the skin, ii. 839
— contagious, febrile, ii. 854
— infectious, ii. 855
— zymotic, ii. 857
— of the kidneys, ii. 690
Disordered functions, i. 135
— — of the brain and senses, i. 135
— — of sight, i. 135
— — of hearing, i. 136

Disordered functions of the intellect, i. 138
— — of voluntary motion, i. 139
— — of the circulation, i. 142
Displacement of parts, i. 41
Diuresis, chronic, ii. 738
Diuretics in ovarian dropsy, ii. 454
Dizziness, a symptom, i. 133
Dolor atrox, ii. 555
Dorsal region, ii. 15
Double pleurisy, i. 175
— consciousness, i. 738
— tertian ague, i. 761
— bellows-sound, ii. 304
Douche, cold, in encephalitis, i. 354
— — in hysteria, i. 731
Dracunculus, ii. 637
Dropped-wrist, ii. 556
Dropsy, relations of congestion to, i. 61
— its general pathology, i. 290
— varieties of, i. 292
— general, i. 294
— passive, i. 296, 305
— cardiac, i. 304; ii. 759, 760
— active, i. 300, 305
— renal, i. 300, 304; ii. 760, 765
— phenomena, i. 305
— prognosis, i. 308
— treatment, i. 308
— of the brain, i. 417
— spinal, i. 540
— of the chest, ii. 112
— of the belly, ii. 440
— ovarian, ii. 440. See *Ovarian Dropsy*
— abdominal, ii. 445
— of the abdomen, encysted, ii. 445
— omental, ii. 445
— of the Fallopian tubes, ii. 445
— of the uterus, ii. 445
— of the kidney, ii. 445, 760
— inflammatory, i. 307; ii. 759
— febrile, i. 307; ii. 979
— chronic, ii. 759, 766
— cardiac, ii. 759, 760
— renal, ii. 760, 765
— acute, ii. 766
— after scarlet fever, ii. 988
— See *Anascarca; Dropsy; Ovarian Dropsy*
Drowning, death by, i. 69
Dry cure in coryza, ii. 30
— sounds in respiration, ii. 22
— catarrh, ii. 58
— pleurisy, ii. 118, 136
— crepitation, ii. 182
— asthma, ii. 395
— belly-ache, ii. 558
Dumb, or dead, ague, i. 763
Duodenum, ulceration of, ii. 431
— a cause of hæmorrhage, ii. 495
Dura mater, inflammation of, i. 332
— — treatment of inflammation of, i. 341, 343
— — disease communicated by carious bone, i. 342
— — idiopathic inflammation of, ii. 344
Dying, modes of, i. 62–76. See *Death*
Dysenteria alba, ii. 606
Dysenteric diarrhœa, ii. 602
Dysentery, ii. 568, 602
— its causes, ii. 603
— decline of, in London, ii. 605
— question of the contagion of, ii. 605
— alba, ii. 606
— morbid anatomy of, ii. 607
— treatment, ii. 607
— sporadic, ii. 609
Dysmenorrhœa, a cause of hysteria, i. 719
Dyspepsia, ii. 471, 508
— pathology of, ii. 508
— symptoms of, ii. 512
— treatment of, ii. 524
— prevention of, ii. 526
Dysphagia in hysteria, i. 723
— in tonsillitis, i. 855
Dyspnœa, a symptom, i. 140; ii. 760, 762
— in tonsillitis, i. 856
— general account of, ii. 3
— produced by ipecacuan dust, ii. 57, 402
— in phthisis, ii. 218
Dysuria, ii. 606

EAST wind, its effect on intermittents, i. 766
Eccentric tetanus, i. 570
— — its treatment, i. 574
— — hypertrophy of the heart, ii. 270
Echino-cocci, ii. 630
Ecchymosis, i. 290, 295
Ecstasy, i. 737
Ectozoa, ii. 617
Ectropium, ii. 932
Eczema rubrum, ii. 753, 1017
— its various kinds, ii. 1018
— solare, ii. 1018
— infantile, ii. 1019
— treatment of, ii. 1019
— impetiginodes, ii. 1026
Efferent nerves in palsy, i. 537
Effusion of serum an event of inflammation, i. 174, 201
— of coagulable lymph an event of inflammation, i. 175
— purulent, i. 182
— in pleurisy, ii. 114
Electrolysis for hydatids, ii. 634
Emaciation, i. 28
— a symptom, i. 145
— in phthisis, ii. 221
Emetics in ague, i. 801
— — fever, ii. 914
Emphysema of the lungs, ii. 71, 173
— pulmonary, ii. 173, 765
— — vesicular, ii. 173
— — interlobular, ii. 173, 188
— — atrophic, ii. 180
— — sub-pleural, ii. 188
— causes, ii. 177, 185
— relation to tubercles, ii. 186
Emprosthotonos, i. 559

Empyema, i. 181; ii. 146
Encephalitis, acute, its symptoms, i. 349
— modes of access, i. 352
— morbid anatomy, i. 353
— treatment, i. 353
— chronic, or partial, i. 359
Encysted dropsy of the abdomen, ii. 445
Endosmose, its relation to dropsy, i. 294
Endocarditis, ii. 320, 353
— symptoms, ii. 325
— treatment, ii. 353
Endocardium, morbid states of, ii. 303
— acute inflammation of, ii. 320
Engorgement of the lungs, ii. 78
Engrafting small-pox, ii. 937
Enlargement of the spleen, i. 798; ii. 451, 498
— of the liver, ii. 798, 799
— of the kidney, ii. 798
— of the mesenteric glands, ii. 798
Enteritis, ii. 530
— symptoms, ii. 530
— erythematica, ii. 530
— phlegmonodea, ii. 530
— ileus, ii. 531
— diagnosis, ii. 531
— causes, ii. 533
— treatment, ii. 535
— idiopathic, ii. 534
— consecutive, ii. 534
Entozoa, ii. 617
— origin of, ii. 641
Entozoon, eggs of, mistaken for tubercles, i. 226
Epidemic catarrh, ii. 43
— — migration of, ii. 48
— cholera, ii. 573
— exanthemata, ii. 845
— small-pox, ii. 950
— erysipelas, ii. 997
Epigastric region, ii. 416
Epilepsy, i. 628
— suddenness of, i. 630
— its symptoms, i. 632
— varieties, i. 632–634
— duration of the attacks, i. 634
— recurrence, i. 634
— periods of life at which they commence, i. 634
— warnings of, i. 635
— its effects, i. 638
— pathology of, i. 640
— morbid anatomy, i. 641
— predisposing causes, i. 646
— exciting causes, i. 651
— diagnosis, i. 653
— feigned, i. 653
— prognosis, i. 657
— treatment during the fit, i. 658, 795
— — during the warnings, i. 665
— of childhood, i. 657
— and hysteria confounded, i. 656, 717
— surgical remedies, i. 664
— precautions, i. 666
— malaria in, treatment of, i. 795
Epileptic fit, i. 632, 649, 717
— vertigo, i. 632, 639
— aura, i. 636, 650
Epistaxis, i. 283, 289, 819; ii. 496
— active idiopathic, i. 820
— a symptom of purpura, i. 821
— its connexion with plethora capitis, i. 820
Epithelial cancer, i. 231
— of lip, i. 239
Epizoa, ii. 617
Epizootic diseases, ii. 47
Eructation, hysterical, i. 724
Erysipelas, i. 199, 201
— phlegmonoïdes, i. 199
— ii. 992
— contagious, ii. 993
— symptoms, ii. 994
— modes of death in, ii. 995
— causes, ii. 995
— treatment, ii. 998
— idiopathic, ii. 996
— epidemic, ii. 997
Erysipelatous inflammation, i. 201; ii. 992
Erythema, i. 201; ii. 992, 1001
— nodosum, ii. 1001
— mercuriale, ii. 1018
Events of inflammation, i. 164, 173
Examination of abdomen, ii. 415, 440
Exanthemata, ii. 840, 842
— are contagious, ii. 842, 843
— sporadic, ii. 849
— sometimes epidemic, ii. 849
— period of the eruption, ii. 851
— — incubation, ii. 853
— — of dormancy, ii. 853
Exciting causes of disease, i. 82
Excito-motor phenomena, i. 314
Exercise, i. 84
Exfoliation, i. 187
Exhalation, hæmorrhage by, i. 275
Exophthalmic goître, i. 840
Exosmose, its relation to dropsy, i. 294
Expectoration, sanguineous, i. 724
— crude, ii. 33
— concocted, or ripe, ii. 33
— of pneumonia, its kinds, ii. 94
— of phthisis, ii. 215
External inflammation, i. 149
— cold in inflammation, i. 259
— warmth in inflammation, i. 259
— — in intermittent fever, i. 798
— membrane of heart, disease of, ii. 331
Extirpation of ovarian sac, ii. 455
Extravasation of fluids, i. 31

FABA Si. Ignatii, i. 564
Face-ache, i. 752
Facial anæsthesia, i. 541
— paralysis, i. 541
— — symptoms, i. 541
— — causes, i. 547
— — treatment, i. 549
— neuralgia, i. 745

Faith, its influence in curing ague, i. 814
Falling sickness, i. 630. See *Epilepsy*
False membranes, how formed, ii. 109
Fallopian tubes, dropsy of, ii. 445
Family likenesses, i. 117
Fasciola hepatica, ii. 640
Fasting in gastric hæmorrhage, ii. 507
Fat, its use in colica pictonum, ii. 566
Fatty growth in the abdomen and about the heart, ii. 188
— degeneration, i. 37
— liver, i. 38; ii. 206
Favus confertus, ii. 1013
Febrile dropsy, i. 307; ii. 979
— diseases, contagious, ii. 854
— urticaria, ii. 1005
— oppression, ii. 866
Febris scarlatina, ii. 985
Feigned epilepsy, i. 653
Fermented liquors in ague, i. 816
Fern-root a remedy for worms, ii. 655
Fever, inflammatory, i. 150, 190
— — fibrin of the blood in, i. 168
— hectic, i. 151, 184
— remittent, i. 194
— — confounded with acute hydrocephalus, i. 401
— — in ague, i. 760
— intermittent, i. 756. See *Intermittent Fever*
— gall, i. 793
— hay, ii. 54
— of pleurisy, ii. 128
— of phthisis, ii. 219
— rheumatic, its relations with carditis, ii. 346
— of various kinds, ii. 864
— puerperal, ii. 423
— contagious, theory of, ii. 855
— continued, ii. 865, 895
— — premonitory circumstances, ii. 866
— — stages, ii. 867
— — chloral or opiates in, ii. 921
— typhus, i. 152, 198; ii. 865, 890
— typhoid, ii. 865, 882, 900
— intestinal, ii. 865
— relapsing, ii. 865, 890, 913
— rubeoloid, ii. 873
— miliary, ii. 873
— variolous, ii. 926
— spotted, ii. 873
— terminations and modes of dying in, ii. 875
— morbid appearances, ii. 890
— varieties of, ii. 893
— its exciting cause contagion, ii. 895
— proofs of this, ii. 896
— arguments against it, ii. 902
— other alleged causes, ii. 905
— predisposing causes, ii. 906
— treatment, ii. 911
— secondary, of small-pox, ii. 931
— scarlet, ii. 971
Fibrin of the blood in inflammatory fever, i. 168
Fibrinous lymph, i. 181
Fibroid phthisis, ii. 233
— morbid anatomy, ii. 235
Fibrous rheumatism, ii. 805
Fièvre ataxique, ii. 918
Filaria Medinensis, ii. 637
First intention, union by, i. 177
Fits, i. 630. See *Epilepsy*
Flatulence in dyspepsia, ii. 514
Floccitatio, ii. 417
Fluctuation, ii. 417, 806
Fluctuations of the pulse, i. 145
Fluids, disorders of, i. 43
Flukes, ii. 617
Flux, bloody, ii. 603, 606
Fluxes, morbid, ii. 568
Fœtus in utero, diseases of, i. 765
Foie gras, how procured, i. 38
Fomentations in inflammation, i. 260
Food, disease caused by bad and hurtful, i. 83
— what to eat and drink, ii. 526
— causing diarrhœa, ii. 569
— in diarrhœa, raw vegetable, ii. 569
— — putrid, ii. 569
— — high, ii. 569
— — fish, ii. 570
Formation of pus an event of inflammation, i. 179
Formication, i. 532
Fowler's solution in intermittents, i. 810
Foxglove in tetanus, i. 581
Fremissement cataire, ii. 282
Friction of pleurisy, ii. 111
Fright as an exciting cause of epilepsy, i. 651
— as cause of chorea, i. 679, 680
— as cure of chorea, i. 680
Frost-bite, i. 95
Fucus vesiculosus in bronchocele, i. 835
Fumigation of the throat in venereal ulceration, i. 879
Functions, disordered, i. 135. See *Disordered Functions*
Fungous ulcer, i. 185
Fungus hæmatodes, i. 236
— skin, ii. 1026
Furunculoid, contagious, ii. 1029
Furunculus, ii. 1027

GALL-BLADDER, rupture of, ii. 432
— — diseases of, ii. 689
— fever, i. 793
— stones, i. 31; ii. 678, 682
— — in bowel, ii. 549
Gallic acid in internal hæmorrhage, ii. 790
Galloping consumption, ii. 226
Ganglionic centres, or foci, i. 314
Gangrena senilis, i. 188
Gangrene, an event of inflammation, i. 186
— from eating diseased grain, i. 189
— difference between dry and moist, i. 189
— result of inflammation of the lung, ii. 82

Gangrene in diabetes, ii. 726
Gargouillement, ii. 207
Gastric hæmorrhage, ii. 496
— — idiopathic, ii. 496
— — vicarious, ii. 496
— — from disease or injury of the stomach, ii. 497
— — capillary, ii. 497
— — from passive congestion, ii. 497
— — from changes in the blood, ii. 500
— — diagnosis of, ii. 502
— — treatment of, ii. 506
Gastric juice, a source of softening, i. 35
— — its effects in disease, ii. 481
— — power of, ii. 502, 510
— — in health, ii. 509
Gastritis, acute, ii. 464
— its symptoms, ii. 466
— — anatomical characters, ii. 467
— — erythematica, ii. 467
— — causes, ii. 469
— — treatment, ii. 470
— chronic, ii. 471
— — treatment, ii. 477
— idiopathic, ii. 466
Gastrodynia, i. 743; ii. 515, 518, 679
Gastro-enteric fever, ii. 864
Gauze veil in ague, i. 817
Gemmation, ii. 629
General convulsions, i. 346
— dropsy, i. 294; ii. 696
Generation, equivocal, ii. 644
— of vermin by filth, ii. 908
Generations, alternating, ii. 624
Germ theory, ii. 50
Ghost-seers, i. 136
Giddiness, a symptom, i. 133
Gin-drinker's liver, ii. 449
Glands and solid viscera, inflammation of, i. 199
— mesenteric, enlargement of, ii. 798
— — inflammation of, ii. 885
— solitary, ii. 883
— of Peyer, ii. 884
Glandulæ agminatæ, ii. 884
Globus hystericus, i. 637
Glottis, œdema of, i. 880
Glucose, ii. 722, 728
Gluten-bread, in diabetes, ii. 735
Glycerinum boracis in aphthæ, i. 851
Goître, i. 821
— remedies for, i. 835, 837
— exophthalmic, i. 840
— treatment, i. 842
Goitrous fœtus, 824
Goose's skin, i. 94; ii. 1006
— — in ague, i. 756
Gout, ii. 815
— its phenomena, ii. 816
— facts relating to, ii. 818
— irregular, ii. 821
— atonic, ii. 821
— lurking or masked, ii. 821
— misplaced, ii. 822
— retrocedent, ii. 821
Gout, predisposing causes of, ii. 556, 822
— and rheumatism, diagnosis between, ii. 824
— its pathology, ii. 826
— prognosis, ii. 830
— prejudices respecting it, ii. 831
— treatment during the paroxysms, ii. 832
— — intervals, ii. 836
— in the stomach, ii. 838
Gouty inflammation, i. 210
— kidney, ii. 449
— diathesis, ii. 818
Grain, diseased, gangrene caused by, i. 189
Grand mal, i. 632, 639, 644
Granular peritoneum, ii. 437
— degeneration of the kidney, ii. 744
Granulations of Bayle, ii. 197, 227
Gravedo, ii. 23
Gravel, fit of the, ii. 690, 697
— different kinds of, ii. 699
— white, ii. 705, 708
Gravity, specific, of diabetic urine, ii. 732
Gray hepatisation of the lungs, i. 181; ii. 80
— softening of the lung, ii. 80
Grease, its relation to cow-pox, ii. 956
Great fire of London, blessings of the, ii. 605
Grinders' asthma, or rot, ii. 238
Griping in the guts, ii. 603
Gripings, ii. 530
Grippe, ii. 43
Grocers' itch, ii. 1018
Guillotine for enlarged tonsils, i. 861
Guinea-worm, ii. 617, 637
— symptoms, ii. 639
— treatment, ii. 639
Gurgling in phthisis, ii. 209
Guts, griping in the, ii. 603,

HABIT in intermittents, i. 784
Hæmatemesis, i. 283; ii. 167, 495
— diagnosis between it and hæmoptysis, ii. 167
— hysterical, i. 724
— idiopathic, ii. 496
— vicarious, ii. 496
— from gastric disease or injury, ii. 497
— from other disease, ii. 498
— from morbid state of blood, ii. 498
— its general phenomena, ii. 500
— diagnosis of, ii. 501
— its treatment, ii. 506
— distinguished from hæmoptysis, ii. 505
Hæmaturia, i. 283; ii. 780
— phenomena, ii. 780
— remedies, ii. 789
— intermittent, ii. 789
Hæmoptysis, i. 283; ii. 155, 157, 160, 165, 217, 504
— diagnosis between it and hæmatemesis, ii. 167
— in phthisis, ii. 217
— distinguished from hæmatemesis, ii. 504

Hæmoptysis, hysterical, i. 724
Hæmorrhage, an event of inflammation, i. 175
— capillary, i. 276; ii. 158
— habitual, i. 276
— periodical, i. 277
— effects of, on brain, i. 377
— cerebral, i. 276, 456, 473
— spinal, i. 530
— nasal, i. 819
— pulmonary, ii. 154. See *Pulmonary Hæmorrhage*
— of mucous membranes, ii. 151
— primary or idiopathic, ii. 157
— spontaneous pulmonary, ii. 158
— its varieties, ii. 162
— from the stomach, ii. 167, 495
— idiopathic, ii. 495
— — gastric, ii. 496
— by deviation, ii. 496
— intestinal, ii. 499
— from the bowels, ii. 885
Hæmorrhages, relations of congestions to, i. 61
— habitual, i. 277
— general pathology of, i. 274
— medical, i. 274
— cutaneous, i. 275
— by exhalation, i. 276
— capillary, i. 276, 819
— habitual, i. 277
— vicarious, i. 277, 819; ii. 496
— idiopathic, i. 279; ii. 496
— active, i. 278; ii. 496
— passive, i. 280
— symptomatic, i. 281
— symptoms and diagnosis, i. 285
— treatment, i. 287
Hæmorrhagic pulse, i. 280; ii. 170
Hæmorrhoïdes vesicæ, ii. 783
Hæmorrhoïs, i. 283, 287; ii. 568
Hæmatoidine, i. 475
Hardening children, i. 108
Hardness of the pulse, i. 190, 256
Hay asthma or fever, ii. 54
— causes, ii. 56
— treatment, ii. 57
Head, death beginning at the, i. 76
— cold in, ii. 23
— symptoms, ii. 752
Headache, ii. 912
— confined to one side, i. 754. See *Hemicrania*
— a premonitory symptom of apoplexy, i. 512
— sick, ii. 522
— bilious, ii. 522
— in typhus fever, ii. 866, 869
Hearing, disordered functions of, i. 126
Heart, death beginning at the, i. 64; ii. 466
— its position in pleurisy, ii. 113, 124
— its state in pulmonary hæmorrhage, ii. 160
— fatty growth, ii. 188
— diseases of, ii. 268
Heart, hypertrophy of, ii. 269, 291
— — simple, ii. 270, 291
— — eccentric, ii. 270
— — concentric, ii. 270
— — with dilatation, ii. 270, 295
— natural proportions of the heart and its several parts, ii. 273
— active aneurism of, ii. 270
— morbid sounds, ii. 275
— dilatation of, ii. 275
— sounds of, natural, ii. 276
— — morbid, ii. 280, 330
— diseases of, ii. 282
— palpitation of, ii. 283
— irregular action of, ii. 284
— diseases affecting its muscular texture, ii. 291
— atrophy of, ii. 297
— fatty degeneration of, i. 38; ii. 297
— rupture of, ii. 299, 368
— valvular disease of, ii. 304
— suppuration of the heart, ii. 352
Heart-burn from dyspepsia, ii.
Heat, a cause of disease, i. 86
— degree of, compatible with life, i. 88
— apoplexy, i. 526. See *Sun-stroke*
— a sign of inflammation, i. 159
— spot, ii. 1018
Hectic fever, i. 151, 194
— — idiopathic, i. 197
— — in ague, i. 760
— — in phthisis, ii. 219
Hemicrania, i. 754
— causes of, i. 754, 755
Hemiplegia, i. 468, 471, 485
— muscles affected, i. 481
— other symptoms combined with, i. 488, 499
— simulated by hysteria, i. 722
Hemispasm, i. 484
Hepatic apoplexy, i. 530
— disease, dropsy referred to, ii. 445
— ascites, ii. 453
— congestion, ii. 605, 664
— abscess, ii. 661
— suppuration, ii. 663
— acute yellow atrophy of liver, ii. 665
— sugar, ii. 729
Hepatisation of the lung, i. 31; ii. 31, 80
— gray, i. 181; ii. 80
Hepatitis, acute, ii. 658
— — causes, ii. 663
— — treatment, ii. 664
— chronic, ii. 667
Hereditary tendency and susceptibility to disease, i. 84, 115, 224
— similarities, i. 117
— — acquired, i. 119
Hernia cerebri, i. 205
— humoralis, i. 158
— strangulated, ii. 532
— — internal, ii. 554
Herpes, ii. 1013
— various kinds, ii. 1013
— præputialis, ii. 1013

Herpes, circinatus, ii. 1013
— iris, ii. 1013
— zoster, ii. 1013
Heterogeneous attraction, i. 294
Hiccup in hysteria, i. 723
Hobnail-liver, ii. 447, 800
Hoffman's anodyne in asthma, ii. 405
Holly-leaves as a remedy for ague, i. 812
Hooping-cough, ii. 68
— symptoms, ii. 68
— duration, ii. 69
— complications, ii. 71
— pathology, ii. 72
— treatment, ii. 74
Horripilatio, i. 758
Horses, broken-winded, ii. 183
Hot-air bath in ague, i. 800
— — anasarca, ii. 771
— weather, a cause of diarrhœa, ii. 571
Hunger a symptom, i. 134
Humoral pathology, i. 43, 45
— asthma, ii. 395
Hydatid tumours, ii. 800
— — secondary, ii. 659
Hydatids in the brain, ii. 617
— in the liver, ii. 445, 617, 630
— their nature, ii. 629
Hydrargyrium, ii. 1018
Hydrocele, i. 310
Hydrocephalic children generally strumous, i. 219
Hydrocephaloid disease, i. 415
Hydrocephalus, i. 291
— chronic, i. 386
— acute, i. 387
— symptoms, i. 388
— character, i. 387
— tubercles in, i. 392
— precursory symptoms, i. 394
— exciting causes, i. 395
— modes of attack, i. 396
— its stages, i. 397
— confounded with remittent fever of childhood, i. 401
— predisposing causes, i. 404
— prognosis, i. 405
— mortality by, i. 407
— treatment, i. 408
— prevention of, i. 412
— spurious, i. 414
— — treatment of, i. 415
— chronic, i. 417
— shape of the head and face, i. 418
— anatomical conditions, i. 419
— symptoms, i. 420
— treatment, i. 425
— internal remedies, i. 425
— mechanical expedients, i. 427
— arachnoidean, i. 435
— ventricular, i. 435
— epilepsy attendant on, i. 646
— in hooping-cough, ii. 72
Hydropericardium, i. 292
Hydrophobia, i. 589
— symptoms, i. 589

Hydrophobia, period of incubation, i. 589
— period of recrudescence, i. 590
— cases, i. 590
— morbid anatomy, i. 601
— questions connected with, i. 602, 606
— — in rabies in the dog, i. 606
— pathology, i. 619
— treatment, i. 620
— prevention, i. 622
Hydrothorax, i. 292; ii. 112
Hydruria, ii. 740
Hyperæmia, i. 49
— asthenic or passive, ii. 59
Hypertrophy, i. 17
— laws of its production, i. 17
— of an organ without enlargement, i. 21
— its effects, i. 22
— its causes, i. 22
— of brain, i. 383
— — its connexion with rickets, i. 383
— of heart, ii. 269, 291. See *Heart*
— — with dilatation, ii. 270, 295
— of stomach, ii. 490
Hypochondria, stitches and pains in, i. 721
— ii. 416
Hypochondriasis, i. 730; ii. 522
Hypogastric region, ii. 416
Hysteria, i. 715
— confounded with epilepsy, i. 656, 717
— two forms of paroxysm, i. 715
— persons most liable to, i. 719
— diseases apt to be simulated by, i. 721
— treatment during the paroxysm, i. 730
— — during the intervals, i. 733
— prevention, i. 734
Hysterical cough, i. 723
— breast, i. 723; ii. 198
— eructation, i. 724
— hæmatemesis, i. 724
— hæmoptysis, i. 724
— affection of the joints, i. 724
— — of the spine, i. 725

ICE, in hæmatemesis, i. 288
— in encephalitis, i. 355
— in tetanus, i. 578
Icterus, ii. 675
— spasmodicus, ii. 682
— gravidarum, ii. 683
— neonatorum, ii. 683
— calculosus, ii. 678
Ideo-motor centre, i. 316
Idiopathic hectic, i. 197
— hæmorrhage, i. 279; ii. 496
— — active, i. 820
— inflammation of dura mater, i. 341
— tetanus, i. 561, 579
— epilepsy, i. 647
— ptyalism, i. 847
— gastric hæmorrhage, ii. 453
— hæmatemesis, ii. 453
— enteritis, ii. 534
— erysipelas, ii. 996
Ileus, ii. 531

Iliac regions, ii. 416
Ilicine in ague, i. 812
Illusions, spectral, i. 137
Impetigo, ii. 1026
— its various kinds, ii. 1026
Impure air, a cause of disease, i. 109
Incipient cancer of stomach, ii. 505
Incubation of congestion, i. 52
— of rabies, ii. 856
— of exanthemata, ii. 855
Indications of treatment, i. 128
Indigestion, pathology of, ii. 508
Induration, i. 29
— of brain, i. 377
Infantile laryngitis, i. 881
Infarction, sudden, of a large bronchus, ii. 40
Infiltration, purulent, of lungs, ii. 80
— tubercular, of lung, ii. 198
Infectious diseases, ii. 848
Inflammation, a cause of softening, i. 54
— relation of congestion to, i. 61
— general account of, i. 147, 164
— signs of, i. 149
— pain in, i. 149, 156, 161
— — tenderness in, i. 157
— heat in, i. 150, 161
— redness, i. 152
— swelling, i. 154
— products of, i. 155
— conditions of, i. 162
— period of incubation of, i. 163
— state of minute blood-vessels, i. 164, 168
— stagnation, i. 164
— buffy coat of the blood, i. 164, 169
— events of, i. 164, 173
— resolution, i. 173
— effusion of serum, i. 174
— effusion of coagulable lymph, i. 175
— adhesive, i. 177
— suppuration, i. 179
— ulceration, i. 183
— mortification, i. 186
— pyrexia, i. 190
— hectic fever, i. 194
— remittent fever, i. 194
— typhus fever, i. 198
— its modifications as it affects different tissues; areolar tissue, i. 198; ii. 803
— — larger glands and solid viscera, i. 199
— — diffused, of the cellular membrane, i. 199
— — its modifications as it affects serous membranes, i. 198
— — synovial membranes, i. 200
— — tegumentary membranes, i. 201
— — mucous membranes, i. 202
— — muscular tissue, i. 203
— — arteries, i. 204
— — veins, i. 204
— — substance of the brain, i. 205; ii. 876
— varieties of, i. 207
— acute and chronic, i. 207
— sub-acute, i. 208
Inflammation, latent, i. 209
— specific, i. 210
— scrofulous, i. 210
— treatment of, i. 241, 242
— — antiphlogistic regimen, i. 243
— — blood-letting, i. 244
— — purgatives, i. 252
— — mercury, i. 261
— — antimony, i. 270
— — ipecacuan, i. 272
— — opium, i. 272
— external remedies, i. 259
— — cold, i. 258
— — warmth, i. 259
— — counter-irritation, i. 259
— of iris, i. 262
— of dura mater, i. 332
— of arachnoid, i. 335
— of pia mater, i, 344
— of brain, i. 351
— of spinal cord, i. 435
— of peritoneum copied by hysteria, i. 721
— pneumonic, ii. 2, 42
— of mucous membranes, ii. 22
— of lungs, ii. 77
— of pleura, ii. 108
— of pericardium, ii. 303, 322, 353
— of endocardium, ii. 322, 353
— pectoral, ii. 347
— of veins, ii. 376
— of œsophagus, ii. 407
— of peritoneum, i. 721
— — chronic, ii. 437
— of stomach, acute, ii. 464
— — chronic, ii. 471
— of bowels, ii. 530
— of liver, ii. 656
— of kidneys, ii. 690
— of mesenteric glands, ii. 885
— erysipelatous, ii. 992
Inflammatory fever, i. 150, 190
— blood, i. 168
— dropsy, i. 307; ii. 759
— sore-throat, ii. 864. See *Laryngitis*
Inflatio, a symptom of dyspepsia, ii. 514
Influenza, ii. 43
— its symptoms, ii. 44
— progress, ii. 45
— conjectures as to its cause, ii. 46, 49
— treatment, ii. 52
Infra-mammary region, ii. 14
Ingesta, their regulation in diabetes, ii. 734
Inguinal regions, ii. 416
Inoculation of tubercles, ii. 193
— of cadaveric venom, ii. 427
— of small-pox, ii. 936
— and vaccination, their comparative merits, i. 950
Insolation, i. 93
Inspiration, emphysema from, ii. 177
— thoracic, in peritonitis, ii. 421
Intellect, disordered functions of, i. 138
Intention, first, union by, i. 177
Interlobular emphysema of the lungs, ii. 173, 188

Interlobular emphysema of the lungs, its anatomical characters, ii. 188
— — symptoms, ii. 189
— — cause, ii. 190
— — cure, ii. 190
Intermittent fever, i. 756
— phenomena, i. 756
— species, i. 759
— predisposing causes, i. 766
— exciting cause, i. 767. See *Malaria*
— speculations respecting its periodicity, i. 780
— prognosis, i. 796
— treatment during the paroxysm, i. 799
— — intermissions, i. 806
— prophylaxis, i. 809
— hæmaturia, ii. 788
Intermittents, vernal, i. 764
— autumnal, i. 764
Internal senses, affections of, i. 138
— membrane of the heart, disease of, ii. 331
— strangulated hernia, ii. 552
Intestinal hæmorrhage, ii. 499
— concretions, ii. 612
— tube, mechanical occlusion of, ii. 538
— — treatment of, ii. 539
— worms, ii. 617
— fever, ii. 865
Intestines, ulceration of, ii. 205, 885
— stricture of the larger, ii. 540
— intus-susceptions of the smaller, ii. 552
— — and of the larger, ii. 553
Intra-scapular region, ii. 15.
Intrusion of solid substances into the air-passages, ii. 258
Intus-susception, i. 42; ii. 553
Iodide of potassium as a remedy in asthma, ii. 406
— — in face-ache, i. 753
— — in chronic rheumatism, ii. 815
Iodine in bronchocele, i. 834
Ipecacuan dust productive of dyspnœa, ii. 57, 402
— in dysentery, ii. 608
Iron, carbonate of, in tetanus, i. 583
— in ague, i. 814
Irregular action of heart, ii. 283
— gout, ii. 821
Irregularity of pulse, i. 140; ii. 284
Irritation, counter, in inflammation, i. 174, 260
Ischuria, ii. 718
— renalis, ii. 718, 757
Itch, ii. 1009
— pocky, ii. 1009
— grocers', ii. 1018
Itching, i. 132
— as a symptom, i. 133

JACTITATION, in heart disease, ii. 329
Jaundice, ii. 658, 672
— constituent features of, ii. 671, 672
Jaundice, appearances, ii. 676
— causes, ii. 682
— prognosis, ii. 683
— treatment, ii. 685
Jaw, locked, i. 557. See *Trismus*
— fall, i. 588
Jerking pulse, ii. 311
Joints, hysterical affections of, i. 724

KIDNEY, dropsy of, ii. 445
— gouty, ii. 449
— diseases of, ii. 690
— inflammation of, ii. 690
— suppuration of, ii. 693
— treatment of, ii. 695
— office of, ii. 700
— BRIGHT'S, ii. 744
— large white, ii. 746
— small red, ii. 746
— mottled, ii. 775
— concretions in, ii. 787
— enlargement of, ii. 798
Kousso, a vermicide, ii. 654
Krameria, its use in internal hæmorrhage, ii. 791

LACTIC acid, its relation to rheumatism, ii. 810
Lardaceous liver, ii. 206, 658
Large crepitation, ii. 27
Laryngeal œdema, i. 880
— chronic disease, i. 907
— inflammation, i. 907
— phthisis, i. 907; ii. 204
— warts, i. 908
Laryngismus stridulus, i. 883, 903
Laryngitis, mock, i. 722
— acute, i. 864
— — symptoms, i. 864, 869
— — pathology, i. 865, 869
— — diagnosis, i. 867
— — treatment, i. 867
— — mistaken for some other malady, i. 871
— — causes of, i. 879
— — distinction between, and œdema of the glottis, i. 880
— infantile, i. 881
— diphtheritic, i. 887
— simulated by hysteria, i. 722
— idiopathic, i. 881
Laryngoscope, the, i. 862, 909; ii. 266
Laryngotomy, i. 877; ii. 262
Larynx, acute inflammation of, i. 864. See *Laryngitis*
— ulceration of, ii. 204
Latent pneumonia, ii. 106
Lateral tetanus, i. 559
— region, ii. 15
Lateritious urine, ii. 704
Laudanum in diarrhœa, ii. 572
Lavement, ii. 542
Lead colic, ii. 556

Lead, sugar of, in pulmonary hæmorrhage, ii. 171
Leaping ague, i. 694
Leeches in pleurisy, ii. 138
Leeching, i. 255
Lemonade, sulphuric acid, in painters' colic, ii. 567
Lemon-juice a specific against scurvy, ii. 1032
Lepra vulgaris, ii. 1023
— alphoïdes, ii. 1024
— syphilitic, ii. 1024
Leucocythemia, ii. 689
Leucoderma, ii. 795
Leucophlegmatic temperament in scrofula, i. 224
— — hysteria, i. 720
Limestone, its connexion with goître, i. 828
Linimentum iodi in cynanche tonsillaris, i. 861
Liquor sanguinis, i. 163, 174
— puris, i. 165
— arsenicalis in hemicrania, i. 755
— — in ague, i. 810
Liquors, fermented, in ague, i. 816
Lithates in the urine, ii. 707
Lithia, a remedy in gout, ii. 835
Lithic diathesis, ii. 701, 819
— remedies, ii. 704
— acid, in relation to gout, i. 819
Liver, effects of heat on, i. 91
— fatty, i. 38; ii. 206, 800
— lardaceous, ii. 206
— waxy, ii. 207, 669, 800
— abscess of, ii. 432
— cysts in the, ii. 445
— cirrhose, ii. 446, 667
— hobnail, i. 800; ii. 447, 667
— diseases of, ii. 656
— acute inflammation of, ii. 656, 664
— — symptoms, ii. 656
— — treatment, ii. 664
— abscess of, ii. 656
— chronic inflammation of, ii. 667
— — symptoms, ii. 667
— — causes, ii. 669
— — treatment, ii. 670
— acute yellow atrophy of, ii. 665
— apoplexy of, i. 530; ii. 658
— gin-drinker's, ii. 669
— nutmeggy, ii. 670
— enlargement of, ii. 799
— cancerous, ii. 800
— fluke, ii. 640
Lobular pneumonia, ii. 38, 137
Local anæsthesia, i. 553
— palsy, i. 553
Locality in croup, i. 857
Locked-jaw, i. 557
Locomotor ataxy, i. 535, 695
— symptoms of, i. 696
— progress of, i. 698
— morbid anatomy, i. 699
— treatment, i. 700
Lotions in inflammations, i. 270
Low fever, ii. 864
Lower lateral region, ii. 15
— sternal region, ii. 15
Lumbricus, ii. 617
Lunar caustic in diphtheritis, i. 865
Lung, collapse of, in bronchitis, ii. 34
— hepatised, i. 31, 37, 85
— carnified, ii. 37, 114, 373
— collapse of, ii. 37, 69
— — diffused, ii. 38
— — lobular, ii. 38
— sudden closure of, ii. 42
— gray hepatisation of, ii. 80
— — softening of, ii. 80
— purulent infiltration of, ii. 80
— diffused suppuration of, ii. 80
— gangrene, ii. 82
— inflammation of its substance, ii. 77
— — membrane, ii. 108
— hæmorrhage, ii. 154
— tubercular infiltration of, ii. 194
— cicatrices, ii. 187
— melanosis of the, ii. 251
— cancer of the, ii. 257
Lungs, death beginning at the, i. 69
— emphysema of, ii. 71, 176, 763
— engorgement of, ii. 78
— inflammation of, ii. 78
— splenisation of, ii. 78
— œdema of, ii. 90
Lurking gout, ii. 821
Lymph, coagulable, an event of inflammation, i. 175
— organised, i. 178
— plastic, i. 178
— fibrinous and corpuscular, i. 180
Lymphatic vessels, the seat of tubercles, i. 213

MACULÆ, ii. 839
Mal, grand, i. 632, 639, 644
— petit, i. 632, 639, 644
Malaria causes tic-douloureux, i. 747
— — ague, i. 755
— — hemicrania, i. 755
— — intermittent fever, i. 767
— conditions necessary to its production, i. 767
— places which it chiefly infests, i. 768
— effects of, on the human body, i. 784
— influence of soils in evolving it, i. 784
— most dangerous at night, i. 785
— lies near the ground, i. 786
— is carried about by winds, i. 787
— cannot pass over water, i. 789
— attaches itself to trees, i. 791
— diminishes with increase of cultivation and of population, i. 792
— ultimate effects of, ii. 854
Malformation, diseases caused by, i. 84
Malignant cholera, ii. 573
— disease, ii. 452
Malingerers, i. 655; ii. 504
Mammæ, mumps in the, i. 845

Mammary region, ii. 14
Mange, ii. 1010
Mania à potu, ii. 365
Marriage should be regulated in the diseased, i. 120
— in hysteria, i. 734
Marsh miasmata, i. 767. See *Malaria*
Marshes cause of ague and hemicrania, i. 755
Masked gout, ii. 821
Mastication, its object, ii. 510
Masturbation a cause of epilepsy, i. 651
— — and of paraplegia, i. 536
Measles, ii. 965
— putrid, ii. 966
— diagnosis, ii. 967
— prognosis, ii. 967
— treatment, ii. 968
Mediastinum, its position in pleurisy, ii. 140
Mediate percussion, ii. 9
Medicated vapours in phthisis, ii. 247
Medicine, definition of its principles, i. 12
— description of its practice, i. 13
Medulla oblongata, i. 314
Megrim, i. 754
Mel boracis in thrush, i. 851
Melæna, i. 283 ; ii. 500
— treatment of, ii. 506
Melanic acid, ii. 782
Melanosis, true, ii. 251, 501
— spurious, ii. 254
Melanotic cancer, i. 237
Membrane, cellular, diffused inflammation of, i. 199
— of croup, i. 884
— false, in pleurisy, ii. 109
— external, of heart, disease of, ii. 331
— internal, of heart, disease of, ii. 334
— arachnoid, inflammation of, i. 335
— mucous, thickening of, ii. 471
Membranes, serous, inflammation of, i. 198 199
— synovial, inflammation of, i. 200
— tegumentary, inflammation of, i. 201
— mucous, inflammation of, i. 202, 283 ; ii. 22
— — hæmorrhage of, i. 160
— spinal, inflammation of, i. 439
— false, how formed, i. 109
Memory, infirmities of, i. 138
Meningitis, i. 345, 445
Meningeal hæmorrhage, i. 474
Menorrhagia, i. 283, 719
Mental origin of diseases, i. 84
Mercurial tremor, i. 703
— ptyalism, i. 706
— treatment, i. 706
— prevention, i. 708
— treatment of influenza, ii. 52
— eczema, ii. 1018
Mercury causes inflammation, i. 261
— in iritis, i. 262, 266
— in brain diseases, i. 356
— in tetanus, i. 580
— in hydrocephalus, i. 411
Mercury induces parotitis, i. 847
— in chronic bronchitis, ii. 62
— in pneumonia, ii. 105
— in pericarditis, ii. 357, 434
— in endocarditis, ii. 357
— in dysentery, ii. 608
— in hepatic disease, ii. 670
— in jaundice, ii. 687
— in fever, ii. 917, 989
Mesenteric glands, enlargement of, ii. 798
— — inflammation of, ii. 885
— tabes mesenterica, i. 217
Mesmerism, i. 738
Metallic tinkling, ii. 135
— sounds, ii. 211
Metastasis, i. 174, 846
— to the brain, ii. 302
— in acute rheumatism, ii. 803
Meteorismus, ii. 920
Miasmata cause of ague and hemicrania, i. 755
— marsh, i. 767. See *Malaria*
Microscope, blood-vessels as seen through, i. 54
Microsporum furfurans, ii. 1025
Middle sternal region, ii. 15
Migraine, i. 754
Migration of epidemic catarrh, ii. 48
Miliary tubercles, ii. 194, 197
— fever, ii. 892
— vesicles, ii. 873, 892
Misplaced gout, ii. 821
Mitral valve of the heart, thickening of, ii. 305
Mixed phthisis, ii. 228
Modes of dying, i. 62. See *Death*
Modified small-pox, ii. 943
Moist sounds in respiration, ii. 27
Molimen hæmorrhagicum, i. 279
Mollities ossium, i. 34, 39 ; ii. 779
Morbid sensations, i. 134
— alterations of the solids, i. 17
— — fluids, i. 43
— fluxes, ii. 568
— conditions of urine, ii. 700, 702
Morbilli sine morbillis, ii. 856
— confluentes, ii. 973
Morbus comitialis, i. 630
— lateris, ii. 116
— cæruleus, ii. 293
— mucosus, ii. 606, 621
— arquatus, ii. 675
— regius, ii. 675
Mortification, i. 151
— an event of inflammation, i. 186, 197
— its signs, i. 186
— internal, i. 186
— external, i. 187
— more common in some parts than others, i. 187
— its causes, i. 188
Motion, voluntary, disordered functions of, a symptom, i. 139
Mottled kidney, ii. 775
Mucous membranes, softening of, i. 24
— — inflammation of, i. 202 ; ii. 22

Mucous membranes, hæmorrhage of, i. 160
— — of the stomach, inflammation of, ii. 465, 469
— — thickening of, ii. 471
— rattle, ii. 28
— catarrh, ii. 58
— fever, ii. 864
Mulberry calculus, ii. 711, 712, 714
— rash, ii. 872
Mumps, i. 844
— treatment, i. 846
Muriate of ammonia in face-ache, i. 752
— — in hepatic disease, ii. 671
Murmur, respiratory, ii. 16, 24
— puerile, ii. 17
— venous, ii. 287
— bellows, ii. 280
Muscæ volitantes, i. 135; ii. 871
Muscles, voluntary and involuntary, i. 316
— affected in hemiplegia, i. 480, 486
Muscular sense, seat of the, i. 315
— tissue, inflammation of, i. 203
— texture of heart, diseases of, i. 283
— system, tone of the, i. 706
Musk as a remedy for tetanus, i. 582
Myelitis, i. 443

NASAL hæmorrhage, i. 819
Nascent tubercles, ii. 195, 227
Nausea, a symptom, i. 133
— — of dyspepsia, ii. 512
Necrosis, i. 187
Negro, proof against endemic fevers, i. 777
Nephralgia, ii. 690
— treatment of, ii. 695
— calculosa, ii. 695
Nephritis, ii. 690
— treatment of, ii. 695
— acute, ii. 690, 746
— albuminous, ii. 756
— acute desquamative, ii. 760
Nerve, pneumogastric, irritation of, causes cough, ii. 215
Nerves, disordered functions of, i. 134
— functions and anatomical arrangements in palsy, i. 545
— polarity of, i. 568
Nervous centres, i. 314
— system, diseases of, i. 311
— tissue, inflammation of, i. 205
— irritation, nature of delirium tremens, i. 364
— disease, asthma a, ii. 395
— fever, ii. 864
Nettle-rash, ii. 1003. See *Urticaria*
Neuralgia, i. 740
Neuritis, optic, i. 453
Nitre-fumes a remedy for asthma, ii. 405
Nodes, i. 343
Numbness, a symptom, i. 134
Numerical method, ii. 101
Nummular sputa, ii. 216
Nutmeggy liver, ii. 670
Nux-vomica, poisoning by, i. 564
Nymphomania, ii. 1008

OATMEAL, its tendency to form concretions, ii. 613
Obesity, influences of, i. 24
— Mr. Banting's case, i. 24
Obliteration of veins, ii. 386
— — symptoms, ii. 387
— — treatment, ii. 391
Obsolete vomica, ii. 202
Obstruction of bowels, mechanical, ii. 508
— of venous trunks, ii. 390
Obtuseness of hearing, a symptom, i. 136
Occasional epilepsy, i. 650
Occlusion, mechanical, of the intestinal tube, ii. 538
— treatment of, ii. 539
Occupation, its influence in phthisis, ii. 237
Œdema, i. 292, 295
— of the glottis, i. 886
— of the lungs, ii. 190
— of ankles in phthisis, ii. 221
— of dropsy, ii. 763
Œsophagus, diseases of, ii. 407
— stricture of, actual, ii. 408
— — spasmodic, ii. 408, 411
— — treatment, ii. 411
— dilatation, ii. 413
Oïdium albicans in aphthæ, i. 851
Oil of turpentine in tetanus, i. 583
— — in chorea, i. 684
— cod's-liver, in phthisis, ii. 245
— its use in colica pictonum, ii. 512
Oligæmia, i. 51
Omental dropsy, ii. 445
Omentum, cancerous degeneration of, ii. 798
Open cancer, i. 233
Operation of necessity, ii. 148
— of election, ii. 148
Ophthalmoscope, the, i. 452
Opisthotonos, i. 559
Opium as a remedy for inflammation, i. 272
— — tetanus, i. 575
— — ague, i. 800
— — acute bronchitis, ii. 31
— — phthisis, ii. 249
— — pyæmia, ii. 385
— — in asthma, ii. 406
— — peritonitis, ii. 435
— — jaundice, ii. 686
— — gravel, ii. 712
— — diabetes, ii. 737
— in continued fever, ii. 919
Optic neuritis, i. 453
Orchitis, i. 158
Organised lymph, i. 178
Orinasal respirator, i. 566, 816
Ormskirk medicine, in rabies, i. 626
Orthopnœa, a symptom, i. 140
— more frequent in the night, ii. 182
Ossa triquetra, i. 418
Ossification of valves of heart, ii. 305, 306

Osteoid cancer, i. 237
Otitis, i. 335
Otorrhœa, i. 337
Ovarian dropsy, ii. 440
— diagnosis of, ii. 442
— pathology of, ii. 450
— progress of. ii. 450
— treatment, ii. 453
— — by paracentesis, ii. 454
— sac, extirpation of, ii. 456
— tumours, ii. 798
Ovariotomy, ii. 457
Oxalic diathesis, ii. 712
Oxyuris vermicularis, ii. 617
Ozone, effects of an excessive quantity of, ii. 49
— its purifying properties, ii. 49
— its absence during cholera, ii. 586, 590

PAIN, a symptom, i. 128
— different kinds and degrees of, i. 129
— indirect or sympathetic, i. 130
— relief of, i. 130
— in pleurisy, ii. 118
— in inflammation, i. 156
— in phthisis, ii. 218
— in indigestion, ii. 515
Painters' colic, ii. 556
Palpation, method of, ii. 19
Palpitation of the heart, ii. 283
Palsy, i. 139, 516
— from inflammation of spinal cord, i. 530
— facial, i. 541
— local, i. 538
— the shaking, i. 701
— wasting, i. 709. See *Wasting palsy*
— simulated by hysteria, i. 722
Pancreas, diseases of, ii. 689
Pang. breast, ii. 314
Papulæ, ii. 840
— of small-pox, ii. 928
Papular small-pox, ii. 928
Paracentesis, i. 310 ; ii. 130, 459
— thoracis, ii. 130, 147, 150
— abdominis, ii. 459
Paralysis of one side, i. 437, 478
— local, i. 513
— partial, i. 532
— facial, i. 541
— agitans, i. 701
Paraplegia, i. 437, 445, 468, 532
— urinary, i. 537
— treatment of. i. 539
— simulated by hysteria, i. 722
Parasites, vegetable, ii. 1025
Parotitis, i. 844
— from mercury, i. 847
Passive inflammation of the brain, i. 208
— hæmorrhage, i. 280
— dropsy, i. 296. 305
— congestion, i. 58 ; ii. 497
— — gastric hæmorrhage from, ii. 498
— rheumatism, ii. 814
Pathognomonic symptoms, i. 127
Pathology, i. 16
— general or special, i. 16
— morbid alterations of the solid parts of the body, i. 17
— alterations in bulk, i. 17
— of sudden death, i. 65
Pectoral inflammation, ii. 347
Pectoriloquy, i. 18 ; ii. 211
Pemphigus, ii. 1020
— acute and chronic, ii. 1021
Penicilium glaucum, ii. 720
Pepsine, ii. 509
Pepsine in indigestion, ii. 515
Percussion, ii. 9, 418
— direct, ii. 9
— mediate. ii. 9
— method of employing, ii. 11
— of chest, ii. 114
— of abdomen, ii. 441, 799
Perforation of the stomach, ii. 429, 473
— of bowels, ii. 888
Pericarditis, ii. 322, 353
— its connexion with acute articular rheumatism, ii. 323
— symptoms, ii. 328
— treatment of, ii. 353
Pericardium, inflammation of, ii. 303
— acute inflammation of, ii. 323
— chronic and partial inflammation of, ii. 359
Periosteum, rheumatic affection of, ii. 815
Peripneumonia notha, ii. 41, 59
Peripneumony, bastard, ii. 41
Peritoneum, inflammation of, in hysteria, i. 721
— — ii. 419
— granular, ii. 436
— chronic inflammation of, ii. 436
— puncture of, ii. 774
Peritonitis, acute, ii. 418
— its symptoms, ii. 421
— — causes, ii. 422
— puerperal. ii. 423
— from perforation, ii. 429
— from the escape of urine. ii. 432
— from rupture of the gall-bladder, ii. 432
— from abscess of the liver, ii. 432
— treatment of, ii. 433
— — puerperal, ii. 434
— chronic, ii. 436
— — symptoms of, ii. 437
— — treatment, ii. 438
Perspiration in ague, i. 756
— in phthisis, ii. 219. 248
Pertussis, ii. 68. See *Hooping-cough*
Petechiæ, ii. 872
Petit mal, i. 632, 644
Peyer, glands of, ii. 884
Phagedenic ulcer, i. 185
Phantom tumour, ii. 456
Pharynx, foreign bodies in the, ii. 261
Phlebitis, ii. 376
— inflammation of veins, ii. 385
— obliteration of veins, ii. 386
Phlebotomy, ii. 383

PHL

Phlegmasia dolens, i. 204, 295; ii. 339
Phlegmon, i. 150
Phlegmonous inflammation, i. 150
Phosphatic diathesis, ii. 708, 710
Phosphorus in chorea, i. 685
Photophobia, scrofulosa, i. 320
Phrenitis, i. 349. See *Encephalitis*
Phthisis laryngea, i. 907; ii. 204
— pulmonalis, ii. 191
— — not merely a lung disease, ii. 192
— — its connexion with tubercles, ii. 193
— favourite seats of tubercle in the lungs, i. 215
— vomicæ, ii. 155, 198, 200, 202
— intercurrent pleurisy, ii. 204
— implication of larynx, ii. 204
— abdominal complications, ii. 205
— auscultatory signs, ii. 207
— general symptoms, ii. 215
— — cough, ii. 215
— — expectoration, ii. 215
— — hæmoptysis, ii. 217
— — dyspnœa, ii. 218
— — pain of the chest, ii. 218
— — hectic fever, ii. 219
— — frequency of pulse, ii. 219
— — diarrhœa, ii. 220
— — wasting, ii. 221
— — œdema of ankles, ii. 221
— — aphthæ, ii. 221
— complications and indications, ii. 222
— diagnosis, ii. 224
— varieties of, mixed and unmixed, ii. 229
— granulations of Bayle, ii. 197, 227
— duration, ii. 235
— statistics, ii. 236
— treatment, ii. 230, 239
— fibroid, ii. 233
— colliers', ii. 251
— prevention, ii. 239
— temperature and food, ii. 244
— exercise, ii. 245
Pia mater, inflammation of, i. 344
Pictonum, colica, ii. 171, 556
Pigs, entozoa of, ii. 637
Piles, bleeding, ii. 568
Pimples, ii. 840
Pins and needles, sensation of, i. 514
Piperine, in the cure of intermittent fever, i. 813
Pitting of small-pox, medicines to counteract, ii. 960
Pituitous catarrh, ii. 58
Pityriasis, ii. 713, 1025
— versicolor, ii. 1025
Plague, ii. 992
Plasma of the blood, i. 162
Plastic lymph, i. 180, 181
Plethora, general, i. 49
— local, i. 53
— capitis, i. 512, 820
— — its relation to epistaxis, i. 820
— — in heart disease, ii. 288
Pleura, perforations of the, ii. 136, 150

POR

Pleuræ, adhesions of, ii. 203
Pleurisy, double, i. 175
— tapping the chest in, i. 175
— its anatomical characters, ii. 108
— false membranes, ii. 109
— liquid effusion, ii. 111
— — effects of, on the chest, ii. 112
— auscultatory signs, ii. 111, 124
— symptoms, ii. 116
— causes, ii. 132
— treatment, ii. 138
— dry, ii. 118, 203
— adhesive, ii. 118, 203
— distinction between pleuritic effusion and pneumonic consolidation, ii. 131
— succussion, ii. 135
— quantity of liquid in pleurisy, ii. 148
— intercurrent with phthisis, ii. 204
Pleurodyne, ii. 118
Pleuro-pneumonia, ii. 83, 104, 108
— treatment of, ii. 104
Pleurosthotonos, or tetanus lateralis, i. 559
Pleximeter, ii. 559
Plumbi acetas in pulmonary hæmorrhage, ii. 171
Plumbism, ii. 565
Pneumonia, ii. 2, 42
— acute, ii. 106
— latent, ii. 106
— auscultatory signs, i. 209; ii. 2, 84, 199
— lobular, ii. 38, 107
— its stages, ii. 78
— morbid anatomy, ii. 78
— parts affected, ii. 83
— crackling of, ii. 84
— symptoms, ii. 89
— course of the disease, ii. 93
— prognosis, ii. 100
— duration, ii. 99
— treatment, ii. 101
Pneumo-gastric nerve, irritation of, causes cough, ii. 215
Pneumo-hæmorrhage, ii. 162
Pneumo-pleuritis, ii. 108
Pneumo-thorax, ii. 133, 151, 181
— its signs, ii. 134
— with effusion, ii. 133, 181
— tapping the chest in, ii. 150
Pocky itch, ii. 1009
Podagra, ii. 815
Point de côté, ii. 116
Poisons, effects of different, i. 67
— notions of antagonistic properties in different, i. 584
Points of stagnation, i. 164
Polarity of nerves and spinal cord, i. 568
Polydipsia, ii. 739
Polypi, bronchial, i. 202; ii. 61
Polysarcia, i. 20
Pompholix, ii. 1021
— diutinus, ii. 1020
— treatment, ii. 1022
Popliteal aneurism, ii. 373
Porrigo scutulata, ii. 1013

Posture, a cause of apoplexy, i. 519
Post-vaccinal, small-pox, ii. 943
Potassium, bromide of, in epilepsy, i. 662
— — in chorea, i. 684
Potass, acetate of, in rheumatism, ii. 811
— — in scurvy, ii. 1034
Potassium, iodide of, in iritis, i. 267
Potatoes in scurvy, ii. 1033
Poverty of blood, i. 51
Predisposing causes of disease, i. 81
Pre-requisite regimen, ii. 243
Pressure, causing atrophy, i. 27
— aggravation of pain by, i. 158
— on brain, effects of, i. 464
— examination by, ii. 440
— in ovarian dropsy, ii. 460
Prickly heat, i. 93
Primary epilepsy, i. 648
— hæmorrhage, ii. 154
Prognosis of disease, i. 122
Prognostic signs of disease, i. 123
Prolapsus, i. 43
Propagation of cancer, i. 234
— of cholera, ii. 577
Prophylaxis of hydrocephalus, i. 412
— of intermittent fever, i. 815
Proud flesh, i. 185
Prurigo, or itch, ii. 1009
— podicis, i. 133 ; ii. 1006
— pudendi, i. 133 ; ii. 1006
— its several varieties, ii. 1006
— treatment, ii. 1007
Pruritus, ii. 1010
Prussic acid as a remedy for tetanus, i. 582
Psoriasis, ii. 1024
— diffusa, ii. 1024
— inveterata, ii. 1024
Ptyalism, mercurial, i. 714
— spontaneous, i. 847
— idiopathic, i. 847
— its symptoms, i. 847
— of small-pox, ii. 934
Puerile breathing, ii. 17, 89
Puerperal fever, ii. 423
— — its contagiousness, ii. 424
— peritonitis, ii. 423
Pulmonary disease, symptoms of, i. 142
— asphyxia, i. 76
— apoplexy, i. 530 ; ii. 160, 162, 306
— — formation of, ii. 163
— — kinds of, ii. 163
— — symptoms, ii. 161
— — remedies, ii. 171
— texture, diffused suppuration of, ii. 80
— hæmorrhage, ii. 154
— — spontaneous, ii. 157
— — its varieties, ii. 157
— — connexion with consumption, ii. 158
— — — disease of the heart, ii. 160
— emphysema, ii. 173, 174
— — vesicular, ii. 173
— — interlobular, ii. 174
— — auscultatory, physical signs, ii. 181
— — general signs, ii. 182
— consumption, ii. 191
Pulse, indications of, in disease, i. 142
— hardness, or incompressibility of, i. 144, 190, 195, 256 ; ii. 128, 310
— irregularity of, i. 140 ; ii. 284
— — its cause, i. 143
— — in pleurisy, ii. 128
— hæmorrhagic, i. 280 ; ii. 170
— in phthisis, ii. 219
— jerking, ii. 311
Pump-water, its relation to goître, i. 827
Puncturing in hydrocephalus, i. 427, 429
— in anasarca, ii. 771
Pulvis antilyssus in rabies, i. 626
Purgatives as a remedy for inflammation, i. 257
— — brain diseases, i. 356
— — in apoplexy, i. 522
— — tetanus, i. 581
— — intermittent fever, i. 804
— — peritonitis, ii. 433
— — continued fever, ii. 917
Purging, colliquative, ii. 220
Purpura, ii. 1031
— epistaxis a symptom of, i. 821
— hæmorrhagica, ii. 163, 1031
Purulent effusion, i. 182
— otorrhœa, i. 337
— infiltration of lungs, ii. 80
Pus, i. 165, 179
— laudabile, i. 179
— ichorous and sanious, i. 182
— modifications of, i. 182
Push, ii. 1027
Pustulæ, ii. 840, 860
Pustules of small-pox, ii. 927
Putrefaction, fever caused by, ii. 909
Putrid fever, ii. 864
— measles, ii. 966
Pyæmia, suppuration of the joints in, i. 193, 201
— origin of, ii. 379, 383
— symptoms of, ii. 380
— acute, ii. 383
— chronic, ii. 380
— treatment, ii. 385
Pyæmic abscesses, i. 377
Pylorus, schirrus of the, ii. 490
Pyrexia, i. 150, 190, 862
Pyrosis, or water-brash, ii. 519

QUARTAN ague, i. 760
— triple, i. 762
Quartana duplex, i. 761
— duplicata, i. 761
— triplicata, i. 762
Quina in ague, i. 808
— in ague-cake, ii. 689
— in erythema, ii. 1002
Quinsy, i. 852
— exciting cause, i. 856
— prognosis, i. 856
— treatment, i. 858
Quotidian ague, i. 759

RABIES canina, i. 606. See *Hydrophobia*
— symptoms and progress of, i. 616
— seat of, i. 619
Ramollissement of the brain, i. 374
— rouge, ii. 80
— gris, ii. 80
Rash, mulberry, ii. 872
Rattle, mucous, ii. 28
Raw potatoes in scurvy, ii. 1033
Recrudescence of rabies, i. 622
Rectum, prolapsus of the, i. 43
— in dysentery, ii. 602
Red softening of lung, ii. 80
Redness, a sign of inflammation, i. 161
Reflex function of the spinal cord, i. 319
Regimen, antiphlogistic or pre-requisite, i. 243
— in phthisis, ii. 244
Regions of the thorax, ii. 14
Relapsing fever, ii. 865, 891, 899
— — phenomena of, ii. 892
— — duration of, ii. 893
— — contagiousness of, ii. 910
— — treatment of, ii. 911
Remittent fever, i. 194, 759
Renal dropsy, i. 300, 304; ii. 702
Respiration, thoracic, i. 4
— dry sounds in, ii. 22
— artificial, i. 77
— — its use in cases of poisoning, i. 77
— abdominal, ii. 7
— bronchial, ii. 86
— puerile, ii. 89, 141
— cavernous, ii. 208
— sounds in phthisis, ii. 208
Respirators, orinasal, in ague, i. 816
— — in ipecacuan dust, ii. 58
Respiratory organs most affected by tubercles, i. 220
— murmur, ii. 16, 24
Resolution, an event of inflammation, i. 151, 173, 854; ii. 82
— to promote, i. 271
Retention of urine, ii. 718
— — in hysteria, i. 727
Retrocedent gout, ii. 821
Revaccination, ii. 945
Revulsion, i. 174, 278
— doctrine of, i. 278
Rhatany root in internal hæmorrhage, ii. 791
Rheumatic sciatica, i. 753
— sore throat, i. 861
— carditis, ii. 323, 325
— fever, its relations with carditis, ii. 337
— gout, ii. 806
Rheumatism, acute, ii. 337, 802
— — articular, its connexion with pericarditis, ii. 323
— — its symptoms, ii. 802
— — causes, ii. 804, 806
— — varieties, ii. 805
Rheumatism, acute, treatment, ii. 807, 809
— — — external, ii. 812
— fibrous or diffused, ii. 805
— synovial, ii. 805
— passive, ii. 814
— chronic, ii. 814
— — its varieties, ii. 814
— — its remedies, ii. 815
— and gout, diagnosis between, ii. 824
Rhonchus, ii. 24
— crepitant, ii. 84
Rhythm of the heart, ii. 277
Rickets, hypertrophied bones in, i. 21
— its connexion with scrofula, i. 225
Rigidity, spastic, i. 556
Rigor mortis, examination of heart in, ii. 271
Rigors, symptoms of inflammation, i. 194
Ringworm, vesicular, ii. 1013
Risus sardonicus, i. 559
Rose, the, ii. 993
Roseola variolosa, ii. 938, 975, 991
Rot, grinders', ii. 238
— in sheep, ii. 640, 891, 899
Rotatio, i. 690
Round worms, ii. 617, 618
— symptoms, ii. 618
— remedies, ii. 618
Rubbing-sound in pleurisy, ii. 111
— of heart, ii. 330
Rubeola, ii. 964
— sine catarrho, ii. 966
— rossalia, ii. 973
— notha, ii. 991
Rubeloid fever, ii. 873
Ructus, a symptom of dyspepsia, ii. 514
Ruminating, power of, ii. 407, 515
Rupture of heart, ii. 299, 368
— of the urinary bladder, ii. 432
— of the gall-bladder, ii. 432
— of the uterus, ii. 432
Ruspini's styptic, i. 289; ii. 172, 790
Rye, spurred, causes gangrene, i. 189

SAC, ovarian, extirpation of, ii. 454
Saccharine urine, ii. 722
Saint Vitus's dance, i. 667. See *Chorea*
— Anthony's fire, ii. 993
Salicine, a remedy for ague, i. 812
Saliva of rabid dog, i. 608
Salivation, spontaneous, or idiopathic, i. 847
— of small-pox, ii. 931
Sanguineous apoplexy, i. 456
— expectoration, i. 724
Santonine, a remedy for ascaris lumbricoïdes, ii. 648
Sarcinæ, ii. 520
Scabies, ii. 1009
— purulenta, ii. 1009
Scald-head, ii. 1013
Scalds, inflammation in, i. 201
Scales, of the skin, ii. 840

Scapular region, ii. 15
Scarification, i. 246
Scarlatina sine scarlatinâ, ii. 786
— simplex, ii. 972, 975, 985
— anginosa, ii. 972, 975, 985
— maligna, ii. 972, 985
— latens, ii. 972, 985
Scarlet fever, ii. 971
— — species, ii. 972
— — diagnosis, ii. 973
— — symptoms, ii. 973, 974
— — its consequences, strumous affections, anasarca, ii. 979
— — treatment, ii. 985
— — prevention, ii. 989
Scelotyrbe festinans, i. 701
Schneiderian membrane, inflammation of the, ii. 22
Sciatica, i. 753
— rheumatic, i. 753
Science, meaning of, i. 2
Scirrhus, i. 231
— of pylorus, ii. 490
Scratch of cat said to produce hydrophobia, i. 608
Scrofula, produced by impure air, i. 115
— its exciting causes, i. 225
— its predisposing causes, i. 226
— a predisposing cause of epilepsy, i. 646
— its connexion with consumption, ii. 197
Scrofulous inflammation, i. 210
— diathesis, i. 211, 646
— — epilepsy attendant on, i. 646
— ulceration of larynx, ii. 204
— disease, ii. 224
— — mode of warding off, ii. 240
Scrotum, cancer of the, i. 239
Scurf, ii. 840
Scurvy, ii. 1032
Scutellaria lateriflora in rabies, i. 627
Sea-scurvy, ii. 1032
Season, epidemic constitution of, ii. 849
Seasons, influence of the, on health, i. 109
Secondary epilepsy, i. 646
— parotitis, ii. 847
— syphilis, its effects on larynx, i. 880
— hydatid tumours, ii. 661
— affections in Bright's kidney, ii. 758
— fever in small-pox, ii. 931
Secundan ague, i. 760
Senile decay, forms of, i. 39
— catarrh, ii. 41
Sensations, uneasy, i. 132. See *Uneasy Sensations*
— defective, i. 330
Senses, disordered functions of, i. 136
Sensible qualities, changes of, i. 144
Sensiferous nerves, i. 319
Sensorium commune, i. 315
Sensori-motor centre, i. 315
Septicæmia, ii. 193
Serous membranes, inflammation of, i. 198, 199; ii. 983
Serous apoplexy, i. 456
— cysts in the kidney, ii. 445
Serum of the blood, i. 165
— effusion of, in inflammation, i. 174
Shaking palsy, i. 701
Sheep rot, ii. 640
Shell-fish, a cause of urticaria, ii. 1004
Shingles, ii. 1014
Shock of apoplexy, i. 458
Shower-bath in chorea, i. 683
— for fortifying the body against injury from cold, ii. 109
— in colds, ii. 31
Sibilus, ii. 24, 26
Sick headache, ii. 522
Sight, disordered functions of, i. 136
Signs of disease, i. 125
Simple apoplexy, i. 453
— bronchitis, ii. 23
— hypertrophy of the heart, ii. 270, 291
Sinking, a symptom, i. 133
Situation, changes of, of some parts, i. 41
Skin, cutis anserina of the, i. 194
— inflammation of the, i. 201
— bronzed, ii. 793
— diseases of, ii. 839
— fungus of, ii. 1025
Skin-bound children, i. 30
Sleep, effects of, on health and disease, i. 84
— induced by extreme cold, i. 96
— phthisis, ii. 219
— in asthma, ii. 406
— walking, i. 738
Slough, i. 152
Small crepitation, ii. 27
Small-pox, ii. 844, 926
— diagnosis, ii. 926
— symptoms, ii. 927
— species, the discrete, ii. 928
— — confluent, ii. 928
— papular, ii. 928
— cohering, ii. 929
— bloody, ii. 934
— secondary fever of, ii. 931
— post-vaccinal, ii. 943
— contagious properties, ii. 935
— inoculation of, ii. 936
— vaccination, ii. 939
— modified after vaccination, ii. 943
— thermometry of, ii. 944
— revaccination, ii. 945
— statistics, ii. 953
— treatment, ii. 959
— modified, ii. 943, 962
Smoking in asthma, ii. 404
Smothering, death by, i. 69
Sneezing, a symptom, i. 141
Snow-water, its relation to goître, i. 827
Snuff a source of indigestion, ii. 511
— a source of lead poisoning, ii. 562
Softening, i. 33

SOF

Softening, causes and varieties of, i. 34
— of the brain, i. 374
— — white, i. 380, 477, 479
— of the spinal cord, i. 532
— of the lungs, red, ii. 80
— — gray, 80
— of stomach by the gastric juice, ii. 481
— of the liver, ii. 665
Softness, causes of, i. 36
Soil, its effect in phthisis, i. 226
Soils which evolve malaria, i. 767
Solids, their alterations in disease, i. 16
— intrusion of, into air-passages, ii. 258
Solitary glands, or glands of Brunner, ii. 883
Sore-throat, inflammatory, i. 864. See *Laryngitis*
— rheumatic, i. 861
— mercurial, i. 880
— of small-pox, ii. 931
— of scarlet fever, ii. 972
— of erysipelas, ii. 994
Sound, rubbing, of pleurisy, ii. 111
Sounds, dry, in respiration, ii. 22
— moist, in respiration, ii. 27
— in phthisis, ii. 210
— of the heart, natural, ii. 276
— — morbid, ii. 275
— — systolic, ii. 277
— — diastolic, ii. 277
— — first and second, ii. 277
— — systolic blowing, ii. 281
— — to and fro, ii. 330
— — alternate rubbing, ii. 330
— — bellows, ii. 332
Spasm, a symptom, i. 140
— its kinds, i. 556. See *Tetanus*
— tonic, i. 140, 556
— clonic, i. 556
— of the gastrocnemius, i. 559
— — œsophagus, ii. 408
— — stomach, ii. 519
Spasmodic diseases, i. 719
— croup, i. 881
— asthma, ii. 77, 187, 393
— stricture of œsophagus, ii. 408
— cholera, ii. 573
Spastic rigidity, i. 556
Specific gravity of diabetic urine, ii. 732
Spectral illusions, i. 137
Sphacelus an event of mortification, i. 186
— of the brain, i. 205
Spaying in pigs, ii. 455
Spider-web in ague, i. 811
Spinal cord, i. 314
— — pathology of, i. 436
— — inflammation of, i. 435
— — — of membranes, i. 440
— — — of substance, i. 443
— — in tetanus, i. 567
— treatment, i. 451
— hæmorrhage, i. 530
— apoplexy, i. 530
— dropsy, i. 540

STR

Spine, hysterical mimicking of diseases of bones of the, i. 725
Spit, black, ii. 253. See *Melanosis*
Spitting of blood a cause of pulmonary consumption, ii. 165
Spleen, enlargement of, i. 793; ii. 451, 498, 798
— — connexion of, with hæmorrhage, ii. 499
— diseases of, ii. 689
Splenisation of the lungs, ii. 78
Sponging in cold, ii. 31
— with cold water, i. 109
— in fever, ii. 915
Spongio-piline, ii. 920
Spontaneous salivation, i. 847
— ptyalism, i. 847
— pulmonary hæmorrhage, ii. 158
— acute inflammation of stomach, ii. 464
Sporadic catarrh, ii. 48
— cholera, ii. 571
— dysentery, ii. 609
— exanthemata, ii. 849
Spots in fever, ii. 873
Spotted fever, ii. 873
Springwurm, ii. 620
Spurious melanosis, ii. 254
— hydrocephalus, i. 414
— worms, ii. 643
Spurred rye causes gangrene, i. 189
Sputa of pneumonia, its kind, ii. 94
— nummular, ii. 216
Squamæ, ii. 840, 1022
St. Anthony's fire, ii, 993
St. Vitus's dance, i. 667. See *Chorea*
Stagnation, points of, in blood, i. 164
Starvation, death by, i. 68
Steam, inhalation of, in phthisis, ii. 247
Steel in phthisis, ii. 248
— in diabetes, ii. 738
Sternal regions, ii. 15
Sternutation, i. 141
Stethoscope, the, i. 16, 17, 19, 20
Stitch in the side, ii. 116
Stomach cough, ii. 7, 215
— hæmorrhage, ii. 167, 495
— enlargement of, in consumption, ii. 206
— inflammation of, ii. 464
— ulceration of, ii. 430, 472
— cancer of, ii. 431, 486
— perforation of, ii. 471, 481, 888
— hypertrophy of, ii. 490
— softening and perforation by the gastric juice, ii. 481
— cramp of, ii. 519
— sarcinæ, ii. 520
— gout, ii. 838
Stone in bladder as a disease, ii. 788
Stones, gall, i. 31; ii. 678, 682
Stop-cock function of minute arteries, i. 511
Straining, a cause of apoplexy, i. 517-519
Stramonium in asthma, ii. 403
Strangulated hernia, ii. 532
— — internal, ii. 554

Strangulation, death by, i. 69
Strangury, a symptom, i. 123
— from peritonitis, ii. 421
Stricture of œsophagus, ii. 408
— — actual, ii. 408
— — spasmodic, ii. 408
— of large intestine, ii. 546
Strongle, ii. 617, 641
Strongulus gigas, ii. 641
Strumous inflammation, i. 210
— diathesis, signs of, i. 221; ii. 197
Strychnia as a remedy for palsy, i. 539
— — severe tetanus, i. 585
— effect of poisoning by, i. 564
Subclavian region, ii. 14
Sub-pleural emphysema, ii. 188
Substances, foreign, in the bowels, ii. 612
— in the air passages, ii. 258
Subsultus tendinum, ii. 870
Succussion, method of, ii. 19
— in pleurisy, ii. 135
Sudamina, ii. 873, 975
Sudatorium, ii. 736, 771
Sudden death, pathology of, i. 65
— — application of the principles of investigating its phenomena in elucidating the symptoms and tendencies of disease, i. 79
Suffocation, death by, i. 69
Sugar of lead in hæmorrhage, ii. 171
— hepatic, ii. 729
— animal, ii. 733
— vegetable, ii. 733
Sulphate of quina as a remedy for intermittent fever, i. 806
Sulphur in scabies, ii. 1011
Sulphuric æther an anæsthetic agent, i. 131
— acid lemonade in painters' colic, ii. 567
Summer cholera, ii. 571
Sun-stroke, i. 93, 526
— phenomena of, i. 527
— cause of, i. 527
— remedies, i. 529
Suppression of urine, ii. 718, 745
— simulated in hysteria, i. 727
Suppuration, i. 151, 194
Suppuration an event of inflammation, i. 178
— to promote, i. 260
— of the brain, i. 377
— hepatic, ii. 593
— of the kidney, ii. 693
— diffused, of pulmonary texture, ii. 80
Supra-renal capsules, disease of, ii. 792
— symptoms of, ii. 793
— functions of, ii. 795
— conjectural causes of, ii. 797
Supra-spinal fossa, ii. 14
Sweating, in ague, promotion of, i. 802
— in phthisis, ii. 219, 248
Swabbing the throat, i. 862
Swelling, a sign of inflammation, i. 154
Symptomatic hæmorrhage, i. 281
— tetanus, acute, i. 557
— catarrh, ii. 58
Symptoms, general account of, i. 121
Symptoms in relation to diagnosis, i. 122
— — — to prognosis, i. 123
— — — to treatment, i. 125
— when signs also, i. 126
— pathognomonic, i. 127
— commemorative, i. 127
— direct and indirect, i. 127
— divisible into uneasy sensations, impeded functions, and changes of sensible qualities, i. 128–145
— head, ii. 752
Syncope, death by, i. 67; ii. 888
— from heart disease, ii. 301
— anginosa, ii. 313
Synocha, ii. 804
Synovial membrane, transformation of areolar tissue into, i. 40
— membranes, inflammation of, i. 800
— rheumatism, ii. 806
Syphilis, secondary, its effects on larynx, i. 880
Syphilitic inflammation of dura mater, i. 344
— affections of the brain, i. 379, 380
— — treatment of, i. 381
— — of the liver, ii. 668
— lepra, ii. 1024
Systolic sound of the heart, ii. 277, 309
— blowing sound of the heart, ii. 280

TABES mesenterica, i. 216
Tænia solium, ii. 618, 621, 649
— lata, ii. 618, 623
— crassicollis, ii. 625
Talicotian operation, i. 178
Tanjore pills in rabies, i. 626
Tank-worm, ii. 638
Tape worms, ii. 618, 621
— — symptoms, ii. 650
— — remedies, ii. 651
Tapping the chest, i. 175; ii, 133
— in dropsy, i. 310
— in hydrocephalus, i. 429
— in pleurisy, ii. 112
— in ovarian dropsy, ii. 459
Taraxacum in hepatic disease, ii. 671
Tartarised antimony as a remedy for pneumonia, ii. 104
Tasteless ague-drop in intermittents, i. 810
Tatula a remedy for asthma, ii. 404
Tegumentary membranes, inflammation of the, i. 201
Temperaments, national, i. 129
— constitutional, i. 129
Temperature, vicissitudes of, a cause of disease, i. 86
— in intermittents, i. 782
— in phthisis, ii. 244
Tenderness in inflammation, i. 157
Tenesmus, i. 133
Tertian ague, i. 759
— — double, i. 761
Tertiana duplicata, i. 761.

Tertiana duplex, i. 761
Testicles, mumps in the, i. 845
Tests in diabetes, ii. 721
— for albumen, ii. 743
Tetanic symptoms in ague, i. 588
— hysteria, i. 588
Tetanus, i. 556
— name of, i. 557
— its symptoms, i. 557
— varieties, i. 558
— causes, i. 561, 571
— lateralis, i. 559
— traumatic, i. 561
— acute and chronic, i. 563
— idiopathic, i. 561, 579
— diagnosis of, i. 563
— effect of poisoning by strychnia, i. 564
— its pathology, i. 567
— centric, i. 570
— eccentric, i. 571
— its treatment, i. 574
— treatment of traumatic, i. 586
— nascentium, i. 588
Thalami optici, i. 315
Thermometry of disease, ii. 862
Thirst, i. 134
Thoracic aorta, aneurism of, i. 871
— respiration, ii. 7
— aneurisms, ii. 361
— — their various situations, ii. 362
— — symptoms, ii. 365
— — morbid anatomy, ii. 372
— — treatment, ii. 327
— inspiration in peritonitis, ii. 421
Thorax, diseases of, ii. 1
— symptoms in diseases of, ii. 3
— — dyspnœa, ii. 3
— — cough, ii. 6
— auscultation, ii. 8
— — percussion, ii. 9
— regions of, ii. 14
— — — succussion, ii. 19
— — palpation, ii. 19
— tapping the, ii. 133, 140
— cancer of the, ii. 257
Thought, derangement of the function of, i. 514
Thread-worms, ii. 619
— symptoms, ii. 649
— remedies, ii. 649
Thrombosis, i. 380
Throttling, death by, i. 69
Thrush, i. 850
— treatment, i. 850
Thymic asthma, i. 903
Thyreoïd gland, enlargement of, ii. 821. See *Bronchocele*
Tic-douloureux, i. 745
— caused by malaria, i. 747
— dependent on condition of the teeth, i. 747
— remedies for, i. 747
Tightness, a symptom, i. 133
Tinctura iodi in phthisis, ii. 247
Tinkling, metallic, ii. 135
Tinnitus aurium, a symptom, i. 137

Tissue, areolar, effects of inflammation in, i. 198; ii. 803
— serous, i. 198
— synovial, i. 200
— tegumentary, i. 201
— mucous, i. 202
— muscular, i. 203
— arterial, i. 204
— venous, i. 204
— substance of the brain, i. 205
— nervous, i. 205
Tissues, hypertrophy of, i. 17
— atrophy of, i. 26
— induration of, i. 29
— cellular, i. 33
— softening of, i. 33
— transformations of, i. 36
— as affected by inflammation, i. 198
To-and-fro sound of the heart, ii. 330
Tobacco in tetanus, i. 581
Tongue, importance of observing the conditions and appearances of, i. 145
Tonic spasm, i. 140, 556
Tonquin medicine in rabies, i. 626
Tonsillitis, i. 582
Tonsils, chronic enlargement of, i. 861
Tormina, ii. 530
Torulæ, in diabetic urine, ii. 720
— cerevisiæ, ii. 720
Toxæmic spoiling of the brain, i. 380
Trachea, ulceration of, i. 873 ; ii. 204
— foreign bodies in, ii. 258
Tracheotomy, i. 872, 878, 901, 909
— in small-pox, ii. 961
Trades, effect of, on disease, i. 84
— in phthisis, ii. 237
Transformations of tissues, i. 36
Traumatic delirium, i. 365
— tetanus, i. 561
— — treatment of, i. 586
Treacle-beer, ii. 567
Treatment, indications of, i. 125
Trembles, the, i. 703
Tremor, a symptom, i. 140
— mercurial, i. 703
Trepanning, i. 424
Trichina spiralis, ii. 635
Trichiniasis, ii. 636
Tricocephalus dispar, ii. 618, 620, 649
Tricuris, ii. 620
Triple quartan ague, i. 762
Trismus, i. 557, 559
— nascentium, i. 588
Trommer's test, in diabetes, ii. 720
Tropical abscesses, ii. 664
True melanosis, ii. 251, 501
Tubercles, i. 211, 392 ; ii. 840
— pathology of, i. 212
— crude, i. 212, 392
— in the brain, i. 392
— their favourite seats, i. 213 ; ii. 194
— deposit and growth of, i. 214
— question of their contagion, i. 222
— production at will, and inoculation of, i. 226

Tubercles, their relation to pulmonary emphysema, ii. 186
— miliary, ii. 194, 197, 227
— nascent, ii. 195, 227
— introduced by inoculation, ii. 193
Tubercula, ii. 840
Tubercular phthisis, ii. 159, 196
— infiltration of lung, ii. 194
Tuberculosis, acute, ii. 226
Tubes, Fallopian, dropsy of, ii. 445
Tumour, i. 156
Tumours, fibrous, in the brain, i. 378
— cancerous, in the brain, i. 379
— in abdomen, ii. 390, 797
— hydatid, ii. 800
— phantom, ii. 455, 456
— secondary, ii. 661
— ovarian, ii. 798
Turkish bath, i. 89
Turpentine, oil of, in tetanus, i. 583
— — a remedy for worms, i. 583; ii. 651
Tussis convulsiva, ii. 68. See *Hooping-cough*
— ferina, ii. 68. See *Hooping-cough*
Twisted bowel, ii. 550
Typhoid fever, ii. 865, 879
— symptoms, ii. 879
— difference between it and typhus, ii. 882
— morbid appearances, ii. 884
— duration, ii. 893
— contagiousness of, ii. 898
— treatment of, ii. 911
Typhomania, ii. 870
Typhus fever, i. 151, 152; ii. 864
— symptoms, ii. 866, 867
— period of defervescence, ii. 868
— delirium, ii. 870
— mulberry rash, ii. 872
— morbid appearances, ii. 876
— duration, ii. 893
— contagiousness of, ii. 899
— treatment, ii. 911

ULCERATION, an event of inflammation, i. 152, 184
— phagedenic, i. 185
— sloughing, i. 185
— callous, i. 185
— fungous or exuberant, i. 185
— proud flesh, i. 185
— of the larynx, ii. 204
— of the trachea, ii. 184
— of the intestines, ii. 295, 885
— of the right ventricle, ii. 300
— of the umbilical region, ii. 416
— of the duodenum, ii. 431
— of the stomach, ii. 430, 472
— — chronic, ii. 431
— — perforating, ii. 473
— — symptoms, ii. 476
— — treatment, ii. 477
Uncircumscribed pulmonary hæmorrhage, ii. 162
Uneasy sensations, pain, i. 128
— — itching, i. 132
— — nausea, i. 133
— — vertigo, i. 133
— — sinking, i. 133
Unguentum aconitiæ in tic-douloureux, i. 752
— hydrargyri iodidi rubri in goître, i. 837
Union by the first intention, i. 177
Unmixed phthisis, ii. 228
Upas antiar, death by, i. 67
Upper sternal region, ii. 15
Uræmic poisoning, ii. 751
— poison, death by, i. 456
Urea, effects of its retention in the system, i. 456; ii. 716, 754
Urethra, hæmorrhage from the, ii. 786
Urinary bladder, rupture of, ii. 432
— paraplegia, i. 537
Urine, retention of, simulated in hysteria, i. 727
— retention and suppression of, ii. 718, 745, 757
— morbid conditions of, ii. 700, 702
— ammoniacal, ii. 711
— saccharine, ii. 722
— — its specific gravity, ii. 723
— albuminous, ii. 741
— chylous, ii. 777
— bloody, ii. 695, 780
— black, ii. 781
Urticaria, ii. 1002
— internal, i. 852
— with small-pox, ii. 930
— varieties, ii. 1003
— evanida, ii. 1004
— febrile, ii. 1005
— treatment, ii. 1005
— factitious, ii. 1006
Uterus, rupture of, ii. 432
— dropsy of, ii. 445

VACCINATION and vaccine disease, ii. 939
— and inoculation, their comparative merits, i. 950
Vaccine disease, ii. 939, 941
Vagina, prolapsus of the, i. 43
Valve, mitral, disease of, ii. 305
Valvular disease of the heart, ii. 304
Vapours, anæsthetic, i. 130
— — known to the ancients, i. 131 *note*
— medicated, in phthisis, ii. 247
Varicella, ii. 962
— lymphatica, ii. 963
Variations of the pulse, i. 142
Varieties of inflammation, i. 207
Variola, ii. 926. See *Small-pox*
— discreta, ii. 928
— confluens, ii. 928
— corymbosa, ii. 933
Variolæ sine variolis, ii. 931
— nigræ, ii. 934

Variolæ pusillae, ii. 962
Varioloid disease, or modified small-pox, ii. 943
Variolous fever, ii. 931
Vegetable decomposition, i. 770, 773
— sugar, ii. 733
— parasites, ii. 1025
Vegetables, raw, ii. 569
Veil, gauze, in ague, i. 817
Veins, diseases of, i. 204; ii. 376. See *Phlebitis*
— obliteration of, ii. 386
Velum palati, paralysis of the, i. 894
Venæsection, propriety of repeated, i. 253
— in dropsy, i. 308
— in ague, i. 801
Venom, inoculation of cadaveric, ii. 427
Venous tissues under inflammation, i. 240
— murmurs, ii. 287
— trunks, obstruction of, ii. 387
Ventricle of the heart, hypertrophy of, ii. 292
— right, ulceration of, ii. 300
Ventricles of the brain in hydrocephalus, i. 389
Ventricular hydrocephalus, i. 435
Ver solitaire, ii. 623
Vernal ague, i. 764
— intermittents, i. 764
Vertigo, as a symptom, i. 133, 512
— epileptic, i. 632, 639
Vesicles, miliary, ii. 873, 892
Vesiculæ, ii. 840, 1012, 1020
Vesicular respiration, ii. 18, 24
— emphysema, ii. 173
— — its anatomical characters, ii. 174
— — physical signs, ii. 181, 182
— — atropic, ii. 180
— — auscultatory signs, ii. 181
— — symptoms, ii. 182
— — causes, ii. 183
— — treatment, ii. 188
Vicarious hæmorrhage, i. 277, 819; ii. 496
— epistaxis, i. 819
— hæmatemesis, ii. 496
— gastric hæmorrhage, ii. 496
Vicissitudes of temperature a cause of disease, i. 86
Vigil, coma, ii. 869
Viscera, inflammation of, i. 199
Vital functions, i. 65
— organs, i. 65
Vitus's (St.) dance, i. 667. See *Chorea*
Voice, the, ii. 18
— unnatural modifications of the, ii. 18
— bronchial, ii. 85
Voluntary motion, disordered functions of, i. 140
Vomicæ, ii. 155, 198, 200, 207
— obsolete, ii. 202
Vomit, black, ii. 501
Vomiting caused by hot weather, i. 91
Vomiting of blood, its symptoms, ii. 476, 505
Vomitoria of scarlet fever, ii. 972
Vomitus, a symptom of dyspepsia, ii. 512
Vox cholerica, ii. 575, 596

WANT of breath, ii. 4
Warm drinks in ague, i. 799
Warmth in inflammation, i. 259
— in tetanus, i. 577
Warty growths on lining membrane of the larynx, i. 908
— — on valves of the heart, ii. 306, 328, 350
Wasting, or emaciation, a symptom of disease, i 145
— palsy, i. 709
— — causes of, i. 710
— — symptoms and clinical history, i. 712
— — prognosis, i. 714
— — remedies, i. 714
— — in ague, i. 799
— — in phthisis, ii. 221
Water obstructs malaria, i. 789
— its relation to goître, i. 827
— difference between hard and soft, i. 828
— impure, in cholera, ii. 588
Water-bed, Dr. Arnott's, i. 159
Water-brash, ii. 519
Watercresses in scurvy, ii. 1034
Water doctors, ii. 777
Water-gilding causes mercurial tremor, i. 704
Waterstroke, i. 397
Water-pox, ii. 962
Waxy liver, ii. 206, 669, 800
Weather, its influence in catarrhs, ii. 49
Web, spider, in ague, i. 812
Wheals, ii. 1004
White softening of the brain, i. 380, 477, 479
Willow-bark as a remedy for ague, i. 812
Wind, east, its effects on intermittents, i. 766
Windpipe, intrusion of solid substances into, ii. 259
Winds carry about malaria, i. 787
Wine, in tetanus, i. 579
Winter, mortality greater in, i. 111
Woorara, recovery from poisoning by, i. 77
— as a remedy in tetanus, i. 584
Worm, Guinea, ii. 617, 638
Worms, ii. 616
— ascaris lumbricoïdes, ii. 617, 618
— — vermicularis, ii. 617, 619
— thread worms, ii. 618, 619
— — long, or tricocephalus dispar, ii. 618, 620
— tænia medio-cancellata or inermis, ii. 618, 623, 626
— cestoid, ii. 618
— tænia solium, ii. 618, 621

Worms, tænia lata, ii. 618, 623
— ver solitaire, ii. 623
— cyst worms, ii. 624
— prevention, ii. 633
— strongulus gigas, ii. 641
— spurious, ii. 643
— their nature, ii. 646
— their symptoms, ii. 646
— treatment, ii. 647
Wrist-drop, ii. 556

YEAST, ii. 855
— as a test for diabetes, ii. 645

ZINC in ague, i. 814
Zona ignea, ii. 1014
Zones for examination of abdomen, ii. 416
Zoster, ii. 1014
Zymotic diseases, ii. 857

THE END.

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LIST

OF

ILLUSTRATIONS.

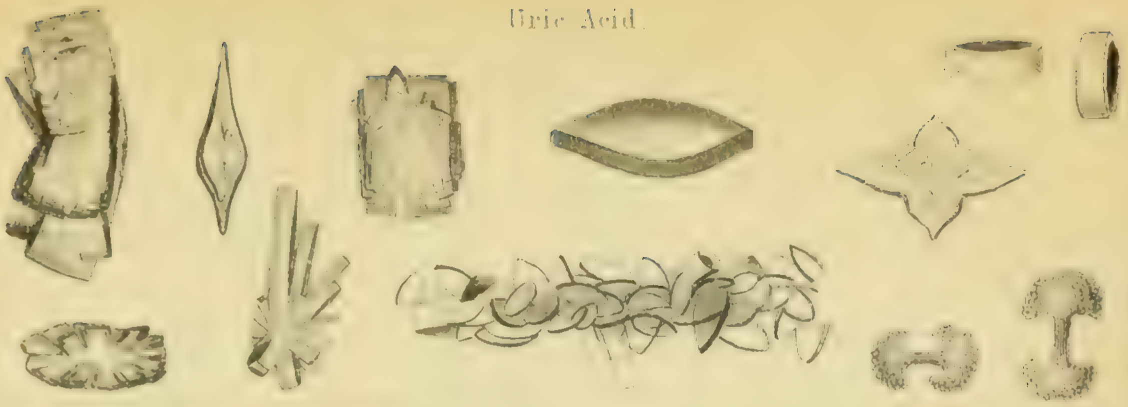
PLATE I. . . . . . *To be placed at end of Vol. II.*

- Uric Acid.
- Urate of Ammonia.
- Oxalate of Lime.
- Cystic Oxyde.
- Triple Phosphate.

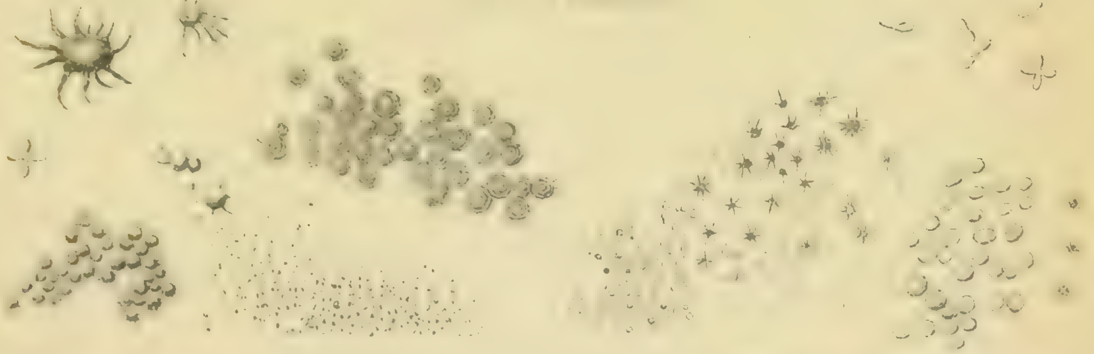
PLATE II. . . . . *Ditto*

- Urea.
- Nitrate of Urea.
- Casts of the Uriniferous Tubes:
  - Epithelial.
  - Blood Cast.
  - Granular.
  - Large Hyaline.
- Blood Globules.
- Pus.
- Pavement Epithelium.
- Casts of the Uriniferous Tubes:
  - Small Hyaline.
  - Oily.

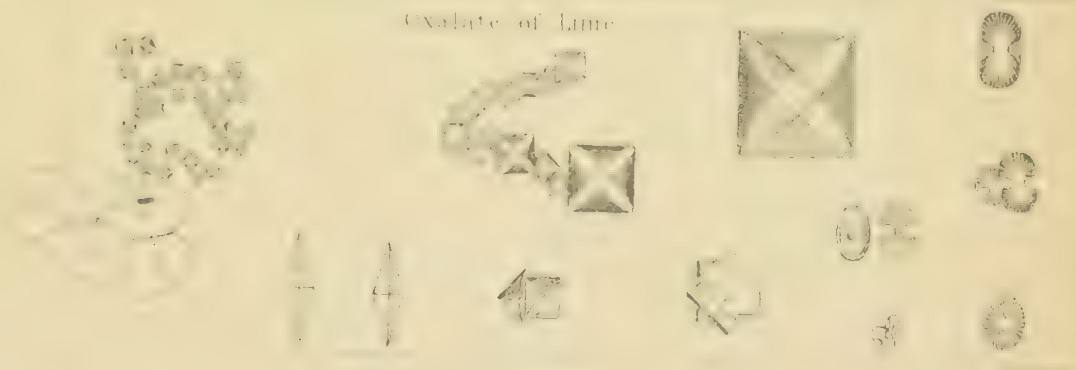
Uric Acid.



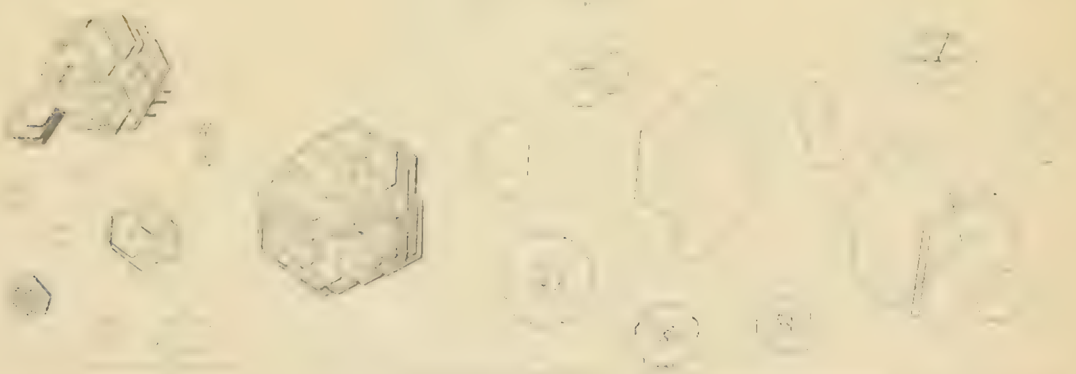
Urate of Ammonia.



Oxalate of Lime.



Cystic Oxyde.



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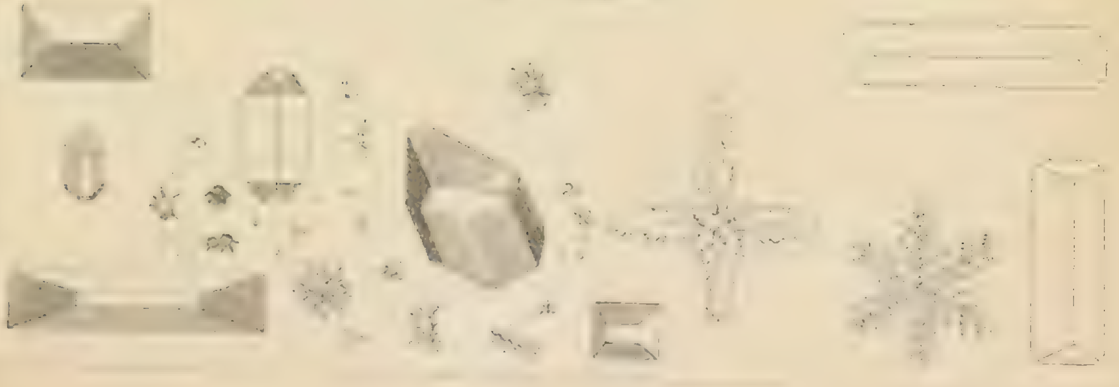

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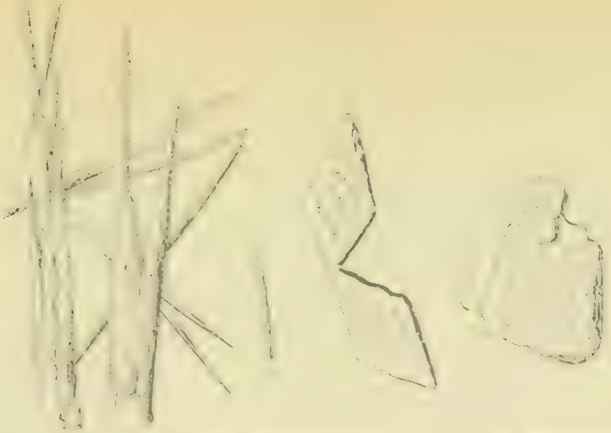
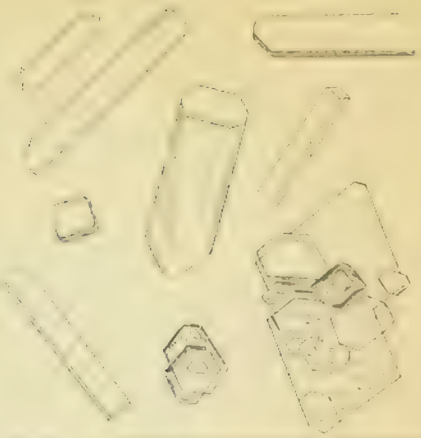
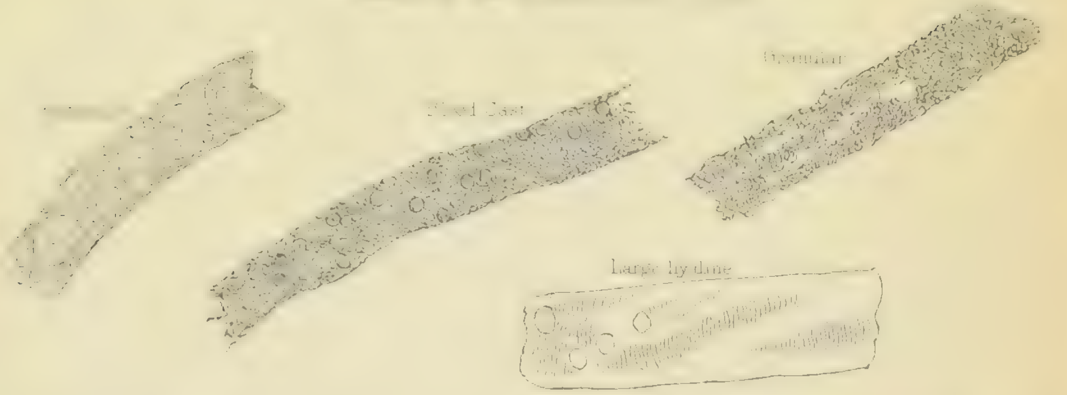
Urea.



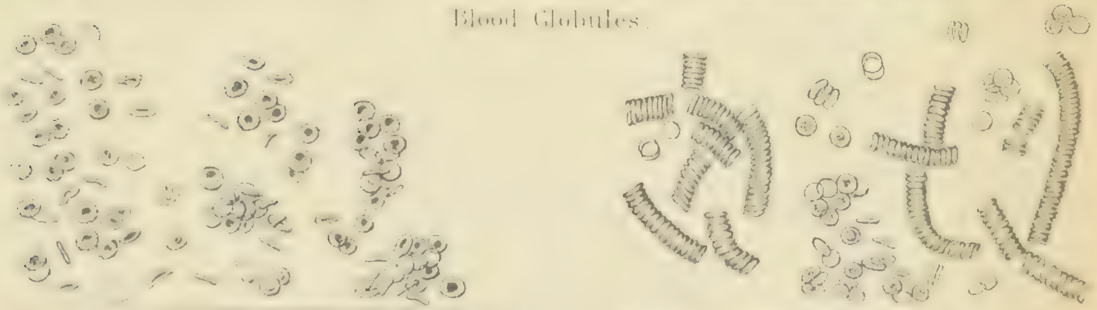
Nitrate of Urea.



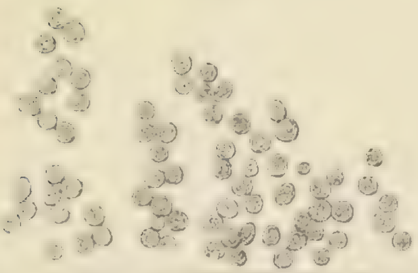
Casts of the Uriniferous Tubes.



Blood Globules.



Pus.



Casts of the Uriniferous Tubes

Pavement Epithelium.

J. H. Lowry